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A  
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OF

NATURAL PHILOSOPHY, CHEMISTRY,  
AND THE ARTS:

*ILLUSTRATED WITH ENGRAVINGS.*

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BY WILLIAM NICHOLSON.

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V O L. II.

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L O N D O N:

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## VI. The Progress of Mechanical Discovery, exemplified in an Account of a Machine for cutting Files. (W. N.) — — p. 309

Chemical and mechanical pursuits compared. The greater number of new machines are of no value. Difficulties of carrying inventions into effect. Progress. Machine for cutting files. General remarks.

## VII. The Dutch Process for making the Blue distinguished by the Name of Turnsol — — — — p. 311

## VIII. Experiments and Remarks on certain Ranges of Colours hitherto unobserved, which are produced by the relative Position of plain Glasses, with regard to each other. (W. N.) — — — p. 312

Short statement of Newton's doctrine of the fits of reflection and transmission of light. Subsequent experiments. A new series of colours formed between plain glasses; different from but coexistent with the Newtonian, and much less affected by changes of temperature, position, or electrization. This effect is produced when the plates are at considerable distances from each other; for example, four feet. Other facts.

## IX. Some Account of the Country and Climate of the North-western Lakes of America. By Major C. Swan, Paymaster to the Western Army. p. 315

## X. An Essay on the Art of conveying Secret and Swift Intelligence. By Richard Lovell Edgeworth, Esq. F.R.S. and M.R.I.A. — p. 319

Description of the French Telegraph, and that of Robert Hooke. Late trial of Hooke's method. Description and use of an apparatus for speedy and swift communication by numbers. Its superiority over the alphabetical telegraph. Carried into effect across the Irish Channel.

## Scientific News, and Accounts of Books — — p. 328

## Mr. Park's Travels in the unexplored Parts of Africa — — p. 329

## N O V E M B E R. 1798.

Engravings of the following Objects: 1. Leaf, Acorn, and Prickly Cup of the Turkey and Spanish Oaks. 2. A new Escapement. And, 3. An Apparatus for saturating the Alkalis with Carbonic Acid.

I. An Account of three different Kinds of Timber Trees, which are likely to prove a great Acquisition to this Kingdom, both in point of Profit, and as Trees for Ornament and Shade. By Charles White, Esq. F.R.S. p. 333  
Historical

Historical facts and observations, which prove the rapid growth and advantages to be derived from the broad-leaved American black birch, the Athenian poplar, and the iron, wainscot, or Turkey oak.

II. Abstract of a Memoir of M. Proust, on the Tanning Principle. By Citizen Descotils — — — — — P. 337

The solution of muriate of tin precipitates the tanning principle from a decoction of nut-galls, and leaves the acid of galls in solution. Methods of obtaining the gallic acid and the tanning principle separate from each other. Precipitate of glue with tanning. The green sulphate of iron is not precipitated by either of the principles of galls. Difference of the precipitates of the red sulphate by those principles. Application of the facts to the process of dyeing black.

III. Notice of a Memoir of Citizen Guyton, upon the Tables of the Composition of Salts, and the Means of verifying the Proportions indicated by those Tables — — — — — P. 340

When two salts, differing in their acids and their bases, are of such a nature as to change their principles by double elective attraction, as for example, the sulphate of potash and nitrate of lime, it is known that two new salts are formed; namely, in the present instance, nitrate of potash and sulphate of lime: but the quantities of each base, sufficient to produce neutral salts in the first combinations, are not the same as will saturate the acids in the new compounds. If, therefore, the quantity of one salt be such as to afford enough of its base to saturate the acid of the other, the base of this last will be either more or less than is sufficient to saturate the acid of the first-mentioned salt. In this case, it may be inferred, that one of the new compounds will be neutral, and the other not so. But the facts refute this conclusion; for the mixture shews no redundancy of either principle. Why this happens is a new and interesting chemical theorem.

IV. On Pasingraphy; or, the Art of Writing which shall be intelligible to all Nations — — — — — P. 342

The universal writing is not the means of rendering all languages intelligible to all nations; in its own nature it supposes the existence of one common language proposed to all nations for their acceptance and use. History of the attempts and observations of various eminent men; Lord Bacon, Des Cartes, Becher, Dalgarn, Frischius, Kircher, Besnier, Wilkins, Leibnitz, De l'Épée, and Condillac. General remarks.

V. Observations on the Natural History of Guiana. In a Letter from William Lochead, Esq. F.R.S. Edin. to the Rev. Dr. Walker, F.R.S. Edin. Regius Professor of Natural History in the University of Edinburgh — P. 347

Observations on the rivers, and their agency in forming the soil, of Guiana; their creeks, floods, tides, &c.

VI. On the supposed Revival of Insects after long Immersion in Wine or other intoxicating Liquor. By Mr. John Gough — — — P. 353

Experiments on various insects, which prove that the popular notion of their revival many months after their immersion in fermented liquors is ill founded.

VII. Various Notices respecting the Arts in Turkey.—Jeweller's Foil.—Glue, or Mastic for Stones and Metals.—Casting of Malleable Iron.—Filtration by Ascent.—Butter preserved without Salt.—Extemporaneous Yeast P. 355  
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- VIII. Observation of the Passage of a Comet over the Disc of the Sun. By Citizen Dangos — — — — — p. 357

Time and duration of the phenomenon. Inferences. Why comets cannot be often seen on the face of the sun.

- IX. Analysis of the Aqua-marine or Beryl; and the Discovery of a new Earth in that Stone. Read before the French National Institute 26 Pluviose, in the Year VI. (Feb. 14, 1798.) By Citizen Vauquelin — — — — — p. 358

Method of Analysis. New earth, soluble in acids and in pure potash; and affording salts of a saccharine taste. Comparative examination of its habitudes, and those of alumine, from which, as well as the other earths, it greatly differs.

- X. Description of a new-invented detached Escapement for Pocket Watches, &c. By Mr. John Prior — — — — — p. 363.

- XI. On Mr. Cartwright's Invention for rendering the Pistons of Steam Engines, Pumps, and other Hydraulic Apparatus, tight by Metallic Parts, without packing or leathering. (W. N.) — — — — — p. 364

Letter of enquiry. Description of the invention. Theoretic and practical reasons why this contrivance must prove less effectual than the methods already in use.

- XII. Information respecting the Zoonic Acid, discovered by Berthollet p. 369

Animal substances, the vegetable gluten, and yeast, afford an acid of destructive distillation, which may be saturated with lime, and the ammoniac then driven off by boiling. By the addition of phosphoric acid, and distillation at the boiling heat, the lime will form a phosphate, and the new acid will come over. Its characters.

- XIII. Historical Notes concerning the Invention of the Air-pump with Metallic Valves; the Necessity of Alkali to produce the crystallized Salt called Alum; and the Electrical Instrument called the Revolving Doubler — — — — — p. 370

- XIV. Description of an Apparatus for saturating Potash and Soda with Carbonic Acid. By Cit. Welther — — — — — p. 371

- XV. Abstract of a Memoir of Klaproth on a new Metal denominated Tellurium. Read at the Public Session of the Academy of Sciences at Berlin. Jan. 25, 1798 374

History of the discovery. The white ore of gold is heated with muriatic acid; to which the nitric is afterwards added. The complete solution is diluted, and pure potash is added in excess. A small portion of iron, together with gold, falls down; the rest is dissolved by the alkali. Muriatic acid throws down a white precipitate. It is the oxyde of tellurium, and is reducible by being made into a paste with oil, and subjected to a gradual heat. The metal rises in distillation like mercury. It is white, shining, brittle, friable, very fusible, combustible with flame, combinable with mercury, and with sulphur, soluble in acids, and in the excess of any alkali; not precipitable by prussiate of potash, &c. &c.

- Scientific News, and Accounts of Books — — — — — p. 376

American Societies. Aerostation. Rumford's Essays. Rivard on the Sphere and Calendar. Townson's Mineralogy. Mr. Parke's Travels in Africa.

D E C E M B E R 1798.

Engravings of the following Objects: 1. A View of the Mansion of Roseapenna, in Ireland, destroyed in Consequence of the Change of Climate supposed to have taken Place in that Island during the present Century. 2. The powerful Electrical Machine of Rouland, which operates by the Friction of Silk.

I. Memoir on the Climate of Ireland. By the Rev. William Hamilton, of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M.R.I.A. Corresponding Member of the Royal Society of Edinburgh, &c. — — — — — p. 381

General opinion that the seasons of Ireland are considerably changed within the memory of man; that the winters are milder, and the summers less warm. Meteorological instruments do not exhibit all the circumstances which influence a climate. Recourse must be had to other observations. The winds of Ireland have, of late years, blown with uncommon violence from the westward. Their effects are particularly marked on the province of Ulster. Interesting facts respecting trees which formerly flourished in Ireland, but cannot now withstand the rigour of the seasons. The sands of the sea have been driven with increased violence on the Irish coast. Instances of the speedy destruction of habitable places and districts from this cause in the Corporation of Bannow, the Mansion of Roseapenna, and other deserted habitations. Increasing violence of the tides from the Atlantic Ocean.

II. Analysis of the Red Lead of Siberia; with Experiments on the New Metal it contains. By Citizen Vauquelin, Inspector of Ores, and Conservator of Chemical Products at the Mineralogical School — p. 387

History of the Siberian Red Lead Ore, and the experiments formerly made upon it. New analysis. 1. By boiling with carbonate of potash, which precipitated carbonate of lead, and formed a neutral salt with the peculiar acid of the ore. Nitric acid deprived this salt of its alkali, which crystallizes by spontaneous evaporation.—Or otherwise, 2. diluted muriatic acid being added to the ore, combined with the lead of the ore, and disengaged the acid. The muriate of lead being insoluble, remained at the bottom, while the peculiar acid of the ore became suspended, together with a small redundant portion of marine acid. This last, after decantation, was abstracted by adding small portions of the oxyde of silver, and left a solution of the acid of chrome. Its nature, properties, and combinations.

III. Information respecting the Earth of the Beryl; in Continuation of the first Memoir on the same Subject. By Citizen Vauquelin — p. 393

Additional Experiments by which the component parts of beryl, and the characteristic properties of the new earth discovered in that stone, are more correctly ascertained.

IV. Observations on Electricity, Light, and Caloric; chiefly directed to the Results of Dr. Pearson's Experiments on Electric Discharges through Water. By a Correspondent — — — p. 396

Remarks on the very loose and inaccurate notions of philosophers concerning electricity, light, and caloric. Animadversions on Dr. Pearson's theory of the decomposition and re-production of water by electricity. On the materiality of heat.

V. An



V. An Enquiry concerning the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F.R.S. M.R.I.A. p. 400

Doubt whether light operates chemically in any other way than by generating heat. Experiments on the vitrification of oxyde of gold by the heat of a candle; and by solar light. Effect of dilution in rendering the particles more minute and susceptible of elevated temperature. Curious instance of gold reduced by charcoal in the humid way with solar light—and without.

VI. Experiments and Observations on the Nature of Sugar, and of Vegetable Mucilage. By Mr. William Cruickshank, Chemist to the Ordnance, &c. p. 406

Chemical examination of sugar, honey, sugar of milk, gum arabic, and gum tragacanth. Results: That sugar is composed of carbon, hydrogen, and oxygen;—sugar of milk contains more oxygen and much less carbon;—gum contains lime and azote as well as the principles of sugar;—vegetable farina cannot become sugar without water and oxygen;—that neither sugar, if deprived of oxygen, or combined with other matters, nor pure vegetable nor animal mucilage, is capable of fermentation.

VII. On the Art of covering Wire Cloth with a transparent Varnish, as a Substitute for Horn; and on other Objects of Public Utility. By Alexis Rochon, of the National Institute of France, &c. — — p. 412

Chinese manufacture of horn. Wire cloth prepared for lanthorns with glue; and with mica. New covering for houses. Medical bougies.

VIII. On the Production of Nitric Acid by the Contact of Oxygen very much heated and the Air of the Atmosphere — — p. 413

New phenomenon of nitric acid from manganese. Conjectures respecting it.

IX. Analysis of the Chrysolite of the Jewellers, proving it to be Phosphate of Lime. By Citizen Vauquelin — — — p. 414

The chrysolite, being pulverised and treated with sulphuric acid, afforded sulphate of lime, and the phosphoric acid was disengaged. In another experiment the lime was taken up by muriatic acid, and precipitated by the oxalic. From both experiments the stone was found to contain 54 parts lime, and 46 phosphoric acid.

X. Account of a singular Instance of Atmospheric Refraction. In a Letter from William Latham, Esq. F.R.S. and A.S. to the Rev. H. Whitfield, D.D. F.R.S. and A.S. — — — p. 417

Uncommon view of the coast of France from Great Britain at the distance of forty or fifty miles. Remarks and references to similar facts.

XI. An Account of Electrical Machines of considerable Power, in which Silk is used instead of Glass. (W. N.) — — p. 420

Various non-conductors used for the excitation of electricity. Enumeration. Description of the machines of Ingenhoufz, Walckiers, and Rouland, which operate by the friction of silk. Their power of excitation.

**XII. Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, Member of the Italian Society, &c. p. 422**

Comparative experiments, shewing the quantities of water emitted under like circumstances through different tubes. Practical proposition of great utility, by which the expenditure is more than doubled. Roman law concerning the enlargement of pipes for water.

**Accounts of Books, &c. — — — — p. 426**

Philosophical Transactions, Part II. 1798.—Rollo on Diabetes Mellitus.—Pajot-des-Charmes on Bleaching.—Chemical Memoirs of Pelletier.—Leybourn's Translation of Dr. Stewart's Propositiones Geometricæ.—Note respecting Dalgarno, Leibnitz, Wilkins, and the Universal Character.

## J A N U A R Y 1799.

Engravings of the following Objects: 1. A New Instrument for ruling parallel Lines for the Use of Engravers: and, 2. Mr. Cavendish's Apparatus for measuring the mutual Gravitation of Bodies, and ascertaining the Density of the Earth.

- I. Description of a New Instrument for drawing equidistant and other parallel Lines, with great Accuracy and Expedition, intended principally for the Use of Engravers; with Specimens of its Performance. By W. N. p. 429

The instrument consists of two flat rulers, one of which confines the plate, and the other is made to shift through small intervals by means of a screw and gear, while it constantly preserves its parallelism.

- II. Memoir on the Climate of Ireland. By the Rev. William Hamilton, of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M.R.I.A. Corresponding Member of the Royal Society of Edinburgh, &c. (Concluded from page 386, Vol. II.) — — p. 431

General effects of the western tempests on the climate of Ireland. The summers are rendered colder, and the winters milder.—This equable temperature is favourable to animal and vegetable life.—Cause of the increased violence of the westerly winds, deduced from the forests of Ireland in particular, and of Europe in general, having been cleared in modern times, and the land cultivated.

- III. Experiments and Observations on Electricity—Excitation—The two States—Points of Difference between the Action of weak and strong Electricities compared together. (W. N.) — — p. 438

Cascade of electric fire produced by cutting a hole in the silk flap of the rubber. Beautiful star formed by the plus electricity upon paper. Experiments to determine whether the glass tube in Bennet's electrometer has any effect on the divergence of the gold leaf. Other enquiries respecting the metallic coatings and size of the head of that instrument.—Whether the electricity of conductors is disturbed according to the same law, by the influence of an electrified body, when the power is weak, as when it is strong. Remarkable difference in the operation of pointed bodies and flame in weak and strong electricities.—Whether the laws of attraction and repulsion be the same in strong electricity as in weak.

- IV. Analysis of the Red Lead of Siberia; with Experiments on the new Metal it contains. By Citizen Vauquelin, Inspector of Mines, and Conservator of Chemical Products at the Mineralogical School. (Concluded from page 393, Vol. II.) — — — p. 441

Combinations of the acid of red lead with the alkalis.—Its reduction to the metallic state.—Properties of the new metal.—Denomination and uses in chemistry and the arts.

- V. Experiments to determine the Density of the Earth. By Henry Cavendish, Esq. F.R.S. and A.S. — — — p. 446

A wooden arm six feet long was suspended in a horizontal position by a slender wire forty inches long, and to each extremity was hung a leaden ball about two inches in diameter. To these balls

balls, on opposite sides, were presented two larger balls, which, by their attraction, drew the arm aside. The arm, with its balls, was inclosed in a wooden case, and the whole apparatus placed in a room adapted to this purpose, and kept shut while the observer attended to the effect from without, by means of a short telescope opposite each end of the bar. The power required to draw the arm aside, was ascertained from the time of its free vibrations, and the density of the earth computed from the comparison between the effect of its attraction on the balls with the effect of the same nature by which the arm was drawn from its stationary position. This last effect amounted to about one inch and a half when a thin wire was used, and about one third of an inch with a thicker wire: the deviations were, therefore, very perceptible. The mean result of the earth's density proved to be 5,48 times that of water.

- VI. An Inquiry concerning the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F.R.S. M.R.I.A.  
(Concluded from p. 405.) — — — P. 453

Reduction of silver by means of charcoal in the humid way under the action of solar light, and likewise by the heat of boiling water without light. Reductions of gold by ether and solar light — by ethereal oil of turpentine and the heat of steam: Oxydes of gold and silver reduced by olive-oil in a similar process. Remarkable precipitation of the solution of gold upon magnesia by solar light.

- VII. Some Account of the Persian Cotton-Tree. By Matthew Guthrie, M.D. F.R.S. &c. &c. — — — P. 457

Account of various species of cotton, particularly the Persian cotton; its cultivation and valuable qualities.

- VIII. Facts and Observations concerning the Measure and Expence of first Movers, namely, Wind, Water, Steam, and Animal Strength, and on other Objects of general Utility. (W. N.) — — — P. 459

Great waste of labour and expence incurred by manufacturers and others, for want of a knowledge of the relative powers of men, horses, and other first movers.—Observation respecting the beneficial tendency of improvements in the application of these forces. Power of wind-mills. Horizontal mills. Water-wheels with inclined pallets. Instructions for measuring the quantity and power of streams of water. Explanation of the manner in which this power operates. Example in detail to ascertain the force of a stream to be applied to an undershot-wheel to raise water, to grind corn, or to perform the work of a certain number of men or horses. Enquiries respecting the medium effect of animal power. Overshot-wheels. Easy method of levelling to determine the fall of a stream of water.

- IX. Enquiries concerning the Invention and Practice of the Art of Hat-making. — — — — — P. 467

- X. New Observations on the Method of producing very loud Fulminations with various Bodies by Means of Phosphorus. By Citizen Brugnatelli. — — — — — P. 468

Experiments in which detonations were produced by placing a small portion of phosphorus upon various substances containing oxygen, and striking them with a hammer. These were nitrate of silver, oxygenated muriate of potash, the nitrates of bismuth and of mercury, common nitre, the sulphates (which did not succeed) and certain metallic oxydes. Experiments in which sulphur and charcoal were used instead of phosphorus. Additions and remarks by Van Mons.

Tunnel

Tunnel beneath the Thames. — — — P. 473

Abridged account of the plan and proceedings relating to the proposed tunnel beneath the Thames, at Gravesend.—List of the committee of subscribers, with other particulars relating to the subscription.

On Mr. Cartwright's Apparatus for rendering the pistons of Steam Engines tight by Metallic Fittings. — — — P. 476

## F E B R U A R Y 1799.

Engravings of the following Objects: 1. A New Air-pump. 2. A remarkable Lunar Halo: and, 3. Figures illustrating the lateral Motions of Fluids.

I. On the Corundum Stone from Asia. By the Right Hon. Charles Greville, F.R.S. — — — P. 477

Early notices of the corundum stone. Particular account, and topographical remarks on the place where it is found in India. It is in common use in China. Observations respecting its general properties, specific gravity, &c.

II. An Account of a singular Halo of the Moon. In a Letter from William Hall, Esq. of Whitehall, to Sir James Hall, Bart. F.R.S. Edin. P. 485

Lunar Halo, consisting of a circle round the moon of about  $12^{\circ}$  in diameter, with another passing through the moon of about  $112^{\circ}$ , and very much inclined to the horizon.

III. Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various hydraulic Phenomena. By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c. — — — P. 487

A sudden bend, or right angle, in a pipe, destroys nearly half the velocity of a fluid moving through it. Enlargements are scarcely less noxious. Explanation of the effects of the water-blowing machine. Curious method of draining land by a fall of water without machines. Causes and effects of the eddies and whirls in rivers.

IV. Concerning a new Variety of Argillaceous Iron-ore. By Samuel L. Mitchell, M.D. of New York. — — — P. 494

Argillaceous iron-ore figured like basalt. Argument deduced in favour of the aqueous origin of basalt.

V. Concerning the Invention of the Electrical Doubler. By Mr. John Read. — — — P. 495

Explanatory letter, with remarks on its contents.

VI. Inquiries respecting the Construction of a Water-wheel, and the Manufacture of Bricks. By a Correspondent. — — — P. 497

VII. On

VII. On the Combustion of Phosphorus. — — p. 498

VIII. Pyrometrical Essays to determine the Point to which Charcoal is a Non-conductor of Heat. By Citizen Guyton. — — p. 499

Comparative experiments with Wedgwood's pyrometer pieces, one of which was surrounded with charcoal, and the other with sand. The former piece was much less affected by the heat than the latter.

IX. Description of an Air-pump of a new Construction. By the Rev. James Little, of Lacken, in the County of Mayo, in Ireland. — p. 501

New Air-pump, with an horizontal barrel, solid piston, and stop-cock, between the barrel and the receiver.

X. Observations on Chemistry and Natural History. By Professor Vandelii. — — — — p. 508

Accounts of fossil Prussian blue at Minas Geraes, in Brazil, and also of a very large mass of native copper.

XI. On the Manufacture of Hats, and other Objects. By a Correspondent. p. 509

XII. Extract of a Letter from Citizen Ramond, Associate of the National Institute of France, &c. to Professor Haüy, Member of the Institute at Paris, respecting two Excursions to Mount Perdu, the most elevated Summit of the Pyrenean Mountains. — — — — p. 510

Various observations, the most remarkable character of which is that the most elevated summit of the Pyrenean mountains is calcareous, and abounds with the remains of marine animals.

XIII. An Abstract of a Memoir on the Fossil Bones of Animals. By Citizen Cuvier. — — — — p. 512

Enumeration of extinct species of animals, of which the bones have been found. Siberian mammoth. Animal of the Ohio. Animal whose teeth form turquois stone. Rhinoceros. Giant sloth of the royal cabinet at Madrid. Animal, or bear, of Bayreuth. — of Montmartre; — of Verona. Stag of Ireland, &c. Bees, &c. General Reflections.

XIV. Extract of a Memoir of Proust, entitled Enquiries concerning Tin. By Citizen Darcet. — — — — p. 515

Tin subjected to the action of strong nitric acid, or with the assistance of heat, acquires 40 parts of oxygen, and is insoluble; but with weak cold nitric acid it acquires only 30 parts. Experiments and observations respecting the transitions of oxygen.

XV. Scientific News, &c. p. 518

Construction of lantern pinions of glass for mill-work. Constitution of the Ligurian Institute. Institute of Cairo in Egypt.

XVI.

- XVI. Observations on the Differences which exist between the Acetous and Acetic Acids. By J. A. Chaptal. — — — p. 518

The acetous and acetic acids are not the same when equally diluted. The acetous acid contains a larger proportion of carbon.

- Accounts of Books, &c. — — — p. 523

Rollo on diabetes mellitus. Condorcet's method of accounts. On Mr. Lowry's ruling machine.

## M A R C H 1799.

Engravings of the following Objects: A new universal Electrometer, by Mr. Cuthbertson; and, 2. Configurations of the Crystals of Corundum Stone from Asia.

- I. An Account of Improvements in electrical Batteries; a Method of augmenting their Power, with Experiments; shewing the proportional Lengths of Wire fused by different Quantities of Electricity, and a Description of a new universal Electrometer. By Mr. John Cuthbertson, No. 53, Poland-street, London, 1799. — — — p. 525

Account of the power of batteries. Mr. Brooke's method of increasing the power of electrical jars. Discovery of a method of producing this effect, by rendering them damp. Description of a new electrometer, which shews the degree of electrization, the repulsive force in weight, and operates of itself as a discharger. Course of Experiments on the explosion of wind, by jars, in the dry, and in the damp states. Inquiry respecting the cause of this effect.

- II. Discovery of Sulphate of Strontan, near Sodbury, in Gloucestershire, by G. S. Gibbs, B.M. F.R.S. — — — p. 535

- III. On the Corundum Stone from Asia. By the Right Honourable Charles Greville, F.R.S. — — — p. 536

Great probability that corundum may be found in Europe. Observations on the History and great utility of Crystallography. An analytical description of the crystalline forms of corundum, by the Count De Bournon.

- IV. On Water-wheels. — — — p. 544

Observations on the power of different Water-wheels.

- V. On the Glass Trundles of Citizen Renaut; and the Duration of the Teeth of Mill-work. By C. B. — — — p. 546

- VI. On Dr. Parr's Theory of Light and Heat. By a Correspondent p. 547

- Scientific News, and Accounts of Books — — — p. 548

Institute of Cairo. An Account of the Operations carried on for accomplishing a Trigonometrical survey of England and Wales, from the commencement in 1784, to the end of the year 1796. Public Institution for diffusing knowledge and facilitating the general introduction of useful mechanical improvements.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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APRIL 1798.

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ARTICLE I.

*A Memoir and new Experiments on the Artificial Cinnabar of Mr. Kirchoff. By the Count APOLLOS DE MOUSSIN POUCHIN, Chamberlain to his Majesty the Emperor of all the Russias, Vice-President of the College of Mines, Honorary Member of the Imperial Academy of Sciences, at Petersburg\*, &c. &c.*

THE annual consumption of cinnabar by a variety of manufacturers and artists, and the certain sale of this beautiful colour, render it undoubtedly an interesting object of political economy for all states which possess the ores of quicksilver, as well as a branch of speculation for the merchant which has hitherto supported the manufactories of vermillion, and has even fixed the attention of the industrious Hollanders, who not being able to derive advantage from the fabrication of this product at first hand, have established, as it is said, mills for cinnabar, in which this colour gains the brilliancy and smoothness necessary for its employ in the arts. This process, which they mysteriously conceal, has hitherto been attended with profits the more considerable, as it is pretended that great part of the cinnabar which is adulterated by different processes is vended through Europe from these manufactories at first hand†. The methods by which cinnabar has hitherto been

\* Translated from the French manuscript, communicated by Charles Hatchett, Esq. who received it from the author, with some specimens of the cinnabar, which he finds to be of the best quality. The memoir was addressed to the Société des Amis Scrutateurs de la Nature.

† Mr Tuckert, who several times assisted, or was present during the preparation of artificial cinnabar at the manufactory of Mr. Brand, without the Utrecht gate at Amsterdam, has given an account of the processes

been obtained are too well known, and it would be superfluous to mention them here. But as the various inconveniences of sublimation, grinding, and the other preparations of this colour, are frequently prejudicial to the health, and sometimes expose the lives of the workmen to inevitable danger; and as the loss of vessels and the expence of fuel are but too frequently felt by those who undertake this fabrication; it is evident that a method of obtaining cinnabar subject to none of these inconveniences, and requiring little of expence excepting the first cost of the mercury, must become truly valuable to this branch of industry. We are indebted to Mr. Kirchoff, a young chemist of the greatest hopes, who is attached to the pharmaceutic department, for the first discovery of a method no less ingenious than obviously of great probable advantage to commerce. His claim in this respect is beyond controversy. For, though cinnabar has been obtained in the humid way by means of volatile liver of sulphur, there never could have been any hope of applying that method to the purposes of trade, on account of the heavy expence of the re-agent. But the intermedium used by Mr. Kirchoff promises, on the contrary, all the advantages which the most scrupulous calculation can require, and, when brought to perfection, will probably cause the method of sublimation to be abandoned by all who may be desirous of engaging in this branch of commerce, without exposing themselves to unnecessary risque.

I shall proceed to lay before this illustrious Society all the details of the process of Mr. Kirchoff, as well as the inconveniences which have hitherto presented themselves. It is with great pleasure that I avail myself of this opportunity of doing justice to the merits of this chemist, which have been hitherto concealed by his excess of modesty; and I have annexed to this memoir my own proper experiments, which have enabled me to remove some of the principal difficulties which Kirchoff met with in his work. I have no

in Crell's Journal, of which an abstract is inserted in the fourth volume of the *Annales de Chimie*, page 25. It is concisely as follows: (1.) Ethiops mineral is prepared by combining 150 pounds of sulphur with 1680 pounds of pure mercury, by a moderate heat, in a flat-bottomed polished iron vessel. 2. The ethiops, after slight pulverization, is put into earthen bottles, each capable of containing a quart of water. 3. Three large pots or sublimatory vessels, made of very pure clay and sand and luted, are then taken and placed upon iron circles over furnaces, constructed in such a manner that the flame of the fuel, which is turf, circulates freely round the vessels to two-thirds of their height. 4. When the vessels are red-hot, a bottle of the ethiops is poured into the first, another into the second, and another into the third. In the subsequent progress of the operation, two, three, and perhaps more bottles may be poured in at a time; but this depends on the strength of the inflammation exhibited by the ethiops after its introduction, the flame of which sometimes rises to the height of four and even six feet. When this is a little diminished, the mouth of the vessel is covered with a plate of iron, one foot square and an inch and a half thick, which perfectly closes it. In this manner during thirty-four hours the whole of the prepared matter is introduced into the three pots; that is to say, into each pot 360 pounds of mercury and 50 of sulphur. 5. The fire is then kept up till the sublimation is completed, and afterwards suffered to go out; which requires 36 hours from the time of the complete charge. It is judged to have the proper intensity, when, upon taking off the cover, a brisk flame appears, but does not rise more than three or four inches out of the pot. 6. During the last 36 hours, the mass is stirred every quarter or half hour with an iron rod. 7. When all is cold, the vessels are removed by means of the iron circles, which prevent their breaking. The cinnabar is taken out by breaking the vessel. Each pot constantly affords 400 pounds of cinnabar, the loss of original weight in each being 10 pounds. The cinnabar does not attach itself to the plates of iron, because they are continually taken off excepting towards the end, when the vessels are left untouched. These plates are not in the least corroded. N.

other

other claim than that of having endeavoured to render his discovery as useful as possible to technical chemists.

Mr. Kirchoff, who has operated in the small way only, gives the following proportions for the production of cinnabar in the humid way:

Three hundred grains of mercury are triturated in a vessel or cup of porcelain, with 68 grains of flowers of sulphur, until an ethiops is accurately produced, in which no metallic globules can be perceived by the magnifier. As this degree of union of the mercury and sulphur cannot be obtained when both are perfectly dry, Mr. Kirchoff adds a few drops of a solution of caustic vegetable alkali; and when the union of the mercury and sulphur is complete, he adds to the mixture a solution of 160 grains of the same alkali dissolved in an equal quantity of water. He heats the vessel containing these ingredients over the flame of a candle, and triturates without interruption during the heating with a glass pestle. In proportion as the liquid evaporates, he adds clear water from time to time, so that the ethiops may be constantly covered to the depth of near an inch. For the above quantities, it is requisite to continue the trituration nearly two hours; at the end of which time the mixture begins to change from its original black colour to a brown, which usually happens when a large part of the fluid is evaporated. From this first moment the transition from brown to red is very rapid. No more water is then to be added, but the trituration must be continued without intermission. When the mass has acquired the consistence of a jelly, the red colour becomes more and more bright with an incredible degree of quickness. This is the most important point of time for the success of the operation. For, if the heat be not continued long enough, the colour will not acquire the utmost degree of beauty; and on the contrary, if it be continued only two seconds too long, the fine red of the cinnabar passes as quick as lightning to a dirty brown. Mr. Kirchoff, as well as Mr. Lowitz, who has repeated his experiments, could not succeed in amending this brown colour; which consequently renders the success of the operation very precarious. This inconvenience is productive of another; namely, that it is nearly impossible, at the instant of the production of the colour of the cinnabar, to make any observation of the quality of the tint. For the colour of the liver of sulphur masks that of the cinnabar; so that in some instances the most beautiful red is obtained, and in others a colour inclining to orange, which, though bright and vivid, is not the tinge required in this product. In one of his experiments, Mr. Kirchoff obtained a cinnabar which absolutely possessed the colour of carmine; but he had this good fortune only once, when he had no expectation of so beautiful a product, and did not therefore note the attendant circumstances.

This is nearly the whole detail of the experiments hitherto made by Messrs. Kirchoff and Lowitz. The cinnabar they obtained still exhibited mercury in the metallic state, which it was rather difficult to get rid of, and did not appear until after the filtration of the liver of sulphur, and the edulcoration and drying of the vermilion.

I shall now proceed to relate what I have done respecting this object, principally with a view to facilitate the manufacturing in the large way, the restoration of the cinnabar which had assumed a brown colour instead of red, and the certainty of the tint in masses somewhat considerable. I am very far from being content with the success of my experiments; but I think I have at least discovered great part of the theory of the operation, and that in

practice the manufacturer will soon discover the necessary proportions and manipulations. I therefore claim and hope for the grant of indulgence in favour of the desire I have, that the valuable discovery of Kirchoff may speedily become as useful to society as undoubtedly it will sooner or later be.

The first experiment I made in small, according to the exact proportions of Kirchoff, did not succeed. I could not transform my ethiops into cinnabar. I attribute this want of success to an evaporation too rapid, and the want of a sufficient quantity of fluid; not having added water in proportion as the mixture became thick, so that the whole soon became perfectly dry. The mass became strongly attached to the china cup, and constantly preserved its black colour. I recommenced the operation with more success, and all my experiments afforded cinnabar more or less beautiful, provided the mixture at the commencement was kept at the proper degree of fluidity. Most frequently however I obtained a colour, which, though very brilliant, inclined rather to yellow than red; a fault which the cinnabar of sublimation possesses but too frequently, and is sometimes a proof of its falsification. Among the colours which I obtained, brown presented itself but too frequently, in consequence of having neglected the moment of terminating the operation. I made the following experiments on these defective colours:

After welledulcorating a cinnabar of this kind, I poured on three different portions: the nitrous, the acetous, and the muriatic acids. These were exposed for several hours to a mild digesting heat. I had supposed that the defect of the colour might arise either from a partial reduction of the mercury, or partial fusion of the sulphur, or perhaps from an adulteration of the mercury by lead. In these three experiments, therefore, my aim was to dissolve the reduced mercury, to oxide the sulphur, or to carry off the lead by the acetic acid. None of these experiments answered my expectation; for, though part of the mercury was dissolved and part of the sulphur oxidized, the colour continued brown. That with the nitrous acid was even darker. The acetic acid afforded no indications of lead.

As the result of these experiments showed that the bad colour of the cinnabar did not arise from a portion of reduced mercury, I wished to know whether the metal were not combined with too large a portion of sulphur; for which purpose, I digested caustic alkali on the cinnabar. The alkaline solution became yellowish, and the colour of the cinnabar somewhat lighter, but was not a good red. It always inclined to yellow; and though it approached very near the colour of common cinnabar, and was even more vivid, yet it was not red enough.

I had little expectation of restoring the proper colour and vividness to my damaged cinnabar. Nothing more remained but to try the addition of mercury to the brown colour which was the object of my research. I therefore moistened near an ounce and a half of this colour with water, then gradually added near three ounces of running mercury, and triturated the whole on a very hard Siberian porphyry. Contrary to my expectation, the mass, instead of becoming black, assumed the colour of an extremely pale cinnabar, without the smallest degree of fire or liveliness. I then mixed two drams of the flowers of sulphur, and collected the whole in a porcelaine cup. Upon this I poured a solution of the caustic alkali, evaporated as before to a very thick consistence. The mixture passed from yellow to an obscure brown, and at the moment of the thickening a large quantity of running mercury was seen at the bottom of the cup. I added a dram and a half of sulphur,

sulphur, with a few drops of water, and trituated the whole until the mercury had entirely combined with the rest of the mixture. I then added water, and began the evaporation, agitating the mixture continually with the pestle. The brown colour began towards the end of the evaporation to brighten, and at last changed to the colour of bad sealing-wax, constantly inclining to brown. Foreseeing that if I continued the evaporation I should risk a second obscuration of the colour, I placed the cup which contained the mixture on a stove which had been heated. At that time I totally despaired of correcting the colour of cinnabar spoiled at the first evaporation; but having inspected my cinnabar at the end of two hours, I saw with pleasure that the colour had become finer as the mass became thicker. I trituated it in the cup with the addition of a few drops of water, and repeated this operation five or six times in the course of the day. At the end of 24 hours, my cinnabar was very thick and very good. I added more water, and left the colour on the stove for three days; at the end of which the cinnabar had acquired the utmost brilliancy, and was equal to the finest specimens of Mr. Kirchoff, excepting that in which he had obtained a shade of carmine, as I have already remarked. After repeating this experiment with success with one pound of mercury, thirty-five drams of flowers of sulphur, and ten ounces of caustic alkali, I found reason to make the following general observations upon this process:

It is necessary for the production of cinnabar,

1. That the mercury should be perfectly mixed with the flowers of sulphur; for which reason, after having trituated it with the pestle in a glass mortar, with the addition of a small quantity of caustic alkali to moisten it, it must be levigated upon porphyry, or such other hard stone as cannot mix its particles with those of the cinnabar, because every foreign substance is highly prejudicial to the beauty of the colour. If the quantities operated upon exceed one pound, it will be necessary to procure a mill for grinding colours, which must also be of a siliceous stone. The mixture upon a levigating stone is evidently too expensive for a manufacturing process in the large way.

2. The best vessels for evaporating the mixture are those of porcelaine, or the yellow ware of England. The pestles ought to be glass.

3. The heat of a sand-bath is sufficient to this operation. The heat of ebullition is not necessary; but at the beginning, particularly when there is much liquid, the heat must approach as nearly as possible to that of boiling.

4. The receptacle in which the sand is placed must be deep enough to admit the whole of the porcelaine or earthen vessel in which the cinnabar is produced, to be plunged therein, and environed by an equal atmosphere of heat. The sand round the vessel need not exceed the thickness of a full inch.

5. The mixture must be perpetually stirred: otherwise the mass will subside by its weight, and become clotted; a circumstance which it is essential to avoid.

6. A certain degree of thickness is requisite during the operation, in order to afford a good colour; but great care must be taken not to suffer the mass to become too thick, and more particularly the agitation must be strongly kept up at these periods of condensation.

7. Though it is possible to obtain from the sand-bath a very beautiful cinnabar which may be immediatelyedulcorated, yet it is much more prudent and advantageous not to  
suffer

suffer the product to acquire its whole perfection on the evaporating furnace, but to take it in time from the sand-bath, and leave it for three or four days in a degree of heat equal to that of the stove of an apartment after it has been heated for two or three hours. Care must be taken to add a few drops of water, and to agitate the mixture from time to time. By this precaution, the disagreeable event of seeing the colour pass from red to yellow, and often to brown, is avoided, as well as the subsequent labour and expence of trituration with mercury, and additional liver of sulphur.

8. Before the cinnabar is edulcorated, it may be left to become almost perfectly dry by the heat of the stove; by which means the colour will acquire the most brilliant tint.

After this account of the production of cinnabar in the humid way, I shall speak of the edulcoration and drying of this colour, which have presented phenomena of the greatest importance to the fabrication in the large way, and are alone sufficient to secure real advantages to the speculator in this article.

Having obtained ten ounces of cinnabar, I poured twenty pounds of boiling water upon it in a large earthen jar, for the purpose of disengaging the liver of sulphur. At that moment the liquid was obscured and became absolutely black. I slightly agitated the mixture. The cinnabar very speedily fell to the bottom, and permitted me to decant this black liquor, of which I shall hereafter speak. After having poured other water upon the cinnabar, which was considerably beautiful, it acquired a slight yellow colour. After the settling of the colour, I decanted it, and continued to edulcorate until the decanted water had neither the saline sulphureous taste, nor hepatic smell.

When I had poured for the last time five or six pounds of boiling water on my colour, I agitated it strongly, and poured it out into a vessel of crystal glass. A plentiful deposition of colour was made in an instant. It was not bad, but did not possess the brilliancy of the particles which floated in the water above this deposition. I agitated the water very slightly, that it might become more highly charged with colouring matter, but not so much as entirely to derange the mass which had subsided. I rapidly decanted the liquid part into another vessel. The most dense portion remained at the bottom, and on the sides of the first glass, and was of a very beautiful colour. I repeated this washing four times with the same precautions, taking care always to pour the liquid part hastily into another vessel. The residue of the last washing was entirely brown, and the tints of my cinnabar were beautiful in the same order as I had observed in the decantation. That is to say, the colour in the first vessel was more brilliant and less deep than in the second, and so of the others. This experiment showed not only that washing adds to the beauty of the cinnabar obtained by the process of Kirchoff, but that it is possible, more especially in works on a large scale, to obtain a great variety of tints by mixture, without admitting any foreign colour or material.

After having thus obtained different cinnabars by washing, I dried them on a furnace slightly heated, and found them more or less deep, but generally very good. They did not, however, possess the fire of such cinnabars as inclined to a yellow colour. I put these colours into capsules of paper, and left them for some days on the same stove, which had continued to be regularly heated. At the end of this time, upon opening my papers it appeared to me that the colour was improved; and had in particular become more lively

than on the first day. In order to ascertain whether this phenomenon was owing to simple drying, or to heat, I placed a pinch of the lightest colour on a small piece of glass, which I heated very strongly with a candle. I then enjoyed a very beautiful sight, of which the result gave me the greatest pleasure. The powder of cinnabar became brown in an instant, and soon afterwards nearly black, or rather of the deepest violet grey; but when, on the supposition that I had spoiled my cinnabar, I threw it from the glass upon a paper, I was very agreeably surprised to observe it in the twinkling of an eye pass through all the transitions from obscure violet to a beautiful carmine red, and to observe more particularly that this colour had acquired the fire and brilliancy which was wanting before the experiment. As an object of comparison, I had some cinnabar of commerce, of which the pond, or forty pounds (Russian), is sold at Petersburg for eighty roubles. I put a pinch of this on the same slip of glass, and subjected it to the same treatment. The colour speedily became black, and, after cooling, acquired a very bad deep brown tinge, resembling umber of a bad quality, without recovering its original red. I repeated this experiment in a larger way with the same success, and by that means ascertained a method of giving in general much brilliancy to my cinnabar, and of producing the most brilliant carmine tint. This heat, which must be very strong, requires, nevertheless, to be conducted with much prudence. The cinnabar must not fume, nor exhibit grey globules on its surface; for in these cases it would be effectually spoiled. This phenomenon seems to prove a decomposition and absorption of air; but it remains to be shown whether it be hydrogen, oxygen, or azote, which the cinnabar takes up in this experiment.

These constitute the most essential facts I have observed with regard to the production of cinnabar in the humid way. I intend, when time permits, to give a continuation of this memoir. I have already made some new experiments, which, though they may not yet promise any advantage to the manufacturer in the large way, may be useful to the painter and interesting to the chemist.

The black liquor which I at first decanted from my cinnabar, deposited a small portion of ethiops, not surcomposed. I must add in conclusion, that the manufacturer in the large way must of course submit his colour to several elutriations, collect the depositions in several vessels, and afterwards mix his colours when dried. It would be likewise necessary for the finer sorts, to provide a furnace, in which either the carmine colour or the requisite fire and brilliancy may be given. A furnace of this kind may very easily be contrived, and it would be unnecessary to describe its construction in this place. It is my wish, that the society to whom I have the honour to address the present memoir will consider it as a proof of my respect and gratitude. By admitting me into the number of its members, a new proof is afforded of its indulgence to the weakest efforts in favour of the sciences and natural history. The excellent discovery of Mr. Kirchoff has led me to ascertain some new facts, of which I am desirous that commerce should have the advantage; but the honour of the discovery belongs to him, and deserves to be the more insisted on, as his modesty is no less conspicuous than his knowledge.

## II.

*On the Luminous Appearance said to be exhibited by Phosphorus in Azotic Gas.*

By DR. ALEXANDER NICHOLAS SCHERER, Counsellor of the Mines to his Serene Highness the Duke of Saxe-Weimar\*.

AT the time when the principles of the new theory were newly discussed, the enquiries of chemists were employed upon some of its chief assertions. Besides examining the experiments that metallic oxyds do emit oxygen gas during their reduction, attention was also paid to another, by which it was observed, that the combustion of bodies in oxygen gas is always accompanied by a total absorption.

Mr. Goettling, who exerted his industry on such experiments, made use of the apparatus he employed in several experiments of the same nature. He accidentally observed, that the phosphorus continues to shine for some time in the azotic gas, which remains after its combustion in atmospheric air. What he principally observes on this head is, that phosphorus shines very strongly in azotic gas, even more so than in atmospherical air; that it is converted into phosphoric acid by this process; and that the azotic gas totally disappears. Hence he was led to infer, that azotic gas must contain oxygen, as well as the oxygen gas itself does.—But if so, what is the difference between these two species of gas? This question was apparently solved by another experiment he made. Phosphorus does not shine so well in oxygen gas at a low temperature, as it does in the azotic; and when phosphorus gives light in atmospherical air, it is always with extrication of heat; whereas the light is not attended with heat in azotic gas. He concludes therefore, that the azotic gas consists of oxygen combined with light, in the same manner as, on the other hand, the oxygen gas consists of that principle combined with caloric; and moreover that caloric has a stronger affinity to oxygen than light has.—On this Mr. Goettling builds an entirely new theory of chemistry, from the consideration that the new mixture of azotic gas, which he pretends to have discovered, does not admit of the hitherto adopted way of explaining its influence.

Mr. Goettling has explained these facts at large in his *Beitrag zur Berichtigung der antiphlogistischen Chemie*. Weimar 1794. This work was universally well received, and some were inclined to give it full credit without further examination. Mr. Goettling himself has publicly explained his new theory, and adapted the whole of his chemical lectures to that system. A few philosophers only ventured to offer their doubts concerning it. Among these are Dr. Eimbke; *Intelligenzblatt der allg. litt. Zeit.* 1794, No. 92, and Yelin; *Erlang. gel. Zeit.* 1794, No. 80. The former observed, that when phosphorus had ceased to shine in atmospheric air, it would afford no more light in the remaining azotic gas. The second found, that phosphorus, after undergoing a very long continued heat in atmospherical air, does also leave behind an æriform residuum, in which the phosphorus will by no means shine any longer.

At this time I had commenced a public course of lectures on Chemistry. I therefore thought myself obliged to make new enquiries for the purpose of giving an impartial ac-

\* Received from the Author.



count of this new discovery; but to avoid being led into error by individual observations, as is frequently the case with experiments, I joined Dr. Jaeger from Stuttgart, who was just then visiting me on a journey. We made a great number of experiments together, which we endeavoured to diversify in every possible manner, in order to discover all the concurrent circumstances. In the *Jena gel. Zeit.* 1794, No. 113, we published a short account of one of the chief occurrences; but the full description of the results from our investigations some time after was given in the following book, *Über das Leuchten des Phosphors im atmosphärischen Stickgas.* Weimar 1795. To this work we have added another treatise on Goettling's theory by Dr. Pfaff.

The chief aim of this enquiry was to shew by decisive experiments, *whether phosphorus does really give light in azotic gas, and whether it be totally decomposed by it?* Neither of these two we could find by any process of operation. We made comparative experiments over water and over quicksilver, and clearly perceived that Mr. Goettling, having closed his vessels merely by water, could not avoid a source of error. We spared no exertions to investigate as exactly as possible the method of preparing azotic gas, and by that means discovered a variety of interesting circumstances. We always found that the fumes of *phosphoreous acid*, rising by a momentary combustion of phosphorus in atmospherical air, did so far envelope the remaining portion of oxygen, that the luminous appearance from the union of this with another part of the phosphorus could not be again produced until the water had absorbed the acid fumes. We always found however that this light was of short duration, accompanied by a very inconsiderable diminution of the elastic fluid, and that it was altogether impossible to cause the phosphorus to emit any more light in the remaining air. On this account we had every reason to consider this as pure azotic gas, and to declare those experiments of Mr. Goettling incorrect, from which the combustion of phosphorus was said to take place in the *purest azotic gas*.

To all this Mr. Goettling made no reply, except that he gave an answer in the *Intelligenzbl. der allgem. Literat. Zeit.* 1794, No. 117, to our first very short notice, tending to place the whole investigation in an unfavourable point of view. Of our treatise, though printed above two years ago, he has not yet taken any notice.

Soon afterwards Mr. Girtanner took some pains to explain the result of Mr. Goettling's experiments, *Ibid.* 1795, No. 23, by supposing the azotic gas employed had contained some water, which was decomposed by the phosphorus. He did not, however, take the trouble to ascertain by experiments, whether phosphorus does really give light in azotic gas thoroughly freed from oxygen. Mr. Goettling replied to this, by alledging, that if the water which exists dissolved in the gases were the cause of this light, the phosphorus ought also to emit light in oxygen gas, which according to his experiments did not take place. *Ibid.* No. 27. He afterwards published a more ample elucidation of the short answer here related in the *Almanach fuer Scheide Kuenstler*, 1795, asserting that the luminous appearance of phosphorus could not be obtained in an azotic gas, which had been procured by a long continued heating of the phosphorus in atmospheric air, because in that case the phosphorus mixes with the azotic gas, and prevents it from maintaining the light. He added, that the gas thus circumstanced is found to recover this property by adding a few drops of an acid. But he was by no means aware that his observation contained a very important objection against his own theory. For if, according to that theory, azotic

gas be really a compound of oxygen and light, why should the phosphorus, on being mixed with it, cease to act on its oxygen? Ought it not rather to do this more speedily when the cohesion of its parts is destroyed? Besides which, the addition of acids is not fair, because more acid than is necessary to saturate the phosphorus dissolved in it, may in this way become united with the azotic gas. Thus I have found that the phosphorus, which does not shine in pure carbonic acid gas, begins to give light in that medium as soon as a small portion of acid is added. The answer given by Dr. Jaeger was more explicit. He discovered in particular, that the volume of the azotic gas was never diminished when the luminous appearance of the phosphorus was reproduced by means of acids. *Gren's Neues Journal der Physik.* B. II.

It was not long after, that Professor Lampadius published his experiments on this subject. At first he seemed to find every thing exactly as described by Mr. Goettling, *Ibid.* B. I.; but soon afterwards he made the interesting discovery, that by a careful decomposition of atmospherical air, by means of nitrous gas, azotic gas may be procured, so free from oxygen, that phosphorus cannot be made to shine in it. *Sammlung pract. chemisch. Abhandl.* B. I.

Professor Hildebrandt repeated Mr. Goettling's experiments with the greatest care. *Crell's Annal.* 1796, B. I. These experiments are, no doubt, the most exact of any that were made for this purpose. He has perfectly confirmed our experiments. He has proved this beyond all question, by several experiments, of which I shall mention but one.—In the same azotic gas in which phosphorus had ceased to shine, and fresh phosphorus would not afford light, the luminous appearance took place as soon as oxygen gas was added. But this lasted no longer than till the latter was consumed; for there was precisely as much of the whole volume of the mixture absorbed, as made up the quantity of the oxygen gas that was added.

There are several well known papers on this subject, in the *Annales de Chimie*, written by French chemists; but none of their authors have paid the least attention to what has been transacted on this matter: a circumstance which I have thought proper to be mentioned in this place. But they have added the important fact, that phosphorus is soluble in azotic gas. Probably they had no intelligence of what has been written about it in Germany; and as this appears to me to be the case with England, I considered it of some importance, to give the shortest possible view of the chief points of these transactions. I have endeavoured to give a more ample detail of all the particular facts, as far as they were known down to October 1796, in a work of mine, equally unknown in this country: *Nachtraege zu den Grunzuegen der neuern chemischen Theorie*, pag. 326—349, in which the whole extent of these enquiries may be seen with greater ease by the systematical arrangement of the experiments. It is to be hoped that the dispute will end here, and that the facts, (1.) that no shining of the phosphorus can in any respect take place in such azotic gas as is quite free of oxygen, and (2.) that it cannot be made to shine unless some other body be added which is capable of affording oxygen,—will be admitted as incontrovertibly established.

LONDON, Feb. 27, 1798.

## III.

*On M. LAZOWSKI'S new Barometer, or Weather Instrument. By a Correspondent.*

*To Mr. NICHOLSON, Editor of the Philosophical Journal.*

SIR,

Kendal, Jan. 31, 1798.

THE following experimental essay is too trifling to require an introductory apology, were it not for the high name of Dr. Hutton, which unavoidably occurs in the course of it, as the subject of the present communication was first suggested by a paragraph in his Mathematical and Philosophical Dictionary, published in 1795.

When any thing new in science is announced, it ought to be made public as generally and as speedily as possible. In this respect Dr. Hutton has only done his duty as a compiler, in bringing the English reader acquainted with the supposed discovery of a new instrument capable of indicating approaching changes of the weather. On the other hand, when any thing novel is communicated to the lovers of science, it is the business and undoubted right of every friend to enquiry, not to receive the proposal on bare authority, but to subject it to the test of argument and experiment, in order to establish it as a fact, or refute it as an error.

Having premised thus much in my own vindication, I will in the next place transcribe the paragraph alluded to above. "To the foregoing may be added a new sort of Barometer, or Weather Instrument, by the sound of a wire. This is mentioned by M. Lazowski, in his Tour through Switzerland: it is as yet but in an imperfect state, and was lately discovered there by accident. It seems that a clergyman, though near-sighted, often amused himself with firing at a mark, and contrived to stretch a wire so as to draw the mark to him to see how he had aimed. He observed that the wire sometimes sounded as if it vibrated like a musical cord; and that after such soundings, a change always ensued in the state of the atmosphere, from whence he came to predict rain, or fine weather, &c.\*"

After perusing this singular narrative, I found myself at a loss in attempting to refer the discovery to any known class of phenomena, or to explain it by the affections of the atmosphere, considered as depending on the barometer, electricity, or even the hygrometer, without admitting a supposition, which is not discountenanced by the preceding report, viz. that one end of the wire was fixed to a frame of wood, while the other end was stretched over a nail or metal pin in the same by a weight. Under these circumstances, it appeared not impossible, that the wood-work might contract and expand from successive variations in the air's humidity, thereby putting the pin in motion, which may be supposed to scratch the wire, and give rise to the sound in question. But with a view to ascertain if a vibratory motion can be excited in metallic strings by any other change in the state of the atmosphere, I fixed a number of copper and iron wires, in the beginning of April 1795, in an open place: they were different lengths, from three to six feet, and of different diameters, varying from the thickness of a fine thread to the size of a small cord; they were all stretched by metal pins, resembling those used in a harpichord, and so disposed as to make various angles with the meridian and the horizon. The apparatus, being thus completed,

\* See Hutton's Dictionary, article *Barometer*.

was carefully attended to through the summer, particularly when the aspect of the sky and state of the barometer seemed to predict an approaching change; but the enquiry was fruitless, the promised sound was never heard; nor did any other circumstance worthy notice occur, except that some of the smallest wires snapped occasionally in the cold nights of the spring. The failure of this experiment seems to confirm the supposition that M. Lazowski's instrument is at best but an imperfect Hygrometer, and that it does not promise any new discovery relative to the properties or composition of the atmosphere \*. J. G.

## IV.

*Observations on Scylla and Charybdis †. By the Abbé LAZZARO SPALLANZANI, Professor of Natural History at Pavia, F.R.S. &c. &c.*

SCYLLA and Charybdis, according to the fables of the poets, are two sea monsters whose dreadful jaws are continually distended to swallow unhappy mariners; the one situated on the right, and the other on the left extremity of the Strait of Messina, where Sicily fronts Italy.

Dextrum Scylla latus, lævum implacata Charybdis  
Obsidet: atque imo harathri ter gurgite vastos

\* The passage in Dr. Hutton's Dictionary will admit of the sense, that the observer (who perhaps never took notice of his wire, except when busied in the amusement there mentioned) did not hear any spontaneous sound, but merely perceived a musical vibration at certain times during the act of drawing the mark along the wire, which must have agitated it. This might arise from tension, by some hygrometric change in the situation of the points of support. It may be remarked, that a wire for the use the clergyman applied it to must have been much longer than six feet; and that it was probably less stretched than the wires exposed by my correspondent.

If the sounds were spontaneous, the fact will be referable to the Eolian harp of Mercurius, since re-invented by Oswald. The instrument consists of a sounding board about two feet or more in length, upon which ten or twelve (catgut) strings of equal length but different thickness are tuned in unison, at as low a pitch as the smallest of the strings can bear. When this is placed in a current of air, such for example as passes through a window partly opened, it emits a variety of contemporaneous and successive tones, which from their changes in melody, harmony, and swell, and a certain wild strangeness in the whole effect, rivet the attention, and sometimes afford exquisite pleasure. The Eolian harp has been celebrated by Thomson and other poets. I have not made experiments to analyse its mode of operation; but am disposed to think, (1) that it can only give the trumpet notes; (2) that the note to be afforded by any one string depends on the quantity of the impulse of the wind being greatest near one end of that string; (3) that the same quantity and direction of impulse will agitate a longer portion of the slack small string than of the tenor thick string, so that in the vibratory subdivisions the smaller string will give the graver tone; (4) that a powerful tone drawn from one string may dispose the other strings to vibrate unisons, fifths, thirds, octaves, and other concords, more or less remote according to the circumstances.

If M. Lazowski's wire acted upon the principle of the Eolian harp, it might be presumed that the recurrence as well as the nature of the sound emitted would be governed by the force and angle of direction of the wind; and, if so, much would depend on the exposure the direction and the tension of the wire. Whether on this supposition it could afford any more certain indication of approaching change of weather, than is usually obtained from a simple observation by the wind vane, must be decided by facts, if such should hereafter appear. N.

† Travels in the Two Sicilies. English Transl. IV. 168.

Sorbet in abruptum fluctus, rursusque sub auras  
 Erigit alternos, et sidera verberat undâ.  
 At Scyllam cæcis cohibet spelunca latebris,  
 Ora exertantem, et naves in saxa trahentem.  
 Prima hominis facies, et pulchro pectore virgo  
 Pube tenus : postrema immani corpore pristis,  
 Delphinum caudas utero commissa luporum.

VIRG. Æneid. Lib. III.

Far on the right her dogs foul Scylla hides :  
 Charybdis roaring on the left presides,  
 And in her greedy whirlpool sucks the tides,  
 Then spouts them from below : with fury driv'n  
 The waves mount up, and wash the face of heav'n.  
 But Scylla from her den with open jaws  
 The sinking vessel in her eddy draws,  
 Then dashes on the rocks. A human face  
 And virgin bosom hide her tail's disgrace ;  
 Her parts obscene below the waves descend  
 With dogs enclos'd, and in a dolphin end.

DRYDEN.

I have no difficulty in availing myself of the description of a poet in a work dedicated to the investigation of truth, nor shall I hesitate to cite similar passages from another poet ; since, however exaggerated these may be by the glowing colours of imagination, they contain truth, and afford a subject for interesting enquiries.

I should have thought myself to have merited the greatest censure, if, when I was in the strait of Messina, I had not visited two places of which so much has been written, and which have been rendered so famous by the numerous shipwrecks they have occasioned.

I first proceeded in a small boat to Scylla. This is a lofty rock, distant twelve miles from Messina, which rises almost perpendicularly from the sea on the shore of Calabria, and beyond which is the small city of the same name. Though there was scarcely any wind, I began to hear, two miles before I came to the rock, a murmur and noise like a confused barking of dogs, and, on a nearer approach, readily discovered the cause. This rock, in its lower parts, contains a number of caverns, one of the largest of which is called by the people there *Dragara*. The waves, when in the least agitated, rushing into these caverns, break, dash, throw up frothy bubbles, and thus occasion these various and multiplied sounds. I then perceived with how much truth and resemblance of nature Homer and Virgil, in their personifications of Scylla, had portrayed this scene, by describing the monster they drew as lurking in the darkness of a vast cavern, surrounded by ravenous barking mastiffs, together with wolves to increase the horror :

Εὐθα δ' ἐνὶ Σκυλλῇ καὶ δεινὸν Λελαήνῃ  
 Τῆς ἥτοι φωνῇ μὲν ὅση σκυλακὸς νεογλῆς  
 Γυνέσται.

HOM. Odyss. XII.

Here

Here Scylla bellows from her dire abodes,  
 Tremendous pest ! abhorr'd by man and gods !  
 Hideous her voice, and with less terrors roar  
 The whelps of lions in the midnight hour !

POPE.

The Greek poet, when he pourtrays the rock which is the habitation of Scylla, finishes the picture higher than the Latin, by representing it as so lofty that its summit is continually wrapped in the clouds, and so steep, smooth and slippery, that no mortal could ascend it, though he had twenty hands and twenty feet.

Οἱ δὲ δύο σκοπεῖται, ὁ μὲν οὐρανὸν εὐρὺν ἰκάνει  
 Ὀξεῖν κορυφῇ, νεφέλῃ δὲ μιν ἀμφιβέβηκε  
 Κυανέη, τὸ μὲν οὐπὸτ' ἔρῳει, οὐδέ ποτ' αἰδρῇ  
 Κεῖνου ἔχει κορυφὴν, οὐτ' ἐν θέρει οὐτ' ἐν ὀπῳῇ.  
 Οὐδὲ κεν ἀμβραῖν βροῖος αἰθρῆς, οὐ καλαβαῖν,  
 Οὐδ' εἰ οἱ χεῖρες γέ εἰκοσι καὶ πόδες ἦεν.  
 Πείρη γὰρ λῆς ἐστὶ περιξέστη εἰκυσία.

HOM. Odyss. XII.

High in the air the rock its summit shrouds  
 In brooding tempests and in rolling clouds ;  
 Loud storms around and mists eternal rise,  
 Beat its bleak brow and intercept the skies.  
 When all the broad expansion, bright with day,  
 Glows with th' autumnal or the summer ray ;  
 The summer and the autumn glow in vain,  
 The sky for ever low'rs, for ever clouds remain.  
 Impervious to the step of man it stands,  
 Though borne by twenty feet, though arm'd with twenty hands.  
 Smooth as the polish of the mirror rise  
 The slippery sides, and shoot into the skies.

POPE.

Such, three thousand years ago, or nearly so, appeared the rock of Scylla, according to the observation of Homer ; and such is nearly its appearance at this day.

The accuracy of this truly "first great painter of antiquity," which has likewise been observed by scientific travellers in other descriptions which he has given, shews that the level of the waters of the sea was at that time at nearly the same height as at present, since, had it sunk only a few fathom, it must have left the foot of the rock, which, according to my observations, is not very deep, entirely dry. And this I consider as one among several strong arguments, that the most remarkable sinkings of the sea are anterior to the time of Homer.

Such is the situation and appearance of Scylla : let us now consider the danger it occasions to mariners. Though the tide is almost imperceptible in the open parts of the Mediterranean, it is very strong in the strait of Messina, in consequence of the narrowness of the channel,

channel, and is regulated, as in other places, by the periodical elevations and depression of the water. Where the flow or current is accompanied by a wind blowing the same way, vessels have nothing to fear, since they either do not enter the strait, both the wind and the stream opposing them, but cast anchor at the entrance; or, if both are favourable, enter on full sail, and pass through with such rapidity that they seem to fly over the water. But when the current runs from south, to north, and the north wind blows hard at the same time, the ship which expected easily to pass the strait with the wind in its stern, on its entering the channel is resisted by the opposite current, and, impelled by two forces in contrary directions, is at length dashed on the rock of Scylla, or driven on the neighbouring sands; unless the pilot shall apply for the succour necessary for his preservation. For, to give assistance in case of such accidents, four-and-twenty of the strongest, boldest and most experienced sailors, well acquainted with the place, are stationed night and day along the shore of Messina; who, at the report of guns fired as signals of distress from any vessel, hasten to its assistance, and tow it with one of their light boats. The current, where it is strongest, does not extend over the whole strait, but winds through it in intricate meanders, with the course of which these men are perfectly acquainted, and are thus able to guide the ship in such a manner as to avoid it. Should the pilot, however, confiding in his own skill, condemn or neglect this assistance, however great his ability or experience, he would run the most imminent risk of being shipwrecked. In this agitation and conflict of the waters, forced one way by the current, and driven in a contrary direction by the wind, it is useless to throw the line to discover the depth of the bottom, the violence of the current frequently carrying the lead almost on the surface of the water. The strongest cables, though some feet in circumference, break like small cords. Should two or three anchors be thrown out, the bottom is so rocky that they either take no hold, or, if they should, are soon loosened by the violence of the waves. Every expedient afforded by the art of navigation, though it might succeed in saving a ship in other parts of the Mediterranean, or even the tremendous ocean, is useless here. The only means of avoiding being dashed against the rocks or driven upon the sands in the midst of this furious contest of the winds and waves, is to have recourse to the skill and courage of these Messinese seamen.

In proof of the truth of this assertion, I might adduce many instances related to me by persons deserving of credit. But I was myself an eye-witness to the situation of a trading vessel from Marseilles, which had one day entered the Strait by the mouth on the north side, at the time that I was on a hill looking towards the sea. The current, and a north wind which then blew strong, being both in its favour, the vessel proceeded under full sail into, and had passed one half of the Strait, when, on a sudden, the sky became overcast with thick clouds, and violent gusts of wind arose, which in an instant changed the direction of the current, and turned up the sea from its bottom. The mariners had scarcely time to hand the sails, while the furious waves broke over the ship on every side. Whether they merely followed the practice usual with ships in distress, or whether they were acquainted with the laudable custom of the Messinese, I cannot say; but they fired two guns; immediately upon which one of the barks employed on this service hastened to the assistance of the distressed vessel, and, taking it in tow, began to make every exertion to carry it safely into the harbour.

If I had seen with fear and shuddering the danger of the sailors on board the vessel,  
which

which I expected every moment would be swallowed up in the waves; I beheld with wonder and pleasure the address and bravery of the Messinese mariners, who had undertaken to steer safely through so stormy a sea the ship entrusted to their care. They extricated it from the current, which impelled it towards destruction; changed the helm to this side, or to that; reefed or set the sails, as the wind increased, or abated; avoided the impetuous shocks of the waves, by meeting them with the prow, or opposing to them the side, as either method appeared most proper to break their violence; and by these and other manœuvres, which I am unable to describe, these brave mariners, amid this dreadful conflict of the sea and the wind, succeeded in their undertaking, and brought the vessel safe into the harbour.

But enough of Scylla:—we will now proceed to Charybdis. This is situated, within the Strait, in that part of the sea which lies between a projection of land named *Punta Secca*, and another projection on which stands the tower called *Lanterna*, or the light-house, a light being placed at its top to guide vessels which may enter the harbour by night.

On consulting the authors who have written of Charybdis, we find that they all supposed it to be a whirlpool. The first who has asserted this is Homer, who has represented Charybdis as a monster, which three times in a day drinks up the water, and three times vomits it forth.

——— δια Χάρυβδιδος ἀναρροῖ βδεῖ μελάν ὕδωρ,  
 Τρὶς μὲν γὰρ τ' ἀνιήσιν ἐπ' ἡμᾶσι, τρὶς δ' ἀναρροῖ βδεῖ  
 Δείνον.

Hom. *Odyss.* XII.

Beneath Charybdis holds her boisterous reign  
 'Midst roaring whirlpools, and absorbs the main;  
 Thrice in her gulphs the boiling seas subside,  
 Thrice in dire thunders she refunds the tide. POPE.

The description of Virgil above cited, differs from that of Homer only in placing a deep gulph below. Strabo, Isidorus, Tzetzes, Hesychius, Didymus, Eustathius, &c. repeat the same. The Count de Buffon adopts the idea of Homer in full confidence, and places Charybdis among the most celebrated whirlpools of the sea. “Charybdis, in the strait of Messina, absorbs and rejects the water three times in twenty-four hours\*.” Strabo tells us, that the fragments of ships swallowed up in this whirlpool are carried by the current to the shore of Tauromenium (the present Taormina) thirty miles distant from Charybdis†. In confirmation of this tradition, an amusing though tragical anecdote is related of one Colas, a Messinean diver, who, from being able to remain a long time under water, had acquired the surname of *Pesce* (the fish). It is reported that Frederic, king of Sicily, coming to Messina purposely to see him, made trial of his abilities with a cruel kind of liberality, by throwing a golden cup into Charybdis, which if he brought up was to be the reward of his resolution and dexterity. The hardy diver, after having twice astonished the spectators by remaining under water a prodigious length of time, when he plunged the third time appeared no more; but, some days after, his body was found on the coast, near Taormina.

\* Buffon, *Hist. Nat.* tom. ii. in 12mo.

† Καταπονθέντων δὲ καὶ διαλυθέντων τὰ ναυαγία παρασύρεται πρὸς τὴν αὐτὴν τῆς Ταυρομενίας. Lib. VI.



From the authorities here adduced, it is evident that Charybdis has hitherto been considered as a real whirlpool, by both ancient and modern travellers who have given any account of it.

As I was therefore so near to this celebrated place, I determined to endeavour to ascertain, if possible, what it really is. It is distant from the shore of Messina about seven hundred and fifty feet, and is called by the people of the country *Calofaro*, not from the agitation of the waves as some have supposed, but from *καλος* and *φαρος*; that is, *the beautiful tower*, from the light-house erected near it for the guidance of vessels. The phenomenon of the Calofaro is observable when the current is descending; for, when the current sets in from the north, the pilots call it the *descending rema*\*, or current; and when it runs from the south, the *ascending rema*. The current ascends or descends at the rising or setting of the moon, and continues for six hours. In the interval between each ascent or descent, there is a calm which lasts at least a quarter of an hour, but not longer than an hour. Afterwards, at the rising or setting of the moon, the current enters from the north, making various angles of incidence with the shore, and at length reaches the Calofaro. This delay sometimes continues two hours. Sometimes it immediately falls into the Calofaro, and then experience has taught that it is a certain token of bad weather.

As I was assured by the pilots most experienced in this practical knowledge, that there was no danger in visiting the Calofaro, I resolved to avail myself of the opportunity. The bark in which I made the excursion was managed by four expert mariners, who, perceiving me somewhat intimidated as I approached the place, encouraged me, and assured me they would give me a very near view of the Calofaro, and even carry me into it without the least danger.

When I observed Charybdis from the shore, it appeared like a group of tumultuous waters; which group, as I approached, became more extensive and more agitated. I was carried to the edge, where I stopped some time to make the requisite observations, and was then convinced beyond the shadow of a doubt, that what I saw was by no means a vortex, or whirlpool.

Hydrologists teach us, that by a whirlpool in a running water we are to understand that circular course which it takes in certain circumstances; and that this course, or revolution, generates in the middle a hollow inverted cone, of a greater or less depth, the internal sides of which have a spiral motion. But I perceived nothing of this kind in the Calofaro. Its revolving motion was circumscribed to a circle of at most an hundred feet in diameter; within which limits there was no incurvation of any kind, nor vertiginous motion, but an incessant undulation of agitated waters which rose, fell, beat, and dashed on each other. Yet these irregular motions were so far placid, that nothing was to be feared in passing over the spot, which I did; though our little bark rocked very much from the continual agitation, so that we were obliged constantly to make use of our oars to prevent its being driven out of the Calofaro. I threw substances of different kinds into the stream. Such as were specifically heavier than the water sunk, and appeared no more; those which

\* I have observed that at Messina, as well as in other parts of Sicily, words of the Greek language, which was once that of this island, are still retained. Thus the word *rema* derived from *ρεμα*, a flowing, or stream, is used to signify the current of this strait.

were lighter remained on the surface, but were soon driven out of the revolving circle by the agitation of the water.

Though from these observations I was convinced that there was no gulph under the Calofaro; as otherwise there would have been a whirlpool, which would have carried down into it the floating substances; I determined to sound the bottom with the plummet, and found its greatest depth did not exceed five hundred feet. I was likewise informed, to my no small surprise, that beyond the Calofaro, towards the middle of the strait, the depth was double.

I could not therefore but conclude from these facts, that at that time there was no whirlpool in Charybdis. I say *at that time*, since the case might be very different when the sea was tempestuous. I therefore made enquiry relative to this of the pilots, those, especially, who, from their tried experience, were appointed by the public to give assistance in storms to foreign vessels, and who had frequently seen Charybdis in its greatest fury. The following is the substance of the answers they gave me:

When the current and the wind are contrary to each other, and both in their greatest violence, especially when the scilocco, or south wind, blows; the swelling and dashing of the waves within the Calofaro is much stronger, more impetuous, and more extensive. It then contains three or four small whirlpools, or even more, according to the greatness of its extent and violence. If, at this time, small vessels are driven into the Calofaro by the current, or the wind, they are seen to whirl round, rock, and plunge; but are never drawn down into the vortex. They only sink when filled with water, by the waves beating over them. When vessels of a larger size are forced into it, whatever wind they have they cannot extricate themselves; their sails are useless; and after having been for some time tossed about by the waves, if they are not assisted by the pilots of the country, who know how to bring them out of the course of the current, they are furiously driven upon the neighbouring shore of the Lanterna, where they are wrecked, and the greater part of their crews perish in the waves\*.

If we consider maturely these facts, we shall find that a great part of what has been written relative to Charybdis is very erroneous. We have seen how many authors, from Homer to the present time, have described it as a real whirlpool, or great gulph revolving in itself, within the circumference of which should any ship enter, it is immediately drawn to the centre and swallowed up. When the current is dying away, or when there is no current, this description has no resemblance to truth. Charybdis is then perfectly innocent, as I have been fully convinced by my own observations; and even when it is agitated

\* The following account of the shipwreck of a vessel in the Calofaro was sent me, after my return from Sicily, by the Abbate Grano from Messina:

“About three weeks ago, we were spectators of the sinking of a Neapolitan polacca in the Calofaro, on its passage from Puglia, laden with corn. A most violent south-easterly wind blew, and the vessel, with all sails set, endeavoured to reach the harbour, standing off from the Calofaro; but the head of the current from the entrance by the faro took her, and drew her impetuously into it; where, without being able to make use of her sails, she remained for some time tossed about by the waves, which at length, either breaking over her, or opening her sides by their furious beating, sent her to the bottom. The crew, however, and a part of the cargo were saved by the speedy assistance given by our mariners in two small barks, who had the courage to encounter the danger. You will perceive from this, in what manner the waves may sink ships in Charybdis, without the necessity of supposing a whirlpool.”

and dangerous, it still contains no incavation, or gulph of the nature of a vortex; but merely a strong agitation and dashing of its waves, which produces those small whirlings of its waters, which are only accidental and not to be feared. So far likewise is Charybdis from drawing to itself and swallowing vessels, that it rather repels them, and throws them to a distance.

This error has arisen like many others with respect to the productions of nature. Homer, in relating the voyage of Ulysses through the Strait of Messina, was the first who described Charybdis as an immense vortex, which absorbs and rejects the water, and the ships that approach it; exemplifying his account by the fate of some of the companions of his hero, who were carried away by the whirlpool. The writers who came after him, whether poets, orators, historians, or geographers, have followed him in this description, without any one of them taking the pains to repair to the place and examine it himself. Even Fazello the Sicilian, who was so industrious in ascertaining facts, and whose accounts of his country are so accurate, clearly shews in his description of Charybdis, that he had never observed it himself; and concludes his narration with the erroneous supposition above cited, that the things swallowed up by Charybdis are conveyed by submarine currents to the shores of Taormina.

Among all who have written on this subject, we only find Cluverius who seems, at least at first view, to have visited the place. I shall transcribe his words:

“Ego sane, cum Charybdis noscendæ gratia aliquot dies Messanæ subsisterem, et ab hominibus ejus loci, maximè vero nautis, non Siculis modo, sed et Belgis, Britannis et Gallis, qui hoc fretum frequentes navigant, diligentius eam rem sciscitarer, nihil omnino certi ipsis perdiscere potui, adeo scilicet totum negotium omnibus obscurum et incognitum erat. Tandem tamen reperi Charybdim, quæ incolis, patriis vocabulis, dicitur Calosaro, sub prædicta ad Messanensem portum pharo esse mare rapidè fluens, atque in vortices actum: quod non *επις επι' ημερη* ut tradit Homerus, id est *singulis diebus ter*, absorbet ingenti gurgite, revomitque aquas, sed quoties vehementiori fluctu fretum comitatur.”

“I remained some days at Messina, with a view to obtain some information relative to Charybdis; but though I made every enquiry of the people of the place, and principally the sailors, not the Sicilian only, but the Italian, Dutch, English, and French, who frequently navigate that Strait, I could learn nothing satisfactory, so little was known by them on the subject. At length however I found Charybdis, which the natives call *Calosaro*, under the lighthouse before mentioned near the harbour, to be a sea rapidly flowing, and forming vortices. It does not absorb the waters in its vast gulph, and reject them *thrice in a day*, as Homer tells us; but as often as the sea runs high in the Strait.”

From the expression “I found Charybdis” we might be induced to believe, that he made his observations on the spot. It is certain however that he does not explicitly tell us so: and when treating of a phenomenon, of which he was so anxious to obtain an accurate knowledge, which he could not procure even from the Messinese sailors, it is strongly to be presumed, that he would not have suppressed a circumstance of that importance. As Charybdis may be seen from the shore, if he only went thither and turned his eyes towards it, he might with truth assert that he had discovered it. The other adjuncts to his account, that Charybdis is a rapid sea, and that it absorbs and rejects the water in

a storm, convince me that he had not a just idea of it, but satisfied himself with the old tradition concerning Charybdis.

It may be observed, that the situation of Charybdis, as it has been hitherto described, does not exactly agree with that assigned it by Homer. Let us refer to the poet. The goddess Circe gives the following directions to Ulysses, with respect to the navigation of the Strait of Messina :

‘Οἱ δὲ δύο σκοπελοὶ, ὁ μὲν οὐρανὸν εὖρυν ἱκάνει . . .  
 Τὸν δ’ ἕτερον σκοπελὸν χθοναλωτέρῳ οὔει Ὀδυσσεύ,  
 Πλησίον ἀλλήλων, καὶ κεν δίοισευσεῖας.  
 Τῷ δ’ ἐν εἰνεῶς ἐστὶ μέγας φυλοῖσι τεθῆλως,  
 Τῷ δ’ ὑποδία Χάρυβδις ἀναρρόεσθαι μέλαν ὕδωρ.      HOM. Odyss. XII.

High o’er the main two rocks exalt their brow . . .  
 Close by, a rock of less enormous height  
 Breaks the wild waves, and forms a dangerous streight;  
 Full on its crown a fig’s green branches rise,  
 And shoot a leafy forest to the skies:  
 Beneath, Charybdis holds her boisterous reign  
 ‘Midst roaring whirlpools, and absorbs the main.      POPE.

The first of the rocks here mentioned by Homer is Scylla, which he describes at length; and near the other, according to this poet, Charybdis is situated. The distance from one of these rocks to the other is an arrow’s flight, καὶ κεν δίοισευσεῖας, which does not at all accord with the present situation of Scylla. How are we to explain this disagreement? Shall we say that Homer, availing himself of the licence in which poets are indulged, has spoken hyperbolically? I know not whether the connoisseurs in poetry will permit such a licence. Or shall we suppose that Charybdis was once much nearer to Scylla; but that in a long series of ages it has changed its place, and removed under Messina? Such a suggestion might, perhaps, be favourably received, if in remote times any considerable change had taken place in the Strait: but we know not of any; and it is not probable, that a change so remarkable as the removal of Charybdis from its place would have been passed over in silence by Sicilian writers. Within the present century, it is true, this Strait, of which so much has been said, has become narrower; but at the same time we know, that long before this event Charybdis was situated where it is at present. The ancient and uninterrupted tradition of the Messinese respecting this fact is confirmed by the authority of the most celebrated Italian, Latin, and Greek writers. Fazello tells us, “Charybdis ex parte Siciliæ paulo supra Messanam.” “Charybdis is situated on the side of Sicily, a little beyond Messina.”—Ovid says,

“Hinc ego dum muter, vel me Zanclexa Charybdis  
 “Devoret!”  
 “Let dire Charybdis in Zanclexan seas,  
 “Devour me if I change!”

And it is well known that Zancle was the ancient name of Messina, now Messina. Tzetzes in Lycophron says, Ἡ Χάρυβδις περὶ Μεσσηνῆς ἐστὶ. “Charybdis is situated near Messina.”

finæ." Strabo likewise, after having mentioned Messina, proceeds, Δεικνυται και Χαρυβδις, μικρον προ της πολεως, εν τω πορθμω. "Charybdis is seen in the strait a little before we reach the city." Several other writers might be cited to the same purpose.

From all these reasons and historical testimonies we must then conclude that Homer was not exact with respect to the situation of Charybdis; nor can it be a great offence to say, that in this passage of his long poem he has certainly nodded. The accuracy of several of his descriptions of various places in Sicily cannot be denied. It is such that we must either suppose that he had himself travelled in those parts, as is the opinion of many; or at least that he had procured very faithful and circumstantial information from others. Of this the rocks of Scylla are an example. But, as to the supposed whirlpool of Charybdis, and its situation, I think we may venture to affirm he never saw it himself, and that the accounts he had received of it led him into error.

We will now enquire what foundation there is for the saying, which became proverbial, that "he who endeavours to avoid Charybdis dashes upon Scylla;" and which was applied by the ancients to those who, while they sought to shun one evil, fell into a worse.

On this subject I likewise made enquiries of the Messinese pilots abovementioned, and to what better masters could I apply for the elucidation of such a proverb? They told me that this misfortune, though not always, yet frequently happens, unless proper measures are taken in time to prevent it. If a ship be extricated from the fury of Charybdis, and carried by a strong southerly wind along the strait towards the northern entrance, it will pass out safely; but should it meet with a wind in a nearly opposite direction, it would become the sport of both these winds, and, unable to advance or recede, be driven in a middle course between their two directions, that is to say, full upon the rock of Scylla, if it be not immediately assisted by the pilots. They added, that in these hurricanes a land wind frequently rises, which descends from a narrow pass in Calabria, and increases the force with which the ship is impelled towards the rock.

Before I began to write on Scylla and Charybdis, I perused the greater part of the ancient authors who have written on the subject. I observe that they almost all represent these disastrous places in the most gloomy and terrifying colours, as continually the scene of tempests and shipwrecks. These terrors and this destruction, however, they are far from exhibiting in the present times, it rarely happening that any ships are lost in this channel, either because their pilots possess the knowledge requisite for their preservation, or because they apply for the necessary assistance. Whence then arises this great difference between ancient times and the present? Can we suppose that Scylla and Charybdis have changed their nature, and become less dangerous? With respect to the former, we have seen that this hypothesis is contradicted by fact; Scylla still remaining such as it was in the time of Homer: and with regard to the latter, from the Strait of Messina becoming narrower, Charybdis must be at present more to be feared than formerly, as it is well known that an arm, channel, or strait of the sea is the more dangerous in proportion as it is narrow. I am rather of opinion that this difference arises from the improvement of the art of navigation, which formerly, in its infancy, dared not launch into the open sea, but only creep along the shore, as if holding it with its hand.

Alter remus aquas, alter tibi radat arenas,  
Tutus eris; medio maxima turba mari.

PROPERT. Lib. III.

To shun the dangers of the ocean, sweep  
The sands with one oar, and with one the deep.

But time, study, and experience have rendered her more mature, better informed, and more courageous; so that she can now pass the widest seas, brave the most violent tempests, and laugh at the fears of her childhood.

To exemplify and support the probability of this opinion, it will not be necessary to recur to the early and rude ages; much more modern times will furnish us with sufficient proofs. That part of the Adriatic, which separates Venice from Rovigno in Istria, is certainly not the most propitious sea to navigators. The danger of being hurried in six hours from one shore to the other, and there stranded; the frequency of violent winds which prevail there; the shallows and sand-banks which break the waves and render them wild and irregular, may certainly cause some serious reflection in those who embark to make the passage. So lately as the last century, the shipwrecks in this sea were so numerous, and had so terrified the people of Rovigno, that, when any one was obliged by urgent business or any other cause to go to Venice, he considered himself as more likely to die than live; and, if he was the father of a family, used to make his will before he embarked. The Advocate Constantini, a native of that country, and a man of learning and ingenuity, told me when I was there, that he had read more than one of these testaments, deposited among the public archives.

But at present I will not say it is a diversion or pleasure to make this passage, since, as storms are not unfrequent, it is necessary to be cautious; but serious accidents rarely happen. I have myself three times made it without meeting with any cause of alarm. To what can this difference be attributed, but to the improvement of the nautical art? Besides that the mariners of Rovigno were not then so expert in the management of their vessels as at present; they made use of certain barks of so improper a construction, as I was assured by the abovementioned Constantini, that it was impossible they should long resist the violence of the sea. Those on the contrary that have been built since that time, being of a broad and flat figure and very solid, are capable of withstanding the most furious storms. They are there called *bracere*, and are in great reputation in all the neighbouring countries. We here find a part of the sea in which vessels were formerly so frequently wrecked, and which could not be traversed but at the risk of life, now deprived of all its terrors, and rendered easily passable, merely by the improvements made in the art of navigation.

As a farther and still more convincing proof that the dangers of Charybdis and Scylla, though in themselves the same that they anciently were, have been diminished, and the dread they inspired removed by the rapid advances to perfection which this art has made in modern times; I shall adduce an example in another sea no less an object of terror from tempests and shipwrecks, I mean the Cape of Good Hope, called the Stormy Cape by the first discoverer, and, by the mariners of those times, the Raging Lion. How dreadful were the dangers of this place, where the two oceans descending down the oppo-

sits

the sides of Africa met and clashed together; where contending winds, whose power was greater in the boundless ocean; where mountainous waves, rocks, and whirlpools threatened inevitable destruction! What preparations, what caution were thought necessary for the ship which was to make this dangerous passage? Able pilots who had frequently made the voyage; masts and yards secured by additional ropes; a large supply of sails and cables, thicker and stronger than usual; and a double rudder, that in case one should be damaged there might be another to act. The mariners were to be fastened to their posts by strong ropes; the passengers shut down below, and the deck left clear for the crew; a number of whom stood with hatchets in their hands, ready to cut away the masts should it be necessary. The guns were stowed in the hold as ballast, and the port-holes, windows, and every kind of aperture, carefully closed. Such were the precautions taken in the last century, on doubling the Cape of Good Hope; but how few of them are now necessary to perform this voyage in perfect safety! Of this I have had the satisfaction to be certified by an English gentleman, Mr. Macpherson, with whom I had the pleasure of conversing in Pavia, in July 1790; and who had twice doubled this Cape in his voyages to India; a gentleman of great respectability for his information, for the various long voyages he has made, and the honourable employments he has held.

The facility with which this passage may now be made, is therefore the consequence of the perfection to which the art of navigation has arrived; and the same we may conclude with respect to Charybdis and Scylla, which at present have nothing terrible but the name, to those who pass them with the requisite precautions.

## V.

*Instructions for refining Saltpetre by a new Process. By J. A. CHAPTAL, J. P. CHAMPR and BONJOUR\*.*

THE crude saltpetre is to be beaten small with mallets, in order that the water may more easily attack every part of the mass. The saltpetre is then to be put into tubs, five or six hundred pounds in each tub. Twenty per cent. of water is to be poured into each tub, and the mixture well stirred. It must be left to macerate, or digest, until the specific gravity of the fluid ceases to augment. Six or seven hours are sufficient for this first operation, and the water acquires the density of between 25 and 35 degrees. (Sp. Gr. 1.21, and 1.306. See *Philos. Journal* I. 39.)

The first water must then be poured off, and a second portion of water must be poured on the same saltpetre amounting to 10 per cent.; after which the mixture must be stirred up, suffered to macerate for one hour, and the fluid drawn or poured off.

Five per cent. of water must then be poured on the saltpetre; and after stirring the whole, the fluid must be immediately drawn off.

When the water is drained from the saltpetre, the salt must be thrown into a boiler containing 50 per cent. of boiling water. When the solution is made, it will mark between 66 and 68 degrees of the hydrometer. (Sp. Gr. 1.848, and 1.898.)

\* Translated from the *Journal de Physique*, bearing date August 1794, but lately published.

The solution is to be poured into a proper vessel, where it deposits by cooling about two-thirds of the saltpetre originally taken. The precipitation begins in about half an hour, and terminates in between four and six hours. But as it is of importance to obtain the saltpetre in small needles, because in this form it is more easily dried, it is necessary to agitate the fluid during the whole time of the crystallization. A slight motion is communicated to this liquid mass by a kind of rake, in consequence of which the crystals are deposited in very slender needles.

In proportion as the crystals fall down, they are scraped to the borders of the vessel, whence they are taken with a skimmer, and thrown to drain in baskets placed on treffels, in such a manner that the water which passes through may either fall into the crystallizing vessel, or be received in basins placed underneath.

The saltpetre is afterwards put into wooden vessels in the form of a mill-hopper or inverted pyramid with a double bottom. The upper bottom is placed two inches above the lower on wooden ledges, and has many small perforations through which water may pass to the lower bottom, which likewise affords a passage by one single aperture. A reservoir is placed beneath. The crystallized saltpetre is washed in these vessels with 5 per cent. of water; which water is afterwards employed in the solution of saltpetre in subsequent operations.

The saltpetre, after sufficient draining, and being dried by exposure to the air upon tables for several hours, may then be employed in the manufacture of gunpowder.

But when it is required to use the saltpetre in the speedy and immediate manufacture of gunpowder, it must be dried much more strongly. This may be effected in a stove, or more simply by heating it in a flat metallic vessel. For this purpose the saltpetre is to be put into the vessel to the depth of five or six inches, and heated to 40 or 50 degrees of the thermometer (or about 135 of Fahrenheit). The saltpetre is to be stirred for two or three hours, and dried so much that, when strongly pressed in the hand, it shall acquire no consistence, nor adhere together, but resemble a very fine dry sand. This degree of dryness is not required when the powder is made by pounding.

From these circumstances we find, that two saline liquids remain after the operation, (1) the water from the washing; and (2) that from the crystallizing vessels.

We have already remarked, that the washing of the saltpetre is performed in three successive operations, in which, upon the whole, the quantity of fluid made use of amounts to 35 per cent. of the weight of the crude saltpetre. These washings are established on the principle, that cold water dissolves the muriates of soda, and the earthy nitrates and muriates, together with the colouring principle, but scarcely attacks the nitrate of potash.

The water of these three washings therefore contains the muriate of soda, the earthy salts, the colouring principle, and a small quantity of nitrate of potash, the amount of which is in proportion to that of the muriate of soda, which determines its solution.

The water of the crystallizing vessels contains a portion of the muriates of soda, and of the earthy salts which escaped the operation of washing, and a quantity of nitrate of potash, which is more considerable than that of the former solution.

The waters made use of at the end of the operation, to whiten and wash the crystals deposited in the pyramidal vessel, contain nothing but a small quantity of nitrate of potash.

These



These waters are therefore very different in their nature. The water of the washings is really a mother water. It must be collected in vessels, and treated with potash by the known processes. It must be evaporated to 66 degrees (or 1,848 sp. gr.), taking out the muriate of soda as it falls. This solution is to be saturated with 2 or 3 per cent. of potash, then suffered to settle, decanted and poured into crystallizing vessels, where 20 per cent. of water is to be added to keep the whole of the muriate of soda suspended.

The waters which are thus obtained by treatment of the mother water, may be mixed with the water of the first crystallization. From these the marine salt may be separated by simple evaporation; and the nitrate of potash, which they hold in solution, may be afterwards obtained by cooling.

The small quantity of water made use of to wash and whiten the refined saltpetre, contains nothing but the nitrate of potash: it may therefore be used in the solution of the saltpetre when taken from the tubs.

From this description it follows, that a manufactory for the speedy refining of saltpetre ought to be provided with (1) mallets or rammers for pounding the saltpetre, (2) tubs for washing, (3) a boiler for solution, (4) a crystallizing vessel of copper or lead, in which the saltpetre is to be obtained by cooling, (5) baskets to drain the crystals, (6) a wooden case or hopper for the last washing and draining the saltpetre, (7) scales and weights for weighing, (8) hydrometers and thermometers to ascertain densities and temperatures, (9) rakes to agitate the liquor in the crystallizing vessel, (10) skimmers to take out the crystals and convey them to the baskets, (11) syphons or hand-pumps to empty the boilers.

The number and dimensions of these several articles must vary according to the quantity of saltpetre intended to be refined.

If it be proposed to refine ten thousand weight of crude saltpetre per day, the requisite men and utensils may be determined as follows:

Part of the ground near the magazine may be disposed for conveniently breaking and pounding the saltpetre.

This ground should be paved with large flat stones very uniformly, or with thick pieces of wood. Mallets similar to those used in pulverizing gypsum may be applied to this use. Two men are sufficient to weigh and pound the saltpetre, and stow it in the magazine.

As the three washings require two days, and each tub can hold only five or six hundred pounds of saltpetre, it would require twenty days to refine ten thousand weight (with one tub).

These tubs are two feet and a half in height, and the same in diameter. They must be very well made, in order that the water of the washing may not leak out. They are to be placed solidly on a plane slightly inclined, of such a material as shall not imbibe the water which may be spilt during the operation, but transmit it to a reservoir placed at the extremity of the row of tubs.

Twenty of these tubs must be disposed in two parallel lines; the planes on which they are set may incline towards each other, and form, by their union, the gutter or cavity for transmitting into the common reservoir such waters as may escape. These tubs are perforated at the distance of two inches from the bottom. The aperture is closed by a spigot.

Four men may be appropriated to the washing of the saltpetre. It is a part of their duty to convey the saltpetre from the magazine to the tubs, and from the tubs to the boiler.

It is scarcely necessary to observe that the tubs must stand sufficiently apart, and be so disposed that the work may be easy and convenient.

A boiler of a conical form, five feet wide and four deep, will serve for three operations per day, and consequently to refine fifteen thousand weight. A single man is sufficient to attend the boiler.

The vessel for crystallization is of lead or copper, and must be as near the boiler as possible. Its depth is fifteen inches, its length ten feet, and its width eight. It must be placed on a very solid support, so that its bottom may every where rest upon it. It is convenient to raise this support of masonry about twelve inches above the ground. By this means the borders of the crystallizing vessel will be 27 inches above the floor, which will render the operation easy and convenient.

It has appeared to us of advantage to give to the bottom of the crystallizing vessel, an inclination of four inches from the sides to the middle, merely in the longitudinal direction. The solutions may be emptied for several successive times from the boilers into the crystallizing vessel, after having taken out the crystals deposited from each solution.

Four men appear necessary for the operation of the crystallizing vessel. Their business is to agitate the fluid continually with the rakes. They collect without intermission on the borders of the vessel the crystals which fall down, and convey them with a skimmer to the baskets prepared for their reception and draining. These same workmen put the saltpetre into the wooden vessel for the last washing and drainage, and carry the refined saltpetre into the magazine.

For want of a large cooler for crystallization a shallow boiler may be used, or the same vessels which serve for crystallization in the present works for refining this salt.

To prepare the saltpetre for the manufacture of gunpowder, it may be dried, after refining, by two processes, (1) by exposing it to the open air, or the sun, for several hours, upon tables; or (2) by exposing it in a shallow metallic vessel for two hours, to the heat of 40 or 50 degrees (about 135 Fahrenheit). In either case it must be agitated and stirred with scarcely any interruption, in order to dry it speedily and equally.

Considerable experience has shewn us, that the process here described is the most simple and economical. But, to prevent others from trying such methods as might seem promising, though we have thought fit to reject them, it will be proper to offer the following remarks:

1. Trial has been made to dissolve the saltpetre, crystallize it, and then wash it to separate the sea-salt.

This process appears most advantageous at first sight, because it saves the pounding; but it has great inconveniences. 1. Crude saltpetre dissolved in 50 per cent. of water, and poured into the vessel for crystallization, does not deposit the same quantity of saltpetre as it would do if washed before the solution. This difference depends on the muriate of soda in the crude saltpetre, which facilitates the solution of the nitrate of potash, and consequently the water of the crystallizing vessels must retain in solution more nitrate of potash, when the crude saltpetre is dissolved, than when it has been previously washed with cold

cold water, and deprived of the marine salt which it contains. 2. The washing of saltpetre, when it is performed after the solution and crystallization, requires 40 or 50 per cent. of water instead of 35.

3. Trial has been made to dissolve saltpetre in 20 or 25 per cent. of boiling water; to take out the sea-salt in proportion as it falls by the ebullition of the liquor; to dilute the fluid with 30 per cent. of additional water, and then to convey it into the vessel of crystallization. It was expected that the washings with cold water might be avoided, or considerably diminished by these means; but, not to mention that ebullition maintained for four or five hours to separate the sea-salt supposes very great consumption of time, fuel, and saltpetre, the washings are still indispensable to remove the colouring principle, and to carry off the last portions of muriate of soda.

4. It may be thought that the quantity of water in the washings might probably be diminished; but there is reason to fear that when the saltpetre is loaded with sea-salt, it cannot be perfectly refined by the use of a less quantity of water than we have prescribed.

5. The operator may also be tempted to diminish the proportion of water employed in the solution; but we are convinced by numerous experiments, that this proportion is the most suitable. If it be increased, the saltpetre remains dissolved in the fluid; if it be diminished, it settles or falls down in a solid mass. Observation has proved, that the degree of saturation best adapted to this work, is between the 66th and 68th degrees of the hydrometer (sp. gr. 1.848, and 1.866.)

6. It may likewise be thought more simple and economical to treat the solution of crude saltpetre with potash. But it is to be feared in this case that part of the alkali might be employed in decomposing the muriate of soda, to convert it into muriate of potash; and it must be observed, that this last salt is not at all proper to decompose the earthy nitrates, notwithstanding the assertions of skilful chemists to that effect.

It appears, therefore, more convenient to defer the treatment of the mother waters, and not to use potash till after the sea-salt has been separated by evaporation.

This process, therefore, unites a number of advantages.

1. It consumes much less fuel: for, instead of two long solutions and ebullitions, nothing more is required than to give the water a boiling heat in order to dissolve the saltpetre.

2. It requires less time. Three days are sufficient to purify the saltpetre to the degree suitable for making gunpowder.

3. It disposes the saltpetre to dry more readily. As the crystals are no larger than small needles, a few hours' exposure to the air are sufficient for its complete desiccation. This advantage is inestimable, particularly in a season wherein several months would be required to drain the large loaves of nitre; and in which, consequently, the fabrication of gunpowder would be either retarded or suspended, and the drying-houses encumbered with quantities of humid nitre.

4. It requires less space. A boiler five feet in diameter and four in depth, a vessel for crystallization of a few feet dimensions, and thirty tubs, are perfectly sufficient to refine fifteen thousand weight daily.

5. It occasions less loss. Very accurate experiments have shown that the solutions used in the ancient process occasioned a loss of saltpetre, by mere evaporation, which amounted to 7 per cent. of the original quantity. By this new process, the water which holds the salt-

petre in solution is never heated to boiling, the salt does not remain in the boiler, and the evaporation is almost nothing.

## VI.

*On the Light emitted by Supersaturated Borate of Soda, or Common Borax. By Mr. F. ACCUM.*

*To Mr. NICHOLSON.*

SIR,

**T**HAT two flints and several other siliceous stones, struck against each other, appear luminous on the side struck upon; and that phosphate of lime, tremolite, sugar, gum elemi, black jack, and various resins become luminous, and emit phosphoric sparks in the dark, when scratched with a sharp instrument, or struck against one another, are well known to every tyro of natural philosophy; but that *supersaturated borate of soda* possesses this property in the highest degree, has not perhaps been hitherto remarked. Two pieces of this salt, of considerable magnitude, struck against another, or a swift blow with any sharp instrument upon it, produces such a flash of white light, as none of the before-mentioned substances are capable of giving. It deserves therefore a place under the class of those kinds of phosphoric substances which give a perceptible light by attrition or percussion, without having been exposed either to the solar or artificial light; for which reason I take the liberty of laying this before you, begging you will give these lines a place in your much-admired Journal of Natural Philosophy.

I remain, Sir, your very humble servant,

FREDERICK ACCUM.

*Hay-Market, No. 17.*

## VII.

*New Construction of the Air Pump. By Sir GEORGE S. MACKENZIE, Bart.*

**F**IG. I. plate 1, represents a section of the barrel. C is a cup for oil to moisten the collar of leathers L, in which the piston rod R works.  $\phi$  is a plug—P P P P is the piston, which is solid, except a space for a collar of leathers  $\lambda$ , through which the wire W attached to the valve V, passes into a perforation in the piston rod. N is a small nut to prevent the valve from rising too high. This method of lifting the valve was invented many years ago by Dr. Rutherford of Edinburgh. X is a perforation in the side of the bottom of the barrel, into which is inserted a piece of metal, as in fig. 5, with a silk valve tied over it, opening downwards into the pipe K. E is the pipe leading to the receiver.

The construction of the bottom of the barrel is seen in fig. 2 and 3. Fig. 4 is the valve and wire. Fig. 5 shews the construction of the pipe E leading to the receiver, which is better than bent copper tubes, as these are apt to crack. Fig. 6 is cast solid and bored.

Fig,

Fig. 7 is the cap screw by which the pipes are fixed. Fig. 8 is the pipe used for the condensing apparatus.

That this pump may work well, it is necessary that the bottom of the piston be perfectly flush with the bottom of the barrel.—The method of operation is as follows:

When the piston is to be raised, let the plug  $\phi$  be opened.—The piston rising, expels the air above it through  $\phi$ . When the piston is at the highest, shut the plug. There will now be no pressure above the piston, which will greatly facilitate the working. As the piston rises, the friction of the collar of leathers  $\lambda$  will raise the valve  $V$ , and the air in the receiver through the communication  $E$  will expand itself into the barrel. When the piston is depressed,  $V$  shuts, and the air is expelled through  $K$ , to which a pipe as fig. 8 may be attached for condensation. When the piston reaches the bottom, no air will be left in the barrel, except the very small quantity in the very small hole of the valve  $X$ , which is very little when compared to the capacity of the barrel. By proceeding in this manner, a very perfect vacuum will be formed in the receiver.—By taking off the receiver and applying the pipe fig. 8, and attaching it to any vessel, and opening the plug  $\phi$ , we have a complete condensing apparatus. If required, the air may be taken from the receiver and thrown into another vessel. Moistened leather ought not to be used for fixing the receiver, as vapours are constantly issuing from it; a drying lute is better.

This air pump may be made of a much cheaper construction than that of the plate, which, however, is the more convenient.

In the obliging letter which accompanied this communication, the author assures me, that its simplicity and convenience have been found considerable by experience. The reader will perceive, by turning to the first volume of our Journal, p. 128, that the happy contrivance of the wire for lifting the lower valve is also claimed by Cuthbertson, who in his pamphlet\*, page 6, informs us that the hint of such an apparatus was first given to him by M. Paets van Troostwyk. It is not said that Dr. Rutherford carried his invention into practice. This merit is due to Mr. Cuthbertson and Sir G. M. I remember the same ingenious thought having been also stated by another philosophical gentleman, in 1783, when the air pump of Haas† was much talked of.

The air pump of Sir George Mackenzie differs in effect from that of Mr. Cuthbertson in the solidity of the piston, and in not having an oil vessel to the valve through which the air is extruded. The air pump of Sir George will cease to exhaust, supposing every thing else perfect, when the mass of air in the receiver bears the same proportion to an equal volume of external air, as the capacity of the bore of the valve-piece  $X$ , bears to the interior capacity of the barrel when the piston is up. Cuthbertson's pump will have a similar limit with relation to the communication pipe as to the upper valve (Philos. Journal, pl. 7, vol. 1, fig. 1.) and the capacity of the barrel above the piston when down. In Prince's air pump, and in the project mentioned at p. 131 of the same volume; if the

\* Description of an improved Air Pump, &c. by John Cuthbertson, London; sold by Johnson. No date, but, as I think, published seven or eight years ago.

† Phil. Transf. M, DCC, LXXIII.

valves be made to open mechanically, the maximum of exhaustion will in theory be in the duplicate ratio of the smaller space to the larger, assuming both strokes to be equal in the barrels through which the air successively passes. And in Sadler's pump with oil (*Ibid.* plate xix. fig. 1.) the maximum will be indefinitely great, because every stroke must take out a like part of the residue of air from the receiver.

## VIII.

*On the Action of Nitre upon Gold and Platina.* By SMITHSON TENNANT, Esq. F. R. S.\*

**GOLD**, which cannot be calcined by exposure to heat and air, has been also considered as incapable of being affected by nitre. But in the course of some experiments on the diamond, an account of which has been communicated to this society, I observed that when nitre was heated in a tube of gold, and the diamond was not in sufficient quantity to supply the alkali of the nitre with fixed air, a part of the gold was dissolved. From this observation I was induced to examine more particularly the action of nitre upon gold, as well as to enquire whether it would produce any effect upon silver and platina.

With this intention I put some thin pieces of gold into the tube, together with nitre, and exposed them to a strong red heat for two or three hours. After the tube was taken from the fire, the part of the nitre which remained, consisting of caustic alkali, and of nitre partially decomposed, weighed 140 grains; and 60 grains of the gold were found to have been dissolved. Upon the addition of water, about 50 grains of the gold were precipitated in the form of a black powder. The gold which was thus precipitated was principally in its metallic state, the greater portion of it being insoluble in marine acid. The remaining gold, about ten grains in weight, communicated to the alkaline solution in which it was retained, a light yellow colour. By dropping into this solution diluted vitriolic or nitrous acid, it became at first of a deeper yellow, but, if viewed by the transmitted light, it soon appeared green, and afterwards blue. The alteration of the colour from yellow to blue, arises from the gradual precipitation of the gold in its metallic form, which by the transmitted light is of a blue colour: though the gold is precipitated from this solution in its metallic form, yet there seems to be no doubt that, while it remains dissolved, it is entirely in the state of calx. Its precipitation in the metallic state is occasioned by the nitre contained in the solution, which, having lost part of its oxygen by heat, appears to be capable of attracting it from the calx of gold; for I found, that if the calx of gold is dissolved by being boiled in caustic alkali, and a sufficient quantity of nitre, which has lost some of its air by heat, is mixed with it, the gold is precipitated by an acid in its metallic state †.

Having

\* Philosophical Transactions, M,DCC,XCVII.

† As the precipitation of gold in its metallic form by nitre which has lost some of its oxygen, has not, I believe, been noticed, it may not be improper to mention some of those facts relating to it which seem most entitled to attention. Nitre, which has been heated some time, precipitates gold in its metallic state from a solution in aqua regia, if it is diluted with water. If a solution of gold in nitrous acid is dropped into pure water, the calx of gold is separated, which is of a yellow colour; but if the water contains a very small proportion of nitre, which has lost some of its air by heat (as one grain in six ounces), the gold is deprived of its oxygen and becomes blue. The alkali of the nitre does not assist in producing this effect. Nitrous acid alone, which

does

Having found that nitre would dissolve gold, I tried whether it would produce any effect upon platina.

It has been formerly observed, that the grains of platina, in the impure state in which it is originally found, might, by being long heated in a crucible with nitre, be reduced to powder. Lewis, from his own experiments and those of Margraff, thought that the iron only which is contained in the grains of platina was corroded by the nitre. But by heating nitre with some thin pieces of pure platina in a cup of the same metal, I found that the platina was easily dissolved, the cup being much corroded, and the thin pieces entirely destroyed. By dissolving the saline matter in water, the greater part of the platina was precipitated in the form of a brown powder. This powder, which was entirely soluble in marine acid, consisted of the calx of platina, combined with a portion of alkali, which could not be separated by being boiled in water. The platina which was retained by the alkaline solution communicated to it a brown yellow colour. By adding an acid to it a precipitate was formed, which consisted of the calx of platina, of alkali, and of the acid which was employed.

Silver I found to be a little corroded by nitre: but, as its action upon that metal was very inconsiderable, it did not appear to be deserving of a more particular examination.

## IX.

*An Account of certain Causes of Alteration injurious to the Quality of Corn, and the Means of preventing this Change\*. By B. G. SAGE, of the ci-devant Academy of Sciences, Professor of Chemistry and Mineralogy in the School des Mines de la Monnoie.*

IN the Analysis of Corn, which I published in 1776, I have shewn that when the corn no longer contains glutinous† or vegeto-animal matter, it affords flour not adapted to produce a good panary fermentation; that the bread is not white, and has a disagreeable taste and smell; that it produces an oppression at the stomach, putrid disorders, and the dry gangrene, like smutted rye. I did not at that time know the cause of this alteration of corn; but discovered it last year, by attending to the farming operations in the corn country of Beauce, where I have observed that the method of housing or stacking the corn was more suited to destroy than to preserve it.

In fact, the sickle has scarcely cut the corn before it is collected in sheaves to form shocks, or larger parcels, which are immediately conveyed into the barns, where they are packed

does not contain its full proportion of oxygen, occasions the same precipitation, unless it is very strong; and if a mixture of such strong nitrous acid, and of a solution of gold in nitrous acid, is dropped into water, the gold is deprived of its oxygen, and is precipitated of a blue colour. Two causes contribute to produce this effect upon the addition of water. The adhesion of the calx of gold to nitrous acid is by that means weakened, and the oxygen is attracted more strongly to the imperfect nitrous acid in consequence of their attraction for water when they are united.

\* Journal de Phys. Sep. for 1794.

† Wheat is composed of the cortical part called bran, starch, saccharine matter, and the glutinous substance. The flour obtained at the mills near Paris is composed of 1-16th part of saccharine and extractive matter, 2-3ds of white fecula called starch, and 1-4th part of elastic glutinous matter. The corn of the southern countries contain more. S.

as close as possible, without attending whether the grain and the straw be dry, as well as the more aqueous herbs which are cut along with it. The consequence is, that the corn becomes heated a few hours after it is put away, and this heat is stronger and more durable the larger and the damper the mass. The heat is frequently strong enough to bake an egg, according to the account of the cultivators. For my part, I could not hold my hand in this mass, which is as capable of spontaneous combustion as stacks of hay when put together too wet.

Fire does not manifest itself so often in our barns, because the air can scarcely at all penetrate into them, by reason of the very close stowage. When I asked the farmers why they pressed the corn so much? they affirmed, that their view was to prevent its occupying a large space, and to hinder vermin from finding their way into it.

When this heat is excited in the corn newly stowed away, a smell is emitted for three weeks resembling that of fermenting beer: it seemed at first as if aromatic herbs had been boiled in the neighbourhood.

I have observed the duration of this heat for more than four months, in a barn where the quantity stowed away amounted to a cube of about 40 feet. The corn, when taken out, was rough, ruddy, and more or less decomposed; so that in the lower part of the barn the alteration and decomposition of the glutinous matter was complete, and the grain was no longer proper for vegetation\*. The bread made with flour of this corn does not rise well, and, after baking, exhibits a yellowish grey colour.

The farmers of Beaucé have a prejudice that it is good for the grain to sweat and heat, which is contrary to sound reasoning: for in this case the heat is produced by fermentation, which cannot take place but by the decomposition and loss of some of the integral parts of the corn. It is accordingly found, that the saccharine and glutinous matter are more or less destroyed in proportion to the time which the corn has remained in the heated state.

If the sheaves were dry when housed, they would not heat, and the grain would be preserved in perfection. It is proper therefore to suffer them to dry in the field, and not pack them together until they have given out all their moisture. It will also be of advantage to lay them lightly together instead of pressing, in order that the circulation of the air may carry off the last portions of humidity.

Besides the preservation of the grain, another advantage would be obtained, namely, that the straw would be neither heated nor mouldy. Straw in this last state contracts a disagreeable smell, which is repulsive to cattle.

It is in one of the most fertile corn provinces of France, in which the ground is best cultivated, that a method so prejudicial to the grain is employed. The interests of humanity being the same as that of the cultivator, it is to be presumed that, when once instructed, he will change his practice. In fact his gain will be double: for he will constantly have wholesome corn, and will sell it at a higher price than such as has been heated.

As the state and quality of corn is to be judged from the nature and quantity of the glutinous matter, it is proper to describe the process for extracting it.

Take four ounces of wheat flour separated from the bran; mix it with water to form a paste. Let this be kneaded for a quarter of an hour, and afterwards washed by working it

\* I think it would be proper to suffer the corn to dry in the sheaf for the purpose of affording wholesome grain; for the fermentation alters, weakens, and often destroys it.



with the hands under water, which is to be changed from time to time. This washing is to be continued until the last quantity of water made use of is no longer discoloured. The substance remaining in the hands is the glutinous matter of a whitish grey colour. If the corn be good, this is elastic; (that is to say, it may be drawn into long strings, which have a disposition to shrink or contract.) If the corn has begun to heat, it is short or brittle. If it has fermented, it will afford none of this glutinous matter.

## X.

*Description of an Apparatus for disengaging Oxygen Gas, and applying it to the best Advantage.*

*Constructed by JAMES SADLER, Esq. Chemist to the Admiralty.—To which are added, Observations upon the Blow-Pipe. By W. N.*

**FIG. I.** plate 2, represents the vertical section of a furnace. The shaded parts denote brick work. A is the ash-hole, B the grate upon which the fuel is placed, C the opening for the reception of the fuel. It is covered with a piece of fire stone, or an earthen cover, at all times except when a supply of fuel is wanted. D is the aperture leading to the chimney F; and between D and F is a chamber for the reception of a matrafs or other vessel E, to be exposed to the action of the flame. The vessel E is put in its place, or taken out, by the opening at the upper part of the chamber; and when at work, that opening is closed by two pieces of fire stone, each of which covers half the aperture, and meets the other by a perfect ground edge, having a notch that leaves a space for the neck of the vessel. **Fig. II.** is a ground plan of the same furnace, with the apparatus for receiving and applying the oxygen. The letters B, E, F, denote the same parts as in fig. 1. **Fig. III.** exhibits a vertical section of the matrafs, and other apparatus, denoted by the same letters as in fig. 2. E is the matrafs containing black oxyde of manganese. H is a refrigeratory through which the tube of communication passes. I is a receiver for condensable vapour. The elastic fluid passes through the tube at K into the receiver L, inverted in another vessel of water M; the receiver being suspended by a string passing over a pulley, which therefore admits of a variation at pleasure of the reaction for extruding the air through the blow-pipe N P. The part N of the blow-pipe is of porcelain; and a lamp O is placed beneath for the purpose of heating the air before it issues from the orifice P. The letters G, K, and Q, denote cocks to be occasionally closed when the receiving apparatus is required to be separated or removed.

The advantages of this apparatus are, first, the simplicity of the furnace, which is applicable to a variety of uses, as well as that particularly stated in this account. As the current of atmospheric air from A, through the grate to B, D, and F, does not pass above the roof of the passage D; the upper part of the fire-place towards C, where the combustion cannot reach, may be considered as a repository for fuel, upon the principle of the athanor, and might, if required, be made equally capacious. This fuel, before it arrives at the place of combustion, serves also as a cover when the actual cover is taken off to supply the consumption; besides which, there is not the least possibility of deranging or disturbing the vessels on such occasions, as is too often the case in the common air furnace. The aperture at E

affords

affords a degree of convenience, equally obvious, with regard to the putting in, placing, and taking out the vessels. Of the refrigeratory, the receiver I, and the apparatus M L, little more need be said than that they are constructed in the forms which experience has shewn to be the most simple and efficacious. The parts N O P exhibit an improvement which is found to be of great importance. Mr. Sadler observed, in the course of his experiments, that the effect of the oxygen, when recently produced, was much greater than some hours afterwards. There was no reason to conclude that this difference arose from any change in the purity of the fluid: he therefore concluded that it must be caused by a difference of temperature. When the cold oxygen is brought into contact with a combustible body at a very elevated temperature, it must be concluded, that part of the caloric disengaged at the instant of combination must be employed in raising the temperature of the mass of oxygen, and consequently that the intensity of the combustion will be less. He therefore determined to supply this portion of caloric from another process of combustion, carried on near the external surface of the tube through which the oxygen is transmitted. O is the lamp for that purpose, affording a flame, which heats the tube N, and gives an elevated temperature to the oxygen before it passes out of the small aperture P. The heat excited in a piece of charcoal urged by this stream of oxygen is so great, as to fuse the purest specimens of native rock crystal, and also those of lime. The other effects are likewise proportionally greater.

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*Observations on the Common Blow-Pipe.*

CHEMISTS and mineralogists are too well acquainted with the use of this instrument, to require any long enumeration of its advantages. To behold with ease such processes as demand much labour and time in furnaces, and cannot in this last situation be easily and comfortably inspected;—to see these performed in the open air in a few seconds, with all the changes of colour, ebullition, scoriaion, and the like;—to remark the nature of the vapours which fly off, and to note the precipitation of metallic substances from their fluxes, or the effects they produce on the several kinds of glass:—these are a few of the advantages which have brought this instrument into estimation.

It is well known that the common practice of blowing with the mouth, though very ready, and requiring an instrument of inconsiderable cost, is not so advantageous as the extrusion of air by means of bellows, or other mechanical contrivances. The air exhaled from the lungs has already been deprived of part of its oxygen, and is loaded with humidity. The process of blowing, even to the most skilful, is attended with some fatigue, and requires a degree of confinement of the head and one of the hands, which considerably diminishes the power, as well as the ease of the operator. Bellows, at the price of two and three guineas the set with a few additional implements, have been contrived for this purpose. It seemed probable to me, that these instruments are larger and more costly than is requisite. To ascertain the value of this suspicion, I made the following experiments:

A blow-pipe nearly of the figure described by Bergmann, whose internal diameter was about one fifth of an inch at the smallest part, was inserted through the cork of a bottle, of which the contents were  $17\frac{1}{4}$  cubic inches. The cork was notched in such a manner that

that when the pipe was stuck in the neck of the bottle previously filled with water, the whole could be inverted without any portion running out, though, from the size of the notch, a slight agitation was sufficient to produce that event. In this situation the mouth was applied to the blow-pipe, and the air strongly blown into the bottle. The water immediately flowed out, that is to say, in eighteen seconds. The experiment was twice repeated, and the aperture of the nozzle of the blow-pipe was  $\frac{1}{20}$ th of an inch. Hence it follows, that the quantity of air emitted from the blow-pipe was not quite one cubic inch in a second. For it was  $17\frac{1}{4}$  cubic inches in eighteen seconds. It will appear also from an easy calculation, that the velocity was not quite four feet in a second.

The nozzle used in the foregoing experiment was adapted to the flame of a lamp: but it was too large for the flame of the candle called a short eight, which is of tallow, nine inches long, three qrs. inch in diameter, having sixteen yarns of cotton in its wick, and weighing  $\frac{1}{8}$ th part of a pound avoirdupois. The experiments were repeated with a nozzle well suited to the flame of this candle. The aperture of this last was rather more than  $\frac{1}{10}$ th of an inch in diameter. The bottle was emptied once in 20 seconds, and twice with uncomfortable exertion in 18 seconds each time. If the velocity of emission had been the same in both experiments, this last would have required about 36 seconds. It may therefore be inferred, that in the experiment with the largest aperture, the escape of air was so speedy as considerably to diminish the pressure by which it was driven out.

In order to ascertain the condensation of the air in the blow-pipe, a small quantity of water was put into the bottle, and the blowing continued for a few seconds beneath the thumb, which was applied to the orifice of the neck. Air was suffered to escape at the same time through the lips, so as nearly to produce the same effect with regard to the muscles of the mouth, as if the same had been emitted through a blow-pipe. When the action was at its utmost, the thumb was suddenly closed on the aperture, and the bottle inverted. In this situation the line of the upper surface of the water was carefully marked, after which the thumb was gently withdrawn, and a portion of the water flowed out in consequence of the spring of the included air. By the depression of the water, it was found that the air had been condensed by about  $\frac{1}{8}$ th part of the whole, and consequently would have sustained a little more than an inch of mercury in a simple gage. It seemed probable, however, that the exertion of blowing into a bottle by a short temporary effort, might be very different from the steady action of blowing through a pipe. To prove this more clearly, I bended a glass tube nearly of the same internal diameter as the blow-pipe into a syphon, the legs of which formed an angle of about 45 degrees of each other. Into this, mercury was poured to occupy several inches in length, and the tube was fixed so that one of its legs continued vertical, while the other was accessible to the mouth. Upon blowing into the latter orifice, it was found that, by an easy or moderate action, the mercury was sustained to the height of about  $\frac{4}{10}$ ths of an inch above its level; that when the pressure was strong, the height was about half an inch; and that it was possible, by very strong exertion, to keep the mercury at one inch: but the lips soon became tired. The mercury might indeed be urged to near two inches, but not in a way that could have been maintained for even an extremely short time in actual work.

From these facts, if we take half an inch for the medium station of the gage, the pressure

for extruding the air will answer to about a quarter of a pound avoirdupois upon each square inch of surface, which is not more than an eighth part of the pressure in the regulating belly of the blowing machines at our great foundries. I think the quantity of one cubic inch per second is quite as much and probably more than issues out of the blow-pipe in any course of experiment. A pair of bellows capable of extruding somewhat more than two cubic inches at a stroke, would consequently supply as much air as the pipe would deliver, provided the strokes succeeded each other about once in two seconds, which appears to be a convenient rate of working, and by no means too quick. In very small bellows the internal contents may be estimated at one third of the contents of a parallelopipedon, or square box capable of circumscribing the bellows when open. As a full allowance, let us suppose the contents of this imaginary box to be eight cubic inches, and its depth two inches. Its upper surface must then be equal to four superficial inches. That is to say, a pair of bellows fully sufficient to supply the blow-pipe will not require larger dimensions than three inches in length, one inch and a half in width, with a lift of one inch and a half for each stroke. The same effect may be produced by a syringe one inch in diameter, and two inches and a half long.

The reservoir for the air may either be another pair of bellows rather larger, and disposed to shut by a weight or a spring; or it may be simply a vessel of sufficient capacity to receive the air from the bellows, and emit it in a constant stream by virtue of its spring under condensation, in the same manner as water is emitted from the air vessel of a fire-engine. The emission of air from such a vessel may, in a loose way, be taken to be at half the velocity of its introduction; and, consequently, at the end of every stroke the vessel will contain half as much more air, than the medium state of condensation, as amounts to the whole contents of the bellows; and at the beginning of every stroke, the vessel will contain half as much less than that quantity. From these considerations, it appears that the steady stream from a fire-engine could not be produced but by two pumps alternately acting, and that the air answers scarcely any other purpose in the small air-vessels of those engines, than that of rendering the intrusion of the non-elastic water less sudden and violent. It will also appear from calculation, (as it does from practice, in the regulating bellies of blowing engines, on which the re-action is afforded by a variable force somewhat resembling the spring of the air) that a very large vessel is required to give a practical uniformity to the emitted stream, when the action of intrusion is not constant. Hence it appears, that an air vessel cannot be used to advantage, unless the bellows to our small apparatus be made double by two fixed outer boards, with valves opening inwards, and a moveable diaphragm alternating between them, and forcing the air through valves opening into one common nozzle—or unless the syringe, if used, be made to act both ways, namely, by the returning as well as by the direct stroke. A very neat and compact apparatus for the blow-pipe might be made in this way; but it is probable that the considerations and their advantages may be more particularly applicable to the large engines at the smelting-works.

## XI.

*A short Account of the Life of PELLETIER. Read at the Public Sitting of the National Institute of France, the 15th Vendemiaire, in the Year VI. By CITIZEN LASSUS, Secretary to the Class of Natural Philosophy and Mathematics.*

IN the course of the last trimestre we have had the misfortune to lose one of our colleagues, Bertrand Pelletier, born at Bayonne in 1761. His life was confined to the short space of 36 years; but his actions have left an impression on the minds of men which time shall not efface.

It frequently happens that young men, sincerely desirous of instruction, have no means or place where they can be assisted in the development of their natural talents, no master who may point out the direct road to science, and that order and method without which the efforts of the individual too often lead him from the object of his pursuit, instead of bringing him nearer to it. This was not the case with young Pelletier. He found every advantage in his father's house, where he received the first elements of the art of which he was afterwards the ornament; and his subsequent progress was made under our colleague Darcet, who having remarked in him that sagacity which may be called the instinct of science, admitted him among the pupils attached to the chemical laboratory of the college of France. Five years of constant application and study under such a master, who was himself formed by nature, perfected by experience, and affectionately disposed towards his pupil, afforded this young man a stock of knowledge very unusual at his age. He soon gave a convincing proof of this, by publishing, at the age of 21, a set of very excellent observations on the arsenical acid. Macquer, by mixing nitre with the oxyde of arsenic, had discovered in the residue of this operation a salt soluble in water, susceptible of crystallization in tetrahedral prisms, which he denominated the neutral arsenical salt. It is the arseniate of potash. He was of opinion that no acid could decompose it; but Pelletier showed that the sulphuric acid distilled from it does disengage the acid of arsenic. He showed the true cause why the neutral arsenical salt is not decomposable in closed vessels, and particularly the order of affinity by which the salt itself is formed in the distillation of the nitrate of potash, and the white oxyde of arsenic. He explains in what respects this salt differs from what Macquer called the liver of arsenic. Pelletier had been anticipated in this work by Scheele, by Bergmann, by the academicians of Dijon, and by our colleague Berthollet; but he possessed at least the merit, in the first essay of his powers, of having clearly developed all the phenomena of this operation, by retaining and even determining the quantity of gas it was capable of affording. After the same principles it was that he decomposed the arsenico-ammoniacal salt, by showing how, in the decomposition of this salt, the pure arsenical acid is obtained in the form of a deliquescent glass. In this work we may observe the sagacity with which he was enabled to develop all the phenomena of these compositions and decompositions, by tracing those delicate threads of scientific connection which connect the series of facts, and are imperceptible to ordinary minds.

Encouraged by the success of these first works, which he presented with the sensibility of grateful attachment to his instructor, he communicated his observations on the crystallization of sulphur, cinnabar, and the deliquescent salts; the examination of zeolites, particularly the false zeolite of Fribourg in Brisgaw, which he found to be merely an ore of zinc;

observations on the dephlogisticated or oxygenated muriatic acid, relative to the absorption of oxygen; on the formation of ethers, particularly the muriatic and the acetous; and several memoirs on the operation of phosphorus made in the large way, its conversion into phosphoric acid, and its combination with sulphur and most metallic substances.

It was by his operations on that most astonishing production of chemistry, phosphorus, that he burned himself so dangerously as nearly to have lost his life. After the cure of his wound, which confined him to his bed for six months, he immediately began the analysis of the various plumbagos of France, England, Germany, Spain, and America, and found means to give novelty and interest to his work even after the publication of Scheele on the same object. The analysis of the carbonate of barytes led him to make experiments on animals, which prove that this earth is a true poison, whether it be administered in the form of the native carbonate of barytes, or whether it be taken from the decomposition of the sulphate, even though again combined with another acid.

Chemists have given the name of Strontian to a newly-discovered earth, from the name of the place where it was first found. Pelletier analysed it, and discovered it in the sulphate of barytes. He likewise analysed the verditer of England, of which painters and paper-hangers make so much use. He discovered a process for preparing it in the large way, by treating with lime the precipitate obtained from the decomposition of nitrate of copper by lime. By his process, verditer is afforded equal in beauty to that which comes from England. He was likewise one of the first chemists who showed the possibility of refining bell metal, and separating the tin. His first experiments were made at Paris; after which he repaired to the foundry at Romilly to verify them in the large way. The following year he was received a member of the Academy of Sciences at Paris, and shortly afterwards went to La Fere, with our colleague Borda and General Daboville, to assist in experiments upon a new gunpowder. Being obliged, in order to render his experiments more decisive, to pass great part of the day in the open air during a cold and humid season, his health, which was naturally delicate, became considerably impaired. He began to recover his health, when he again became the victim of his zeal for the science he so successfully cultivated. He had nearly perished by respiring the oxygenated muriatic acid gas. A violent attack of convulsive asthma, which returned during several days, was the first consequence of this unhappy accident. The disorder then seemed to abate, but it was incurable. The assistance of art was insufficient to save him, and he died of a pulmonary consumption in the flower of his age.

Such was the man whose premature loss we now lament. His attachment for the science to which he had devoted himself, remained during the whole of his existence, and in the last moments of his life it formed an interesting object of his conversation. He possessed that activity of mind so necessary to the research of truths which are inaccessible to men of cold and languid sentiments. As a man of science, his reputation is bright and unblemished. As a citizen, his private virtues, his probity and good conduct will long continue objects of regret.

## XII.

*Extracts from the Système du Monde of M. LA PLACE\*.*

THIS work of La Place explains the leading points of the system of the world. It is as it were an abstract of a large work, in which this profound geometer proposes to treat the same objects by the principles of the most sublime geometry, and which he promises soon to publish. I shall copy some of the results of this author.

"The degree measured at the Cape of Good Hope in  $37^{\circ}$  south latitude †, is found to be 307999,8 feet, which is very nearly the same as the degree of France, under the parallel of  $50^{\circ}$ , and greater than that which was measured in Pennsylvania, at the latitude of  $43^{\circ} 56'$ , of which the length is no more than 307195,2 feet. The degree of the Cape is also greater than the degree measured in Italy in the latitude of  $47^{\circ} 80'$ , which was found to be 307680,6 feet. Nevertheless it ought to be smaller than every one of these degrees, if the earth were a regular solid formed by the revolution of a meridian perfectly alike on each side of the equator. Every fact leads us to conclude that this is not the case." Vol. I. page 105.

He concludes, that the terrestrial meridian is a line of double curvature.

"Terrestrial bodies situated under the equator describe, by virtue of the rotation in each second of time, an arc of  $40'' 1395$  of the circumference of the terrestrial equator. The radii of this equator being 19634778 feet very nearly, the versed sine of this arc is 0,0389704 feet. Gravitation causes bodies to fall at the equator through a space of 11,23585 feet in one second. The central force necessary to retain bodies at the surface of the earth, and consequently the centrifugal force arising from its rotatory motion, is to gravity at the equator in the proportion of 1 to 288,3. The centrifugal force diminishes the weight, and bodies do not fall at the equator but by virtue of the difference of the true force. If we therefore use the word *gravity* to denote the total weight which would take place, exclusively of the diminution it undergoes, the centrifugal force at the equator is extremely near  $\frac{1}{288,3}$ th part of gravity. If the rotation of the earth were 17 times more rapid, the arc described in a second at the equator would be 17 times greater, and its versed sine would be 289 times more considerable. The centrifugal force would then be equal to gravity, and bodies would cease to press or weigh towards the earth at the equator." Page 263.

"To explain the double motion of rotation and progression in the earth, it is sufficient that the supposition be admitted of the primitive impulse having been given at a small distance from its centre of gravity; which distance, supposing the planet to be homogeneous, must have been nearly the  $\frac{1}{288,3}$ th part of its radius." Page 299.

"The probability is infinitely small, that the original projection of the planets, satellites, and comets, should have passed through their centres of gravity. All these bodies must therefore have a rotative motion. From a similar reason, the sun, which turns on its axis, must have received an impulse, which not having passed through its centre of gravity, car-

\* Exposition du Système du Monde, par Pierre Simon La Place, de l'Institut National de France et du Bureau des Longitudes, 2 vol. in 8vo. A Paris de l'Imprimerie du Cercle Social, Rue du Théâtre François, No. 4. — I have not the work, but translate from Dr. Lametherie, in the Journal de Physique, August 1794.

† The author divides the circle into 400 parts.



ries it through space with the planetary system, unless this motion be supposed to have been destroyed by an impulse in the opposite direction; a circumstance by no means probable.

“The impulse given to an homogeneous sphere, in a direction which does not pass through its centre, will cause it to revolve constantly round the diameter, which is perpendicular to a plane passing through its centre, and the line of direction of the impressed force. New forces acting on all its parts, and of which the result passes through its centre, will not change the parallelism of its axis of rotation. Thus it is that the axis of the earth remains always nearly parallel to itself in its revolution round the sun, without its being necessary to suppose, with Copernicus, an annual motion of the poles of the earth round those of the ecliptic.

“If the body possesses a certain figure, its axis of rotation may change every instant. The determination of these changes, whatever may be the forces acting on the bodies, is one of the most interesting problems of mechanics respecting hard bodies, on account of its connection with the precession of the equinoxes, and the libration of the moon. The solution of this problem has led to a curious and very useful result; namely, that in all bodies there exist three axes perpendicular to each other, round which the body may turn uniformly when not solicited by external forces. On this account these axes have been called principal axes of rotation.

“A body or system of bodies, possessing weight, and of any figure whatever, oscillating round a fixed and horizontal axis, forms a compound pendulum. No other pendulum exists in nature. The simple pendulums so frequently treated of are pure geometrical conceptions, proper to simplify the objects of discussion. It is easy to refer to these such compound pendulums as have their parts immoveably fixed together. If the length of the simple pendulum, whose oscillations are isochronous with those of the compound pendulum, be multiplied by its total mass, and by the distance of its centre of gravity from the axis of oscillation, the product will be equal to the sum of the products of each particle of the compound pendulum, multiplied by the square of its distance from the axis. It is by means of this rule, discovered by Huyghens, that experiments with compound pendulums have been applied to shew the length of the simple pendulum, which beats seconds.”

The author enters into a considerable detail respecting the atmospheres of the planets. “In all the changes to which the atmosphere is subject (says he, vol. ii. p. 128.) the sum of the products of the particles of the revolving body and its atmosphere, multiplied respectively by the areas they describe round the common centre of gravity, the radii being projected on the plane of the equator, remain the same in equal times. Supposing, therefore, that, by any cause whatever, the atmosphere should become contracted, or that part thereof should become condensed on the surface of the body, the rotatory motion of the body and its atmosphere would be accelerated: for, the radii vectores of the areas described by the particles of the original atmosphere becoming smaller, the sum of the products of all the particles, by their corresponding areas, cannot remain the same unless the velocity be augmented.

“The atmosphere is flattened towards the poles, and swelled out at the equator. But this oblateness has its limits; and in the case where it is greatest, the ratio of the polar and equatorial diameter is as two to three.

“The



“The atmosphere cannot extend itself at the equator to a greater distance than to the place where the centrifugal force is exactly equal to the force of gravity. With regard to the sun, this point is remote from its centre to a distance measuring the radius of the orbit of a planet which would make its revolution in the same period as that luminary employs in its rotation. *The solar atmosphere cannot therefore extend to the orbit of Mercury*; and consequently it cannot produce the zodiacal light, which appears to extend even beyond the orbit of the earth.

“The point where the centrifugal force balances that of gravitation is nearer the body the more rapid its rotation. If we conceive the atmosphere to extend as far as this limit, and afterwards to contract and condense by cooling at the surface of the body, the motion of rotation will become more and more rapid, and the extreme limit will continually approach towards the centre. The atmosphere will therefore successively in the place of the equator abandon zones of fluid, which will continue to circulate round the body, because their centrifugal force is equal to their gravity. But as this equality does not obtain with regard to the parts of the atmosphere distant from the equator, they will not cease to appertain to the planet. It is probable that the rings of Saturn are similar zones abandoned by its atmosphere.” (Vol. ii. p. 125.)

“As the motions of the planets and their satellites are performed nearly in the same plane, we must suppose one cause to have acted on all these bodies; and from the prodigious distances between them, it must have been a fluid of immense extent. To have given them in the same direction a motion, nearly circular, about the sun, it is necessary that the fluid must have surrounded that star as an atmosphere. The consideration of the planetary movements lead us therefore to think, that by virtue of an excessive heat the atmosphere of the sun was originally extended beyond the orbits of all the planets, and that it gradually contracted in process of time to its present limits. These effects may have taken place by causes similar to that which occasioned the strong light for several months in the famous star which in the year 1572 appeared all at once in the constellation of Cassiopeia.

“The great eccentricity of the orbits of the comets leads to the same result. It evidently indicates the disposition of a great number of less eccentric orbits; a circumstance which supposes an atmosphere round the sun extending beyond the perihelia of the observable comets, which, by destroying the motions of those which happened to pass within it during the time of its greatest extent, united them to the body of the sun. Hence it follows, that no other comets can at present be in existence, but such as were at that time beyond that interval. And as we cannot observe any comets but those which come near the sun in their perihelium, their orbits must be very eccentric. It also follows, that their inclinations must offer the same irregularities as if these bodies had been projected casually; because the solar atmosphere has not influenced their motions. The long time employed by the comets in their revolutions, the great eccentricity of their orbits, and the variety of their inclinations, are therefore very naturally explained by means of this atmosphere.

“But in what manner have the movements of revolution and rotation of the planets been effected? If these bodies had penetrated as such into the atmosphere of the sun, its resistance must have caused them to fall to its surface. We may therefore conjecture that they were formed at the successive limits of that atmosphere, by the condensation of the zones which it must have abandoned in the plane of its equator during its cooling and condensation at

the surface of that star. We may also conjecture, that the satellites have been formed in a like manner by the atmospheres of the planets. The five phenomena abovementioned naturally flow from these hypotheses, to which the rings of Saturn afford additional probability." (Vol. ii. p. 301.)

The five phenomena enumerated by the author are : (1) The motions of the planets in the same direction and nearly in the same plane. (2) The motions of the satellites in the same direction as the rotations of their planets. (3) The rotations of these different bodies and of the sun in the same direction as their projectile motion, and in planes very little differing from each other ; and lastly, (4) The eccentricity of the cometary orbits.

This great eccentricity of the comets, and their motions in all directions, appear to the author a sufficient reason to conclude, that their origin is different from that of the planets. He afterwards enquires into the probability that a comet may strike the globe of the earth, and destroy its present regularity of appearance, &c.

These are his words, (vol. ii. p. 60.) " The fears which the appearance of comets at that time inspired, were succeeded by an apprehension of another nature ; lest, among the great number which traverse the planetary system in every direction, one of them should destroy the earth. *They pass so rapidly near us, that the effect of their attraction is not to be feared.* It is only by actually striking the earth that they could produce the dreadful effect : but the shock, though possible, is so very improbable in the course of an age ; it would require so extraordinary a chance for the concurrence of two bodies so small with respect to the immensity of the space in which they move, that no reasonable ground of fear can be maintained in this behalf. Nevertheless, the small probability of such an event, if it be considered with respect to a long series of ages, may become very great. It is easy to imagine the effects of such a shock upon the earth. The axis and rotatory motion being changed, the seas abandon their former position, and rush to the new equator ; great part of the men and animals drowned in this universal deluge, or destroyed by the violent stroke impressed on the terrestrial globe ; entire species annihilated ; all the monuments of human industry swept away :—such are the disasters which might ensue from the shock of a comet. We see therefore why the ocean has formerly covered the high mountains, on which it has left indubitable marks of its presence ; how the plants and animals of the south may have existed in the climates of the north, where their remains and impressions appear ; and lastly, by an event of this kind, we may explain the novelty of the moral world ; the regular processes of which can scarcely be traced beyond three thousand years. The human species reduced to a very small number of individuals, and to the most deplorable state, entirely occupied for a long series of time in the care of its own preservation, must have totally lost the remembrance of the sciences and the arts ; and when the progress of civilization gave efficacy to wants of less immediate pressure, it became necessary to repeat again the various gradations of invention, as if men had then for the first time been placed on the earth. But however adequate the cause may be to these phenomena, for which it is assigned by some philosophers, I repeat, that we may be perfectly at our ease with regard to so terrible an event during the short interval of life individual."

## XIII.

*On the Preparation of concrete Acid of Lemons. By DIZÉ, Apothecary in Chief to the French Army, charged with the Inspection of the General Magazine of Medicines \*.*

**T**HOUGH nature presents us with the citric acid nearly in a disengaged state, it is nevertheless confounded in lemon juice with an extractive mucilaginous matter, which opposes the union of its crystallizable particles, and which cannot be separated by the simple process of evaporation and exposure to crystallize.

Scheele was the first who obtained this acid in the solid form. In the year 1774, Georgius in Sweden, and afterwards Du Buiffon in France, published observations on the method of concentrating and preserving lemon juice. Although their labours, as well as the experiment of Stahl and Guyton, could not determine the crystallization of this acid, it is nevertheless certain that their researches were very useful to assist Scheele in his discovery of the process he has left us.

I availed myself of an opportunity to repeat the process of Scheele in the large way, and to insist on an essential observation, which that chemist has not sufficiently developed; for it is one of the necessary conditions for success in the crystallization of this vegetable acid. Scheele, after several unfavourable attempts, advises the separation of the extractive and mucilaginous matter of lemon juice, by uniting the citric acid to the basis of calcareous carbonate, with which it forms a citrate of lime, that precipitates on account of its sparing solubility, while the extractive and mucous matter remains dissolved in the fluid. This citrate of lime is afterwards decomposed by a sufficient quantity of sulphuric acid diluted with water, with the precaution to add an excess of this last acid. The sulphuric acid seizes the lime from the citric acid, and forms a sulphate, which falls to the bottom because nearly insoluble; while the citric acid, being set at liberty, is dissolved in the water with which the sulphuric acid was diluted.

Filtration and washing with cold water separate the citric acid entirely from the sulphate of lime, and it may be afterwards obtained in the concrete state by evaporation in stone-ware vessels, at the temperature of boiling water. I have observed that it is very useful to suspend the evaporation every two days, in order to permit the sulphate of lime, which is suspended by the assistance of the citric acid, to fall down.

The citric acid which I have prepared is the product of several chests of lemons. The mass of calcareous citrate which was decomposed was somewhat considerable, and the operation was performed in large stone-ware vessels. The sulphate of lime obtained by the decomposition of the calcareous citrate was well washed in tubs of white wood. The different liquors were united together for evaporation in stone-ware vessels, at the temperature of boiling water. They were clear, light-yellow, and contained an excess of sulphuric acid.

As soon as the liquors were sufficiently concentrated by evaporation, the sulphuric acid exerted its action, and the yellow colour became brown, and even blackish at the end of

\* Read to the National Institute of France, and copied in the *Journal de Physique*, published to supply the defect of the number for September 1794.

the evaporation. The mass of crystals which was taken out, after cooling and three days repose, was considerable and black.

I suffered this saline mass to drain in osier baskets, while the evaporation and crystallization of the remaining fluid were performed. When this first operation was ended, the whole of the saline mass was re-dissolved in a sufficient quantity of cold water, and filtered through strainers of linen, covered with filtering paper.

The solution which passed the filter was clear, but of a dirty brown, and the greatest quantity of the matter which had blackened the crystals remained behind. This was again evaporated, left to crystallize, and in this manner exhausted of the citric acid. The crystals were now yellow, and more regularly figured. By a third solution, filtration, and evaporation, the crystals were again obtained white, regular, and of the greatest purity. The black matter deposited on the filter was so trifling that it formed a very slight covering.

Scheele, who was content with having proved the possibility of obtaining this vegetable acid in a concrete state, could not determine its natural form, because he operated on such small quantities.

My results afforded crystals as large as are usually obtained from the ordinary processes of saline solution, and as easily to be described. They present, on simple inspection, rhomboidal prisms, the sides of which are inclined to each other in angles of about 120 and 60 degrees, terminated at each end by four trapezoidal faces which include the solid angles.

I have before remarked, that Scheele had observed, as one of the conditions essential to the ready crystallization of the citric acid, to add a small quantity of sulphuric acid in excess beyond the exact quantity necessary to decompose the calcareous citrate. The remark of this chemist not being followed by any explanation, my trials in the large way have proved the great discernment of the chemist of Göttingen; and that, if he had operated on a quantity of lemon juice equal to that which I purified, he would have ascertained the reasons why this excess of acid was found to be necessary in his more confined experiments.

Having ascertained that the black matter remaining on the filters in this process is charcoal, it follows that it could not have been afforded but at the expence of a considerable portion of the mucilaginous matter of the lemon juice, which the citric acid had carried with it in its combination with the base of the calcareous carbonate; and that afterwards when the citrate is decomposed, the excess of sulphuric acid is required to decompose this mucilage, and precipitate the carbone, as soon as the fluid begins to be condensed by evaporation. The ingenious experiments of Fourcroy and Vauquelin prove the nature of the action of sulphuric acid on vegetable matter, and strongly confirm my inductions\*.

Lemon

\* For these important experiments see *Philos. Journal* I. 385.—In order to shew that mucilage enters into the citrate of lime, and that it is decomposed by sulphuric acid, it seems necessary to make the experiment with only a very minute excess of the latter. Scheele thought the acid (*Essays*, p. 362.) in the citrate to be pure, and he required an excess of sulphuric acid to be added to insure the saturation of the whole of the lime. I would propose to the consideration of the learned author of this paper, to ascertain whether it be not a portion of the acid itself which is altered and made to deposit carbone. If so, the less the surplus of sulphuric acid the better:

Lemon juice separated from all the mucous matter which falls down by exposure to the contact of the air for a few hours, that is to say, such as it ought to be for use in medicine or the arts, marks five degrees of density by the areometer for salts of Baumé (specific gravity 1.034). One hundred pounds of this juice require for saturation six pounds four ounces of calcareous carbonate. The citrate of lime, after being well washed and dried, weighs twenty pounds.

One pound of pure crystallized acid of lemons dissolved in a sufficient quantity of water, demands one pound of carbonate of lime for its saturation. In this experiment the weight of calcareous citrate has diminished the mass rather more than one-fourth part, instead of increasing it upwards of two-thirds, as in the foregoing combination. Whence it may be concluded, that one hundred pounds of lemon juice faithfully prepared, and of the strength of five degrees by Baumé's areometer for salts, contain six pounds four ounces of pure concrete acid. This serves to explain two phenomena; that is to say, the increase of 13lbs. 2oz. in the calcareous citrate from lemon juice, and the presence of mucilaginous matter dissolved in that liquid, which enters into the combination of the calcareous citrate, and is afterwards decomposed by the excess of sulphuric acid necessary to be added when that citrate is decomposed.

One ounce of distilled water dissolves an ounce and two drams of citric acid; and produces 13 degrees (R.) of cold by the solution. A like quantity of distilled water dissolves twice its weight of this acid, when it is heated to 80 degrees, or the boiling temperature. One hundred parts of citric acid dissolved in a sufficient quantity of distilled water, boiling hot, dissolve 50 parts of calcareous citrate.

A lemonade of the most agreeable taste and appearance may be had by dissolving 40 grains of citric acid in a pint of water, with the addition of a sufficient quantity of pure sugar. It may be rendered fragrant by dissolving a small quantity of oleo-saccharum, prepared by rubbing a lemon on a lump of sugar. The sugar imbibes the volatile oil of the lemon, and renders it soluble. It is easy by this means to preserve the whole of the volatile oil of a number of lemons. The oleo-saccharum thus obtained may be mixed in a mortar with a sufficient quantity of sugar. The mixture is then to be dried by a gentle heat, and preserved in well closed glass vessels. This method of procuring the flavour of lemons at all times is preferable to employing the volatile oil obtained by distillation. The action of fire communicates to this last an acrid flavour, easily distinguished by a delicate taste.

I shall finish my observations by enumerating some of the characters of this acid, when mixed with different earthy and metallic solutions.

Solutions of the acetites of magnesia, lime, alumine, of the muriates of barytes, lime, alumine, and magnesia, and of the nitrates and sulphates of these same substances, do not undergo any change by the presence of the citric acid.

The muriates and nitrates of zinc, the sulphate, muriate, nitrate, and acetite of copper,

but, on the other hand, if his inference be correct, the surplus must be a definite quantity, namely, sufficient to destroy the mucilage. The difference of weight in the citrates formed by the crude and the purified acids with equal doses of lime, as mentioned in a subsequent paragraph, may arise either from the presence of mucilage in the former acid, or a change of affinity for lime produced by the re-action of the excess of sulphuric acid on the latter: but experiment must determine which. M.

and

and the nitrate of lead, are not decomposed, but the acetite of lead is immediately decomposed and precipitated in a white powder. The nitrate and acetite of mercury are also decomposed, and the mercurial citrate which falls down is a flaky salt, of a brick-dust colour, more or less red. The citric acid gives a green tinge to the solution of the acetite and sulphate of iron.

The quantity of citric acid which I obtained being considerable, I intend hereafter to give an account of the phenomena it exhibits in its different combinations.

#### XIV.

*Useful Notices respecting various Objects.—Governor for regulating the Motions of Steam-Engines.—Amelioration of Oil.*

##### 1. Governor for regulating the Motions of Steam-Engines.

THE apparatus mentioned page 424 of the present work by the name of a Governor, is there supposed to be the invention of Mr. Watt. My supposition was grounded upon no other fact than that of having seen it in his engines. It was invented by — Bunce, Esq. of the Admiralty, who applied it to a crane several years ago, the construction of which was communicated to the Society of Arts, soon after which period it was adopted in steam-engines. I cannot now refer to the volume, as I do not possess the set. But I had the information from the inventor himself.

##### 2. Amelioration of Oil.

EVERY one who has occasion to use lamps must be sensible that the colour of the light, as well as the quantity and kind of disagreeable vapour emitted from the flame, depends greatly on the quality of the oil. When oil is kept in an open vessel, it gradually becomes more and more oxygenated, and at the same time less fluid. For both these reasons it is less fit for use. It is less combustible, and less adapted to pass between the fibres of the wick. These observations point out the expediency of keeping oil in well-closed vessels. The fluidity of whale-oil, and the facility of its combustion, may be considerably augmented by an addition of cold-drawn linseed oil.

It is well known that oil may be rendered purer by agitation with water, more particularly with the addition of an acid. The effect of this process is stated to be, that it carries off a portion of mucilage, which is not adapted to answer the purposes to which oil is applied. It may easily be imagined, however, that oil thus treated will retain a portion of aqueous or saline matter, which may render it unfit to be applied to the moving parts of instruments for the purpose of diminishing friction. Some clock and watch makers expose olive-oil to the atmosphere in frosty weather; and select that portion which they find to continue fluid after a considerable part is frozen. This proceeding is grounded on the supposition, that the oil may consist of two different fluids, one of which is supposed to congeal in a less heat than the other; and that this congelation is the principal evil which happens to the oil in time-pieces. It does not seem probable that either of these suppositions are well founded.

For

For the whole of the oil will freeze, if time be allowed ; and the thickening of this fluid appears to be produced by chemical change, and not by mere cooling. Mathematical instrument-makers, directed, as I imagine, by experience only, find that oil is greatly improved by exposure to light, which it is asserted causes it to deposit mucilage. A very exquisite regulator having the dead-beat escapement of Graham, which requires oil on the pallets, was found to go much more steadily when this oil was used, instead of the oil commonly applied to such instruments.

Most of the facts here stated respecting oil were communicated to me by an intelligent cultivator of the sciences, whose name I forbear to add because I neglected to ask permission for that purpose.

### NEW PUBLICATION.

Reports of the late Mr. John Smeaton, F. R. S. made on various Occasions in the Course of his Employment as an Engineer. Printed for a Select Committee of Civil Engineers. Sold by Faden in London. Quarto. 412 pages, rather closely printed, with a Portrait of the Author, and 2 plates.

**T**HAT Smeaton was a man of strong natural powers and great industry, that his experience and observations were extensive, and his success highly to the credit of himself and his country, are too well known to be insisted upon. This first volume of his Reports is published at the expence of Sir J. Banks, Capt. J. Huddart, Wm. Jeffop, Robert Milne, and John Rennie, Esqrs. conditionally that the profits should be given to Mr. Smeaton's representatives. If this liberal effort, by which the nation is benefited by so valuable a mass of practical information, should be attended with success, another volume will appear, containing the remainder of the Professional Reports of this great engineer. I shall take the earliest opportunity of communicating the general principles used and adopted by Smeaton, in a paper I intend to draw up respecting the relative value and effect of first movers, and other elementary objects of daily use to engineers ; and in the mean time shall avail myself of the Preface, nearly verbatim, to these Reports, to give some account of the Society of Civil Engineers.

The origin of the Society of Civil Engineers took its rise from the following circumstances :—Before or about the year 1760, a new æra in all the arts and sciences, learned and polite, commenced in this country. Every thing which contributes to the comfort, the beauty, and the prosperity of a country, moved forward in improvement so rapidly and so obviously as to mark that period with particular distinction.

The learned Societies extended their views, their labours, and their objects of research. The professors of the polite arts associated together for the first time ; and they now enjoy a protection favourable to improvement, and not less honourable to real merit than to the Public and the Throne, which have with one accord promoted their prosperity.

Nor have these exertions failed of producing the adequate effects, comparing the present with the past state of things.

Military and naval establishments were made or enlarged, to promote and extend the true knowledge on which these sciences depend.



The navy of England fails now uncontrouled in every part of the habitable world, and her ships of war defy the combined power of all other maritime nations.

It was about the same period that manufactures were extended on a new plan, by the enterprise, the capital, and, above all, by the science of men of deep knowledge and persevering industry engaged in them.

It was perceived that it would be better for establishments to set down on new situations, best suited for raw materials and the labour of patient and retired industry, than to be plagued with the miserable little politics of corporate towns, and the wages of their extravagant workmen.

This produced a new demand, not thought of, till then, in this country—internal navigation. To make communications from factory to factory, and from warehouses to harbours, as well as to carry raw materials to and from such establishments, became absolutely necessary. Hence arose those wonderful works, not of pompous and useless magnificence, but of real utility, which are at this time carrying on to a degree of extent and magnitude to which as yet there is no appearance of limitation.

The ancient harbours of this island, it may be said, have ever been neglected, considering the increase of its naval power, and a foreign commerce of which there has never been an example in the history of mankind. The sea-ports were (I had almost said are) such as Nature formed, and Providence has bestowed upon us; and they were but little better previous to that period, notwithstanding some jettées and piers of defence ill-placed had been made and repeatedly altered, without knowledge and judgment, at *municipal*, not *government's* expence.

This general situation of things gave rise to a new profession and order of men, called Civil Engineers.

In all the polished nations of Europe, this was and is a profession of itself. Academies, or some parts of such institutions, were appropriated to the study of it, and of all the preparatory sciences and accomplishments necessary to form an able artist, whose profession comprehends the variety of objects on which he is employed, and of which the present work is an example, and a proof.

In this country, however, the formation of such artists has been left to chance; and persons leaned towards the public call of employments in this way, as their natural turn of mind took a bias. There were no public establishments, except common schools for the rudimental knowledge necessary to all arts, naval, military, mechanical, and others.

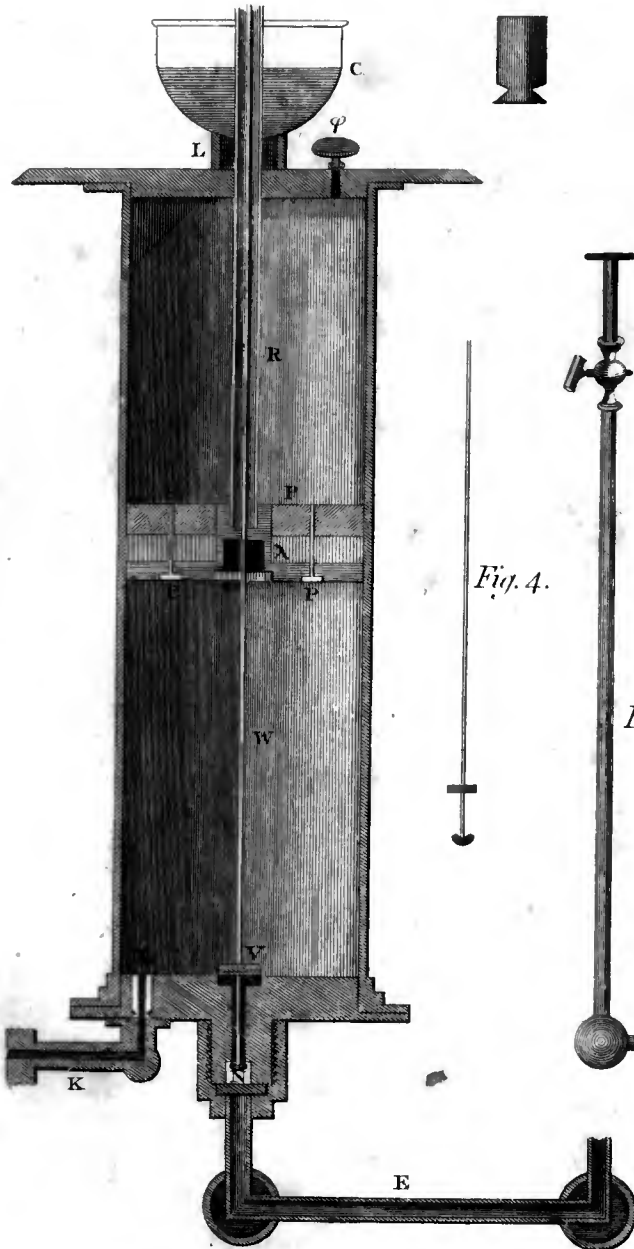
Civil Engineers are a self-created set of men, whose profession owes its origin not to power or influence, but to the best of all protection, the encouragement of a great and powerful nation; a nation become so from the industry and steadiness of its manufacturing workmen, and their superior knowledge in practical chemistry, mechanics, natural philosophy, and other useful accomplishments.

When any one who has read the varied particulars of this publication, shuts and lays it down for contemplation, he will reflect on the natural talents and sagacity requisite in that mind which applies to such a profession; on the patient application necessary to acquire all the subservient learning previous to the commencement of it; and on the wonderful and varied powers which this work exhibits.

*[To be concluded in our next.]*



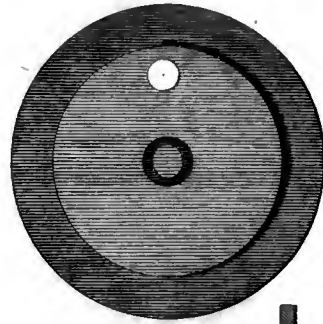
*Fig. 1.*



*Fig. 5.*



*Fig. 2.*



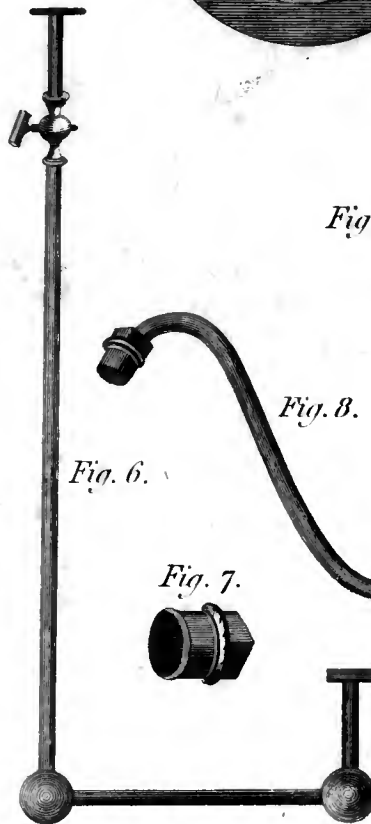
*Fig. 3.*



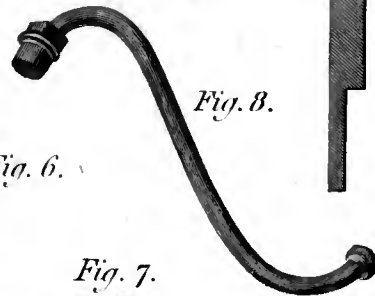
*Fig. 4.*



*Fig. 6.*



*Fig. 8.*



*Fig. 7.*





Fig. 1.

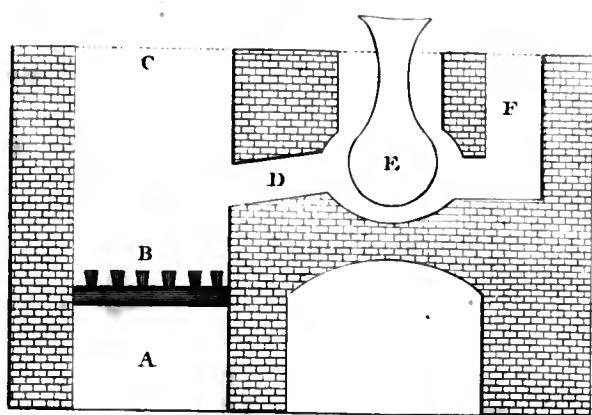


Fig. 2.

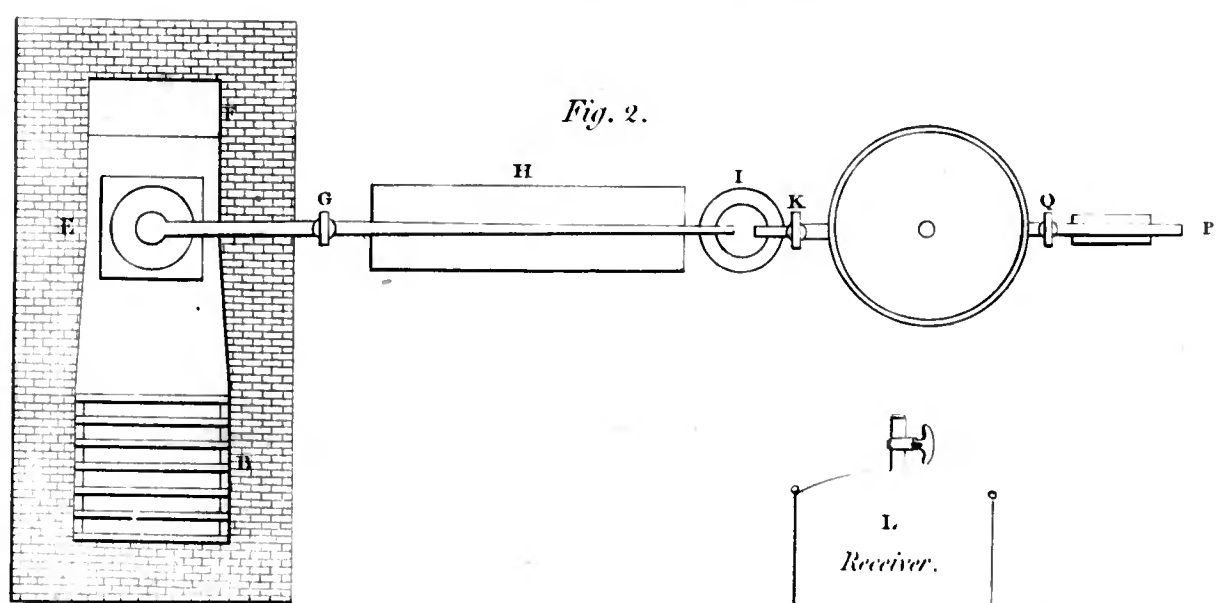
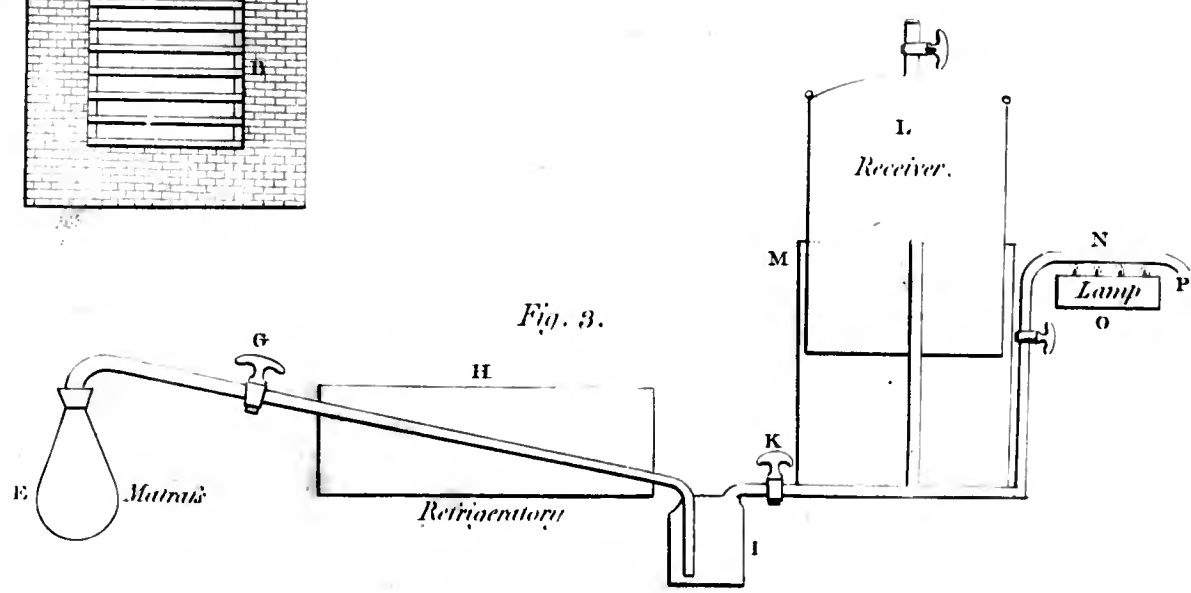


Fig. 3.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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MAY 1798.

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ARTICLE I.

*On the maintaining Power in Clocks and Watches.*

[Concluded from Page 429, Vol. I.]

**I**F we suppose the pendulum of a clock to preserve its length invariable \* during all the changes of temperature to which it is exposed, its vibrations will measure equal portions of time so long as the arc of vibration continues the same. The resistance of the air and of friction are prevented, by the maintaining power, from destroying the vibration. This power may be applied in different parts of the arc; and may, according to circumstances, render the time of vibration either shorter or longer, or it may simply maintain it without altering the time. Whether it alter the whole time or not will be of no consequence to the performance of the regulated machine, provided the effect of the maintaining power is itself subject to no variation. For the thing required is, that all the vibrations, when once brought to an accurate adjustment, should continue invariable. It is therefore essential to a good time-piece, either, 1. that the maintaining power itself shall not vary; or, 2. that it should be applied to the pendulum or balance in such a manner, as that in all variations of the power itself the time of vibration may continue the same; or, 3. that it shall act during a very small part only of the time of vibration, in order that the effect of its variations may be less.

\* Philosophical Journal, I. page 58.

When the train of wheels in a clock is driven by a weight, the more immediate cause of variation consists in the periodical irregularities of the wheels, the pinions, and the pivots, which transmit the force diminished by friction and oblique actions, which are by no means uniform: the remoter cause of variation consists in the oil becoming less fluid the longer it is exposed to the air. But when a train is driven by a main spring, the causes of irregularity are greatly increased by the diminution of force in the spring as it runs down, and its friction in the box; neither of which are so regular as to admit of a good correction from the spiral of the fusee round which the chain, that acts upon the movement, is drawn. Perfect equality of the maintaining power is not therefore to be expected. The only good expedient, independent of the escapement, appears to be that of causing the train to go by a weight or spring, serving only to wind up the last wheel once during every revolution, this wheel being urged by an appropriate weight or spring. By such an expedient the clock in effect becomes a time-piece with one wheel only, in which the irregularities are minute; and their recurrence too frequent to produce any considerable effect on the vibrating part.

When the irregularity of the first mover is proposed to be remedied by its mode of application, it is clear that it must give an increase of velocity to such vibrations as are performed through longer arcs, in order that the times may continue the same. Attempts have been made to accomplish this object, by giving a proper figure to the face of the pallets of the escapement; but it does not appear that such attempts have been successful.

The third method of equalizing the effects of the maintaining power with regard to time is founded on the consideration, that, if this impulse could be given in an instant, the whole of the vibration would be free, and resemble that of a disengaged pendulum or balance; and if the forces, by which the vibrating body returns to its point of quiescence, were as the spaces to be moved through, all the vibrations would be performed in equal times, whether long or short, or whether the maintaining power were greater or less. This disposition is peculiarly adapted to the balance. For it is possible to form the pendulum spring and adapt the maintaining power to so small a part of the vibration, that the times shall not perceptibly vary, even when the power itself is greatly altered.

Under the latter points of view the escapement becomes an object of great importance, since on this the manner of application of the maintaining power must depend. It scarcely seems necessary to subdivide or arrange the principles on which the most eminent artists have endeavoured to give perfection to their escapements. The principles will perhaps best shew themselves in the form of remarks upon the several constructions:

One of the most ancient escapements is that which is at present applied in common pocket watches. It is represented Fig. 1. Pl. III. and is best suited to the long vibrations of the balance, which was invented earlier than the pendulum\*. AB denotes the rim of

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\* Doctor Derham on Clockwork, p. 86. fourth edition, affirms, that vibrations are no where mentioned or alluded to in the clocks described by the ancients: yet we learn from Vitruvius that they possessed time-pieces with wheel-work. Falconet, *Academie des Inscriptions*, vol. xx. thinks that all the mechanical time-pieces made before the 16th century were clepsydræ with wheel-work for reckoning the smaller portions of descent.

a contrate wheel called the crown wheel, having its teeth pointed and sloped on one side only, so that the points advance before any other part of the teeth during the motion. *CD* are two pallets, or flaps, proceeding downwards from the verge *EF*. The pallets are nearly at right angles to each other; and when the balance *FG* fixed to the verge is at rest, the pallets remain inclined to the plane of the wheel in an angle of about 45 degrees; but when it is made to vibrate, one of the pallets is brought nearer to the perpendicular position, while the other becomes more nearly parallel. The wheel must be supposed to have one of its teeth resting against a pallet by virtue of the maintaining power. This tooth will slip off or escape as the pallet rises towards the horizontal position, at which instant a tooth on the opposite side of the wheel will strike against the other pallet which is down. The returning vibration, by raising this last pallet, will suffer that tooth to escape, and another tooth will apply itself to the first-mentioned pallet. By this alternation the crown wheel will advance the quantity of half a tooth each vibration, and the balance or pendulum will be prevented from coming to rest, because the impulse of the teeth against the pallets will be equal to the resistances from friction and the re-action of the air.

On this escapement it may be remarked, that the pendulum or balance is constantly connected with, and influenced by, the maintaining power, except during the exceedingly small time of the drop of the wheel from one pallet to the other; on which account the measure of time will greatly vary, when the force of vibration is merely equal, or not much greater than the maintaining impulse. This is shewn in a striking manner by urging the movement of a common watch by means of the key. If the key be pressed in the usual direction of winding up, the beats of the vibration will become very slow, or even stop; and if the pressure be made in the opposite direction, the vibrations will become very loud and quick.

descent. Professor Venturi, in his *Essai sur les Ouvrages Physico-Mathématiques de Léonard de Vinci*, page 28, has a curious note on this subject, from which I here make some extracts. The common escapement described in the text was well known to de Vinci, who describes an instrument acting by an escapement of this kind, similar, as he says, to the verge of the balance in watches, which he does not seem to mention as a new thing. He died about 1513. The isochronism of the pendulum was known to Galileo in 1600, who before his death, namely about 1633, proposed to apply it to clocks. The actual application by Huyghens was made before 1658, when he published his *Horologium oscillatorium*. He applied it by means of the common escapement already in use with the balance, and still retained in our table clocks. Sanctorius had made the same application near forty years before that time, as appears by his *Commentarii in Avicennam*, (*quest.* 56) printed in 1625, in which several instruments are described as having been publicly exhibited and explained to his auditors at his lectures in Padua for thirteen years previous to that time.

There is a manuscript (No. 7295) in the National Library of France, written about the middle of the 15th century by H. Arnault, Physician to the Dukes of Burgundy, who died in 1465. This author describes a planisphere which Jean de Fondeur (*Fuoris*) his master had constructed for the Duke, and which Arnault himself had afterwards repaired. Whence it appears that the instrument was constructed about the commencement of the 15th century. At folio 59 a design is seen of the watch which gives motion to the planisphere. It is nearly the modern watch. The balance is called *circulus affixus virge paletorum, qui cum ea de vi movetur*. He calls the crown wheel, which forms the common escapement on the pallets of the verge of the balance, by the name of *folietus*. At folio 60 the striking part is described, where we have the terms *voletus*, the *brochie levantes malleum*; *crux media inter movimentum et sonneriam*; *cavale* and *chivola* for *cheville*. M. Venturi thinks that the watches of Wallingford and Dondi, in the 14th century, were constructed on the principle of the balance. Robert Hooke added the spring to the balance in the year 1658.

This escapement not being adapted to such vibrations as are performed through arcs of a few degrees only, another construction has been made which has been in constant use for about a century in clocks, with a long pendulum beating seconds. Fig. 2. A B represents a vertical wheel called the swing wheel, having thirty teeth. C D represents a pair of pallets connected together, and moveable in conjunction with the pendulum on the centre or axis F. One tooth of the wheel in the present position rests on the inclined surface of the inner part of the pallet C, upon which its disposition to slide tends to throw the point of the pallet farther from the centre of the wheel, and consequently assists the vibration in that direction. While the pallet C moves outwards and the wheel advances, the point of the pallet D of course approaches towards the centre in the opening between the two nearest teeth; and when the acting tooth of the wheel slips off, or escapes from the pallet C, another tooth on the opposite side immediately falls on the exterior inclined face of D, and by a similar operation tends to push that pallet from the centre. The returning vibration is thus assisted by the wheel, while the pallet C moves towards the centre, and receives the succeeding tooth of the wheel after the escape from the point of D. In this manner the alternation may be conceived to go on without limit.

The same observation which was made upon the escapement fig. 1. is applicable also to this; namely, that the vibrating part is constantly under the influence of the maintaining power, except during the interval of the drop, or actual escape of the wheel from one pallet to the other. The indirect action of an inclined plane, with the influence of oil which it necessarily requires, may also be mentioned to shew that this escapement is not equal to what might have been produced by a skilful variation of the other. One principal recommendation of this escapement seems to have been the facility with which it affords an index for seconds in the face of the clock. Though the pendulum, according to this construction, is constantly connected with the maintrailing power in a clock, yet the variations of that power have not the same mischievous effect as in a watch, because the momentum of the pendulum, compared with the impulse of the maintaining power, is prodigiously greater in the former of these instruments. A very considerable change in the maintaining power of a clock with a long pendulum, will not cause a greater variation than a few seconds in the daily rate.

The celebrated George Graham improved this escapement very much by taking off part of the slope farthest from the points of the pallets; instead of which part he formed a circular or cylindrical face, having its axis in the centre of motion. Pallets of this kind are seen on the opposite side of the wheel at E and G, having H for their centre or axis. A tooth of the wheel is seen resting upon the circular inner surface of the pallet G, which is not therefore affected by the wheel, excepting so far as its motion arising from any other cause may be affected by the friction of the tooth. If the vibration of the pendulum be supposed to carry G outwards, the slope surface will be brought to the point of the tooth, which will slide along it and urge the pallet outwards during this sliding action. When the tooth has fallen from the point of this pallet, an opposite tooth will be received on the circular surface of E, and will not affect the vibration, excepting when the slope surface of E is carried out so as to suffer the tooth to slide along it. In the two former escapements there is always a certain portion of vibration takes place after the drop which drives the pallets back, and causes the index also to recede through a small arc. This has been distinguished



distinguished by the name of a recoil. Other considerable objections, besides that of the continued action of the maintaining power, have been made against escapements with a recoil; but it would lead us too far into the minute departments of this subject to discuss them. The escapement of Graham, and all such as have no recoil, have been called dead beat escapements, because the index for seconds falls directly through its arc, and remains motionless on the line of division till the next vibration. It may be observed, that the maintaining power in Graham's escapement may be applied during a small portion only of the vibration; and that an increase of the maintaining power tends to enlarge the arc of vibration, but scarcely interferes with its velocity.

The effect of the escapement which has been called horizontal \*, because the last wheel in watches of this construction has its plane parallel to the rest of the system, is similar to that of the dead beat escapement of Graham. In fig. 3, the horizontal wheel is seen with twelve teeth, upon each of which is fixed a small wedge supported above the plane of the wheel, as may be seen at the letters A and B. On the verge of the balance there is fixed part of a hollow cylinder of steel or other hard material, the imaginary axis of which passes through the pivots of the verge. C represents this cylindrical piece, into which the wedge D may be supposed to have fallen. While the vibration causes the cylindrical piece to revolve in the direction which carries its anterior edge towards the axis of the wheel, the point of the wedge will merely rub the internal surface, and no otherwise affect the vibration of the balance than by retarding its motion. But when the return of the vibration clears the cylinder of the point of the wedge D, the wheel will advance, and the slope surface of the wedge acting against the edge of the cylinder will assist the vibration of the balance. When the edge of the cylinder arrives at the outer point of the wedge D, its posterior edge must arrive at the position denoted by the dotted lines of continuation; immediately after which the wedge or tooth E will arrive at the position e, and rest on the outer surface of the cylinder, where it will produce no other effect than that of retardation from friction, as was remarked with regard to the wedge D, until the course of the vibration shall bring the posterior edge of the cylinder clear of the point of the wedge. In this last situation, the wedge will act on the edge of the cylinder, and assist the vibration, as in the former case, until that edge shall arrive at the outer or posterior point of the wedge; immediately after which the leading point will fall on the inner surface of the cylinder in the first position, as was shewn in the wedge D.

\* In the seventh volume of *Machines et Inventions approuvées par l'Académie Royale des Sciences*, I find, p. 137, that the Sieur Bausfré, a French clock-maker, settled in London, contrived the horizontal escapement in 1704. He applied an hollow portion of a cylinder to the verge of the balance, the edges of which were thick enough to afford the inclined surfaces of Graham's pallets, and these were acted upon by two common wheels perfectly similar and fixed on the same axis, so that the teeth of one corresponded with the hollows of the other. In the same work, p. 141, the *Règle artificielle du temps* of Sully, p. 248—251, is quoted to shew that Sir Isaac Newton possessed one of these watches, marked with the name of Bausfré. Peter le Roy improved this escapement by substituting a portion of a cone instead of the cylinder (*Machines et Inventions*, 1742), with a double wheel, the axis of which was at right angles to the verge. And M. Gourdain, in the same year (p. 141 of the work last quoted), made another construction, in which a flat circular piece of metal, out of which a portion of about 90° was cut, was substituted in the place of the cylinder, and received the action of a wheel with wedge-teeth, acting alternately in opposite directions in the notch, and resting on the flat surface of the metal during the time of repose. The escapement described in the text appears to have been a combination of the inventions of Bausfré and Gourdain.

Horizontal watches were greatly esteemed during the last thirty years, until lately, when they gave place to those constructions which are known by the name of detached or free escapements. In the common escapement, fig. 1, an increase of the maintaining power increases the recoil, and accelerates the vibrations; but with the horizontal escapement there is no recoil; and an increase of the maintaining power, though it may enlarge the arc of vibration, will not necessarily diminish or alter the time. It is accordingly found, that the experiment of altering the maintaining power by the application of the key, does not alter the rate in the same perceptible manner as in common watches. Exceedingly perfect time-pieces on the horizontal principle, with a compensation for temperature applied to the spring, were made about fifteen years ago by Emery, of Charing-cross, and other artists; but I am informed that their performance became sensibly less accurate after a few years wear. Whether the difficulty of execution, or the badness of workmanship in low-priced watches of this kind, or whether the subsequent great improvements in this useful engine, have caused the horizontal principle to be laid aside, I know not; but I believe few if any watches of this construction are at present made.

The dead beat escapement of Graham, and the horizontal escapement, both appear to be improvements of the common anchor escapement, fig. 1. which by these expedients is deprived of its recoil, but still retains the wedge action, which has much friction, and necessarily requires oil. The quantity of direct push in the escapement fig. 1, which requires no oil on the face of the pallets, appears to be the cause why it acts so freely, and is so little liable to injury or disorder. I wish it were less difficult to ascertain the inventors and improvers of useful contrivances. But in proportion to the utility and universal adoption of any instrument, it always becomes more easy to mention it by a single term. Definitions, descriptions, and inventors' names naturally become discarded; and from this cause it seems as if a moderate or a speculative invention might carry the name of its inventor to remote ages, while the great benefactors of society are forgotten. I do not know who first undertook to improve escapements by pallets resembling those of fig. 1. in the direct action they receive; but the earliest contrivance of this kind I have met with is that of M. Le Roy fils aîné, of which the account bears date Sept. 6, 1748. As it is in effect the same escapement as is now in the highest esteem in the time-pieces of our best artists, I shall here describe the latter, and mention the particulars of that of M. Le Roy in a note.

Fig. 3. Pl. III. represents the free escapement of our best portable time-pieces. Fig. 4. exhibits the escapement on a larger scale. On the verge of the balance is fixed a circular piece of sapphire, or hard steel, EL, out of which a sectoral piece is cut. HG is a straight spring fixed near its extremity H, and having at the other extremity a pin G, against which one of the teeth of the wheel D rests when the train is at rest. This spring has a slight tendency towards the centre of the wheel, but is prevented by the stop K from throwing the pin farther inwards than just to receive the point of the tooth. I is a very slender spring fixed at the end I, and pressing very slightly against the pin G, in a direction tending to throw it from the wheel D, but which on account of the greater power of HG it cannot effect. It may be observed that the spring I proceeds a little beyond the pin G.—F is a lever proceeding from the verge of the balance directly opposite the end of the spring I, and long enough to strike it in its vibration. The action is as follows:—From

the pressure of the main spring the wheel (fig. 4) is urged from D towards F, but is prevented from moving by the pin G. Let the balance be made to vibrate, and the lever F will move through the arc Ff, strike the inner extremity of the spring I, and displace the pin G. At this instant the face E, which may be called the pallet, will have arrived at the position e, against which the tooth of the wheel will fall, and communicate its impulse through about  $15^{\circ}$  or  $16^{\circ}$  of the vibration. But F quits the spring I sooner than the wheel quits the pallet E, and consequently the pin G will have returned to its first station before the wheel can have advanced a whole tooth, and the spring or detent HG will receive the wheel as before, immediately after its escape from the pallet. The returning vibration of the balance will be made with the piece EL perfectly at liberty between two teeth of the wheel, as in the sketch, and the back stroke of the lever F against the tender spring I, will have no effect whatever on the pin G; this spring being like the back spring of the jacks of the harpsichord, active in one direction only. The third vibration of the balance will unlock the detent as before; the impulse will again be given, and the whole process will be repeated; and in this manner, the balance, though it may vibrate through the greatest part of the entire circle, will be entirely free of the works, except during the very small time of the drop of the wheel\*.

It is hardly necessary to make any remark on this escapement. It requires little or no oil; and when all the parts, particularly the pendulum spring, are duly adjusted, it is found that a very great variation in the first mover will remarkably alter the arc of vibration without affecting the rate. The piece EL might have consisted of a single pallet or arm, instead of a portion of a circle or cylinder; but such a piece would have been rather less convenient to make in sapphire, or ruby, as in the best time-pieces, and would also have been less useful. For if by any accident or shock the pin G should be displaced for an instant, the wheel D will not run down, because it will be caught upon the circular surface of EL.

\* The *échappement à détente* of Le Roy (*Recueil des Machines*, &c. VII. 385) was different from the above described in several particulars not essential to the general principle. 1. The wheel was contrate, and in the drawing has 44 thin teeth. 2. The face E was afforded by an actual pallet concave towards the tooth. 3. The detent was forked, and moved on pivots at the angular point. One leg presented a stop to one of the teeth of the wheel, while the other leg rested against the periphery of a semi-cylinder upon the verge of the balance. The detent was kept to this position by a tender back spring. 4. In the actual performance the vibration of the balance carried the semi-cylinder clear of the leg of the detent, which consequently would have quitted the wheel, if the back spring had been strong enough. Immediately after the arrival at this position, the pallet struck a tooth of the wheel, made it recoil, and delivered the detent, which flew inwards against the radius of the semi-cylinder. The wheel being thus set at liberty, was driven back through an angle of near  $20^{\circ}$ , as I estimate from the drawing, and then in its return followed the balance through about  $60^{\circ}$  or  $70^{\circ}$ , during which period the semi-cylinder recovered the position requisite to throw the detent in the way of the succeeding tooth, and retain the wheel, while the balance continued its vibration, disengaged from every action but that of the rub of the semi-cylinder against the tail of the detent. The judicious mechanic will perceive that the disposition of the parts in the escapement fig. 4. are much for the better. For (a) the flexures of springs afford a less variable deduction from the moving force, than the frictions of surfaces in the pivots of Le Roy's detent, and his semi-cylinder. (b) The force lost in the recoil is greater and more variable than that employed against the spring I in fig. 4. (c) The balance is perfectly disengaged for a much larger portion of both vibrations in the modern, than in the original escapement, and (d) the small number of teeth in the wheel D affords an advantage in regulating the figure and contacts of the parts.

It

It is indeed very easy to observe, that the piece EL would operate without the detent, though with much friction during the time of repose. The tooth of the wheel would in that case rest upon its circular face.

This escapement is better calculated for a long than a short vibration. I have seen it adapted to an half seconds pendulum clock, in the form delineated in fig. 6, Pl. III. It kept up a vibration of  $8^{\circ}$  with a very heavy pendulum, and no great maintaining weight, but I was not informed of the quantities of these weights. The part ABDC is fixed to the pendulum rod. BC is a lever moveable on the joint B, but not capable of falling below its horizontal position. It terminates in a claw at C. At D is a pallet of agate or hard stone, having a flat polished face. FGHI is a detent moveable on the centre H, but kept in its position against the pin or stop K, by means of the counterpoise G. As it is now represented, a tooth I of the wheel bears against the end of the lower arm or branch. Suppose the pendulum to be made to vibrate, and it will pass clear of the teeth IL; but when the claw C strikes against the upper end F of the detent, the slope surface of those extremities will cause the lever BC to rise, and the claw in its return will draw F towards the wheel, and consequently disengage I. The tooth E will therefore fall on the pallet D, and impell the pendulum in its vibration; but before it quits the pallet the claw C will be drawn away from F, and the counterweight G will restore the original position of the wheel, which will receive and hold the tooth M during the whole of the vibration made in the direction BC. The next vibration in the opposite direction will displace the detent as before, and by these alternations the motions will continue so long as the maintaining power continues to act.

In the two last escapements we have seen the variable effects of the maintaining power almost entirely removed, as far as can be practically discerned. Fig. 7.\* exhibits the escapement of Mudge, in which the balance is perfectly detached from the train of wheels, except during the extremely short interval of striking out the parts which serve the purpose of detents. ONEBQ is the circumference of the balance, vibrating by the action of a spiral spring as usual on its axis CA DH passing through the centre C: the axis is bended into a crank, AXYD, to make room for the other work. LM, ZW, are two rods fixed to the crank at the points L and Z, parallel to XY. c d e f r s are fixed parts of the machine. TR is an axis concentric with that of the balance, and carrying an arm Go nearly at right angles to it, and a small auxiliary spring u, which is wound up whenever the arm Go is moved in the direction o h. p is a curved pallet fixed to the axis TR, which receives the tooth of the balance wheel near the axis. The tooth, proceeding along the curved surface, by the force of the main spring turns the axis and its arm Go, and winds up the spring u. A small projection at the extremity of the curved surface of the pallet p prevents the farther progress of the tooth, when the arm o G has been turned through an arc o h, of about  $27^{\circ}$ ; and, consequently, the spring u has been wound up through the same angle or arc, o Gh =  $27^{\circ}$ .—FS is another axis exactly similar to TR. It carries its arm Io, and spring v, and the tooth of the balance wheel Im winds up the

\* From Airwood's Investigations for determining the Times of Vibration of Watch Balances. *Philos. Trans.* 1794.

spring *v*, by acting on the pallet *q*, and is detained by a projection, after having carried it through an angle of  $27^{\circ}$ , exactly as in the former case. The arcs passed through by the arms *G o* and *I o*, and marked in the figure, are also denoted by the same letters on the rim of the balance.

The effect of this escapement may be thus explained: Let the balance be in the quiescent state, the main spring being unwound, and the branch or crank in the position represented in the figure. If the quiescent points of the auxiliary springs coincide with that of the balance spring, the arm *G o* will just touch the rod *LM*, and in like manner the arm *I o* will just touch the rod *W Z*; the two arms *G o* and *I o* in this position are parallel to the line *CO*. This position of the balance and auxiliary springs remains as long as the main spring of the machine continues unwound; but whenever the action of the main spring sets the balance wheel in motion, a tooth thereof meeting with one or other of the pallets *p* or *q*, will wind up one of the auxiliary springs; suppose it should be the spring *u*. The arm *G o* being carried into the position *G h*, by the force of the balance wheel acting on the pallet *p*, remains in that position as long as the tooth of the balance wheel continues locked by the projection at the extremity of the pallet *p*; and the balance itself not being at all affected by the motion of the arm *G o*, nor by the winding up of the spring *u*, remains in its quiescent position: consequently no vibration can take place, except by the assistance of some external force to set the balance in motion. Suppose an impulse to be given sufficient to carry it through the semi-arc *OB*, which is about  $135^{\circ}$  in Mr. Mudge's construction.

The balance, during this motion, carries with it the crank *AXYD*, and the affixed rods *LM*, *ZW*. When the balance has described an angle of about  $27^{\circ}$  = the angle *oCh*, or *oGh*, the rod *LM* meets with the arm *GH*, and by turning the axis *TR*, and the pallet *p* in the direction of the arc *oh*, releases the tooth of the balance wheel from the projection at the extremity of the pallet *p*: the balance wheel immediately revolves, and the lower tooth meeting with the pallet *q*, winds up the auxiliary spring *v*, and carries the arm *I o* with a circular motion through the angle *oIk*, about  $27^{\circ}$ , in which position the arm *I o* remains as long as the tooth of the balance wheel is locked by the pallet *q*. While the spring *v* is winding up through the arc *ok*, the balance describes the remaining part of the semi-arc *hB*, and during this motion the rod *LM* carries round the arm *Gh*, causing it to describe an angle *hCB*, or *hGB*, which is measured by the arc *hB* =  $108^{\circ}$ . When the balance has arrived at the extremity of the semi-arc *OB* =  $135^{\circ}$ , the auxiliary spring *u* will have been wound up through the same angle of  $135^{\circ}$ , that is to say,  $27^{\circ}$ , by the force of the main spring acting on the pallet *p*, and  $108^{\circ}$  by the balance itself, carrying along with it the arm *G o*, or *Gh*, while it describes the arc *hB*. The balance therefore returns through the arc *BO*, by the joint action of the balance spring and the auxiliary spring *u*; the acceleration of both springs ceasing the instant the balance arrives at the quiescent point *o*. When the balance has proceeded in its vibration about  $27^{\circ}$  beyond the point *O*, to the position *Ck*, the rod *ZW* meets with the arm *Ik*, and by carrying it forward releases the tooth of the balance wheel from the pallet *q*. The balance wheel accordingly revolves, and the upper tooth meeting with the pallet *p* winds up the auxiliary spring *u*, as before. The balance with the crank proceeding to describe

the remaining semi-arc  $kE$ , winds up the spring  $v$  through the further angle  $kCE = 108^\circ$ , and returns through the semi-arc  $Eo$ , by the joint action of the balance spring and the auxiliary spring  $v$ , both of which cease to accelerate the balance the instant it has arrived at  $O$ .

It may be remarked, in this curious escapement, that the motion of the balance in its semi-vibration from the point of quiescence is opposed through an arc of no more than  $108$ , but is accelerated in its return through the whole arc of  $135^\circ$ , and that the difference is what maintains the vibrations; and moreover, that the force from the wheel being exerted to wind up each auxiliary spring during the time it is totally disengaged from the balance, this last organ cannot be affected by its irregularities, except so far as they may render it more difficult to disengage the rim of the pallet from the tooth. The balance describes an arc of about  $8^\circ$  during this disengagement.

From a passage in a pamphlet of Mr. Mudge, printed in 1763, which I have seen, and is quoted by Count Bruhl\*, it is clearly shewn that that artist had the notion of the principle of this escapement in his mind at least thirty-five years ago. From the same author's description it appears that Mudge had organized his notions in the present form before August 1771, and completed his first time-keeper two years afterwards, viz. in 1773. The performance of such time-keepers as have been made on this construction has proved excellent, but not superior, as I am informed, to such as have been made with the escapement No. 4. There are, however, many circumstances in the execution of the workmanship of engines of this delicate nature, which must affect their performance whatever may be their principle. The theory of Mudge's escapement appears to be more perfect than that of the other. But whether its six pivots in the time-measurer, which require oil and adjustments for position when intended for the pocket, and the care required in settling the points of quiescence in the three distinct springs, may render it less easy to be carried into practical effect, will be in a great measure an object of opinion, into which I cannot now enter.

If Mr. Mudge was the first who imagined an escapement absolutely detached, I think it is to Mr. Alexander Cumming that we are indebted for the first execution of a like contrivance of his own. This well-known artist, in the year 1763, made a clock for the king, in which there was no friction during the repose of the movement, and the maintaining power was applied to the inclined faces of a pair of pallets†; and some time previous to 1766, he improved the same so as to render it as perfectly detached as, in the nature of things, an escapement can be. As this has been so many years in possession of the public, and is, as I think, simplified in the sketch, fig. 8,—I shall only say that it consists of an anchor like that of Graham, but having a detent or claw at each end instead of pallets, and two separate pallets pressed (by a lever and weight to each) towards the centre of a wheel like  $AB$ , fig. 2; that these pallets are severally raised during the absence of the pendulum, which displaces the detent, by virtue of which, when the pendulum becomes joined with either, the maintaining force is constant and greater in the descent than in the ascent. It seems reasonable to infer, that Mr. Mudge must have paid great attention to the escape-

\* On the investigation of astronomical circles; (page 3, of register) a pamphlet of 32 pages octavo, printed in London 1794.

† Cumming's Elements of Clock and Watch Work, p. 78.

ment of Cumming during his labours for organizing his own, which he did about five years afterwards.

Fig. 8. is the sketch of an adaptation of Mudge's escapement to a clock. LM is part of the periphery of the wheel. GA, GB, are two arms separately moveable on the same axis, and terminating in the pallets A, B. These pallets have inclined faces, with a claw or detent at the lower part of each. GO, IO are tails proceeding from each pallet-piece respectively, and the dark spot at N represents a pin proceeding from the pendulum rod, and capable of moving either of the tails according to the course of the vibration. The dotted circles u and v represent weights which are stuck upon two pins, and may be changed for others, greater or smaller, until the most suitable quantity is found. Suppose the wheel to be urged from L towards M, and the pendulum made to vibrate by external impulse. The pin N proceeding towards L will strike the tail GO, raise the pallet A, and set the wheel at liberty; which sliding along the inner surface of the pallet B, will raise it, and stop against the claw at its lower end. IO will consequently be carried into the position IP; and the pallet A in its return will be opposite a vacancy, which will permit the tail GO to follow the pin N as far as the perpendicular situation. The pendulum will therefore be assisted by the weight u through a longer arc in its descent, than it was impeded by it in its ascent. In the opposite semi-vibration towards M, the pendulum will proceed unopposed by v, while it passes through the angle OIP, when it will raise B, and permit the wheel to elevate the pallet A. In the motion on this side of the perpendicular, it is also clear that the descent will be more assisted than the ascent was impeded. Whence it follows that the clock will continue to go: and no variation of the force of the wheel LM, which raises the pallets in the absence of the pendulum, will affect the vibration, except so far as it may afford a variable resistance at the detent or claw.

From observing that the detached escapements require a very strong first mover\*, and that pendulum clocks measure time with great accuracy by means of Graham's dead beat escapement, fig. 2, notwithstanding the influence of oil and friction during the repose of the movement, I was induced to contrive an escapement which should be as free as the crown wheel escapement, but have no recoil or action during the time of rest, nor any detent, nor require oil on its pallets. I made a drawing at the beginning of the year 1784, which I showed to various mechanics at that time. It has since been adapted to a clock in my possession, and has been going very freely for a year past. Fig. 9 is a sketch in which GH represents a steel wheel; D and E are pallets of agate, with flat polished faces. The pallet D is fixed to the lever DC, which is confined to its present situation by the loaded branch or arm CB resting on the stop k. The lever EC is also kept in its situation

\* A pocket time-piece by Brockbank, making 5 vibrations, or  $2\frac{1}{2}$  beats per second, the radius of whose balance was 0.35 inch, and arc described by the external surface of the expansion pieces 0.45 inch radius, was maintained by a force which, on trial at the axis of the fusee, proved equal to  $9\frac{1}{2}$  oz. troy, passing through 12 inches in 24 hours. Another, of the same dimensions and construction, was driven by  $11\frac{1}{2}$  oz. with the same daily fall. And a large box time-keeper of Arnold, beating half-seconds, with a balance of two radii, each one inch long, was maintained by a like force of 113 ounces. This is nearly as much as is required to afford a semi-vibration of  $3\frac{1}{2}^{\circ}$  in a clock, with a seconds pendulum of 8lb. by the common eight-day train with Graham's escapement (fig. 2.), but three times as much as is required for a well made regulator. A common watch with a balance 0.8 inch diameter, was found to have a maintaining power of  $5\frac{1}{2}$  oz. falling daily through 12 inches.

by the loaded arm CA resting upon I. A pin N proceeds from the pendulum rod to its situation between the levers. The centre of motion of the pendulum is in the continuation of the axis of the pallets. Suppose the vibration to be begun, the pin N in its progress will lift the pallet D towards L, while part of the weight B will be supported by the tooth of the wheel which will follow, and at last escape, and suffer the opposite tooth F to fall on the pallet E. But in its return or descent the pendulum will be acted on by the whole weight B, and consequently its vibration will be kept up. When the pin N raises the pallet E, a similar effect will take place on the side of the perpendicular towards M, and thus the process will go on as long as the wheel GH retains any force.

The ancient escapement with the crown wheel, verge, and pallets, has continued in use for three centuries, because of the advantage of an almost direct push against a retiring surface, which enables it to go without oil. In the escapement last described, the impulse being more direct than on those common pallets, it goes more freely without oil; and instead of the reaction at the ends of the vibrations, the pendulum is perfectly disengaged during the whole time of repose.

From this general view of the principal escapements, together with that of the compensations for temperature before explained\*, the philosophical reader may form some estimate of the cautions requisite to form the valuable instrument by which a measure of time is afforded. I hope it scarcely need be added, that I have endeavoured to do justice in the historical narration, and shall be happy to rectify any mistakes, if better information should detect them.

## II.

*Copy of a Letter from Professor WILSON, of Glasgow, on the Art of multiplying Copies of engraved Plates and Stamps in relief†.*

SIR,

*Glasgow College, 23d March, 1798.*

I DOUBT not but you will have the goodness readily to excuse the liberty which I take upon the present occasion of addressing you by a printed letter. I have been induced to do so, the more conveniently to communicate to you some account of what possibly may be thought entitled to the appellation of a NEW ART, and which, for particular reasons, it has been thought adviseable to make more or less generally known, even at this very early stage of it.

Some years ago, upon the discovery of the singular property of the fluor acid in corroding glass, when it was so common to hand about bits of glass-plate frosted over by this chemical agent, it happened to strike me that we were indulging too long in a barren admiration of mere novelty, and overlooking a matter of real importance to which it evidently pointed. The general effect of what then so much amused us, when the plates

\* Philof. Journal, I. p. 56.

† I first saw this letter at Sir Joseph Banks's; but having some doubt as to the propriety of requesting it of him for publication, I sent to Mr. Tassie, who favoured me with another copy for that express purpose. N.



were viewed by transmitted light, suggested the possibility of formally etching with delicacy and perfection upon glass. This thought no sooner occurred than it challenged some attention in consequence of perceiving that glass, from its extreme hardness and lubricity, would preserve the execution bestowed upon it vastly longer than copper-plate, were it possible to introduce its services at the rolling press. But whilst harbouring these surmises, the extreme fragility of glass-plate, contrasted with the violent pressure it must endure between the rollers, had well nigh made me relinquish them altogether. It often happens, however, that things, which at the first glance seem very unattainable, yield to a little reflexion, when they are fairly set before the mind in the light of valuable desiderata.

In the present case it soon became very evident, that the thinnest and frailest piece of glass-plate, whether straight or uneven, could, in regard to the strongest pressure, either general or topical, be rendered as robust and as rigid as iron or steel. The expedient which occurred to me for that purpose, consisted in nothing but joining the glass-plate to flat pieces of these metals, of considerable thickness, by an intervening lamina of proper cement, taking care that it affords a very complete contact.

Agreeable to this it is well known, that so far back as the year 1791, by fortifying glass-plates of a moderate size in this way, and aiding them further by a simple apparatus on the press board, I succeeded completely in making them pass safely between the rollers, and in obtaining many impressions, such as the fluor acid by the process of etching could afford. Upon finding, however, that the best of these etchings were very paltry, and still regarding the safe passage of the glass between the rollers as an interesting experiment, I was insensibly led to consider, whether by the lapidary's wheel, or by any other possible means whatever, we could so operate upon glass-plate as to render it a productive subject at the rolling press. This step appeared of some consequence, as being all that was now wanted for establishing an improvement not destitute of importance. Not long after, certain views and methods occurred to me, which recently have been prosecuted experimentally, and which raise considerable expectations of our obtaining the mastery over an art, whereby the tame scratchings of the fluor acid would be left far behind, and the execution and spirit of the graver itself transferred from copper-plate to glass-plate, in inscriptions of all kinds, and in designs or embellishments of any sort, especially where hatching constitutes the manner; and all this with the further advantage, that any number of such glass-plates may be obtained quite alike, as being derived from the same original pattern.

The means of effecting a matter of such apparent difficulty, like most other advances in the Arts, or even in the Sciences, as soon as ever propounded, will, doubtless, be thought extremely simple and obvious. But whatever small portion of merit may be allowed in the present instance, should the means prove fully adequate to the end, it will partly consist, as will be seen immediately, in my having given an entire new direction to another ingenious art which has been long cultivated, and by none with so much genius and ability as by the justly celebrated Mr. Tassie of London.

In November last, certain circumstances recalling my attention to the present object, I was led to explain, by letter to Mr. Tassie, the method of printing at the rolling press with glass-plates, and to specify to him, at some length, the steps by which I wished him im-

mediately to proceed, in attempting for me the execution of casts or copies in glass from certain copper-plates which I sent him, and which I had got engraved here for the purpose. It afforded me much satisfaction to find that this eminent artist received my application with great candour, and that he pursued the object pointed out with promptness, about which I had applied to him in the line of his profession. Though, indeed, at the beginning some difficulties stood in the way, yet by a repeated correspondence, and transmission of different essays from his furnace, answering to patterns sent up at different times by the mail coach, Mr. Tassie soon furnished me with several casts in glass and enamel, from engraved copper-plates, which appear to justify my first conceptions, and to go some length, even thus early, to ensure the success of the plan and method, which I had brought forward.

The other day I made ready for printing, three of these plates; two of them bearing inscriptions, and the other a head in profile from Le Brun. I might have mentioned that the plates, when fully finished, have an appearance of great elegance; those especially which are made of white glass, called enamel; and it is remarkable how easily they are cleaned from the ink, in the course of the workmen's operations, though they never are heated farther than by handling them. From the three plates above mentioned, I now take the liberty of sending you proofs \*, or impressions, inclosed. So far as I know, they are the first of the kind ever produced. As the very first offerings, therefore, of a new art, they may possibly appear to you in a light more or less interesting, and as an earnest of something considerably more perfect soon to follow, when we are assisted by engravings of some elegance, and executed with more address in several respects, with a view to a destination so peculiar.

It will not escape you, that by means of a series of glass-plates, inconsiderable in point of number, deriving the engraving from the same original pattern, to succeed one another at the press, just before signs of wearing might appear, a vast many impressions, all *perfectly similar*, might be obtained, even for ages. This at once would be bestowing a new character upon the rolling press, which hitherto has been much circumscribed in this respect, by the perishable nature of engraved copper-plates, and from their having no relation to any common archetype. Much could be said of the advantages of thus commanding an ever enduring identity amongst the impressions afforded by engraved plates, even of a small size, especially in the instance of circulating Bank Paper. I confess, indeed, it was the frequent alarming forgeries upon such currency, especially that of the Bank of England, which moved me in November last to bring to experiment the present scheme, which has been for several years in my mind. Should the glass-plates be carried to a sufficient pitch of delicacy, I perceive, on several accounts, that they would afford a refuge of singular importance against all attempts of forgery; provided we were to found upon archetypes of copper-plate highly elaborated, and, besides the mere inscription, exhibiting by collateral embellishment the peculiar manner of some eminent master in the art of engraving.

\* It would have been evidently useless to have copied these plates by way of exhibition to the reader. They are not distinguishable from copper-plate prints; unless, perhaps, by some minute circumstances arising from the subsequent polishing of the glass. But this last observation must be uncertain, unless proofs from the original copper were also had for comparison. N.

Before

Before concluding, I may just mention, that should the methods followed by Mr. Tassie, of moulding with plaister of Paris and Tripoli, not afford sufficient delicacy to the plates, I am already not without other resources likely to carry us farther in arresting an art, which seems recommended by views of considerable utility. I allude here partly to a method which occurred to me more recently, of having the original engraving done upon a flat and polished surface of steel, which, by the fly press, may be communicated to a flat and polished piece of pure gold. This to serve in the furnace as a *permanent mould* for giving the impression to the glass. From some experiments I have actually made in a small way, it should seem as if this process promises well; particularly, by giving the fire surface still more delicacy, and which, consequently, would require a smaller degree of finishing, in the process for bringing it ultimately to a sufficient polish; upon which circumstance alone, now depends the certainty of our being able virtually to engrave upon glass-plate, with a very considerable degree of perfection. Should it be necessary in the end to resort to the gold, Mr. Tassie's consummate skill and address at the furnace, in the application of the glass, would doubtless contribute greatly to the success.

One recommendation amongst others of this method would be, that, from the indestructible nature of pure gold, if properly managed in applying the heat, all the plates would actually be derived from one and the same mould; whereas, by the other method, the moulds themselves, which are necessary for every cast, are derivative, though indeed from the same original pattern.

I have the honour to be, Sir,

Your most obedient servant,

PAT. WILSON.

*P. S.* I have purposely reserved, to follow in a postscript, a very short mention of another subject bearing a close analogy to the foregoing.

You may perhaps know that of late years the art of cutting designs upon box-wood has arrived at an uncommon degree of perfection, and that the celebrated Messrs. Bewicks, especially, have carried their execution in this respect to a pitch of elegance rivalling copper-plate, and which was believed to be utterly unattainable before their time.

Having often regretted that such rare specimens of art, as they have produced, were so perishable, from the frailness of the materials upon which so much genius and labour were expended, I was induced also to send to Mr. Tassie, amongst other models, some designs in box-wood, executed by Mr. Bewick, with directions to mould from them, in the view of obtaining casts or copies in glass. The returns which I received to all those patterns completely answered my expectations, as being at once as perfect as the originals.

From the success of this experiment, which also I have had long in contemplation, and from what has been established in the way of making glass safely resist any pressure, it will readily occur that an improvement of considerable magnitude has now been shown evidently to depend upon a proper co-operation of the two arts of engraving upon box-wood, or upon brass, and of moulding, with a view of obtaining such cuts or engravings in so durable a substance as glass.

P. W.

III. *Instructions*

## III.

*Instructions concerning the Manufacture of Steel, and its Uses. By VANDERMONDE, MONGE, and BERTHOLLET. Published by Order of the Committee of Public Safety\*.*

*Preliminary Observations.*

**I**RON is a combustible body. It loses its metallic properties by being burned. When iron filings are exposed to strong heat in a crucible, and frequently stirred to bring the parts successively in contact with the air, the metallic aspect disappears. It assumes a brick-dust colour, and is found to have increased in weight. Part of the air of the atmosphere has combined with it, and produced this change. This portion of the atmosphere is called oxygene.

Iron is found in this state in its ores. The process of extracting it from these ores principally consists in depriving it of oxygene.

Charcoal has the property of effecting this change. During combustion it absorbs and unites with oxygene, and it will attract the oxygene from iron when these substances are in contact at an elevated temperature. These effects of air and charcoal are very perceptible when tin is kept in fusion. A grey pellicle is soon formed on the surface, which has no metallic splendour. If this pellicle be taken off, a second is formed, and in this manner the whole of the tin may be converted into a substance resembling earth, and known by the name of dross. By exposure of the dross to heat, together with a small quantity of charcoal powder, tallow, or resin, the oxygene is absorbed by these combustible substances, and the tin becomes reduced to the metallic state.

Charcoal not only possesses the property of depriving the iron of the air which was united with it, but it is also capable of being dissolved in the iron in a strong heat, and by this solution it communicates new properties to it. It changes it into steel†.

Crude or cast iron may be considered as a metal not completely reduced, which consequently retains a portion of the basis of air, or oxygene, to which it was united in the ore; and as this reduction may be carried farther according to circumstances, the consequent variations are among the leading causes of the different properties we observe in cast iron. The white cast iron accordingly holds in solution much oxygene and little charcoal; the grey cast iron on the contrary contains more of this last substance, but is much more perfectly deprived of oxygene. A greater proportion of coal must be used in the furnace to obtain this last. The properties of these two kinds of iron depend only on this difference: the former is more brittle and fusible, but it is easy in the refining furnace to deprive it of oxygene by the action of ignited charcoal, which combines with this principle. The second has retained less oxygene, but contains much more charcoal. It is softer, and preferable for such uses as require this softness; but it is more difficult to convert it into malleable iron, because a larger portion of coal is required to be destroyed, and in this state it considerably resists combustion.

\* I have omitted a few political reflections of a temporary nature contained in this memoir.

† Other ingredients enter into steel, and are perhaps essential to it, particularly phosphorus. See the analysis of Vauquelin. *Philos. Journal*, I. 252.

Forged iron perfectly refined would consist of the metal completely reduced, and containing no foreign substance, not even charcoal. Such iron is not to be met with in the market. The best Swedish iron always contains a portion of oxygen which has escaped the operations of the furnace and the refinery, and it is always contaminated by a dose of charcoal, very small indeed, but which perhaps it is impossible totally to eradicate.

Other circumstances likewise influence the qualities of iron, particularly with respect to the fabrication of steel. This metal, according to the nature of the ores which afford it, may have the defects of being brittle when cold, or brittle when hot. These are respectively called cold-short, or red-short iron. We shall not here discuss the causes which produce these bad qualities\*; but shall only remark that such iron, or its ores, affords bad steel, and must therefore be carefully avoided.

Steel is distinguished into three kinds:—natural steel; steel of cementation; and cast steel.

*Concerning Natural Steel.*

THE steel obtained immediately from the ore by simple fusion, is called natural steel. It is likewise distinguished by the name of German steel, because it comes principally from Germany.

Whether the crude iron shall assume the nature of bar iron, or of steel, depends on circumstances; but these are not difficult to be explained, from the doctrine already laid down.

Grey crude iron is alone proper to afford steel; for which purpose it is requisite that the oxygen it contains should be separated, and the coal from which its grey colour arises should be intimately combined with it. The conversion of the crude iron into steel depends on these processes†.

Hence it follows as a leading rule, that no attempts must be made to convert the white crude iron into steel in this way, notwithstanding it may be capable of affording excellent bar iron. The first operation must be conducted in such a way as to afford grey crude iron, by adding a greater proportion of coal in the charge of the furnace.

The appearance of crude iron is often deceitful with respect to its nature. The grey crude iron in plates or small portions, suddenly cooled, has the colour of the white crude iron. But the nature of these irons may be ascertained by the simple process described at the end of this memoir.

When a proper crude iron is in readiness, it is necessary, in order to convert it into bar iron, that it should be much more exposed to the action of the air by frequent stirring, and by removing the scorix; but when steel is desired, it is less exposed, and suffered to remain covered by those scorix. In the former process the charcoal is burned by the contact of the air, and the iron is left considerably more pure; but in the second the charcoal is preserved; part of which combines with the oxygen which still remained in the iron, and

\* Cold-short iron is afforded by ores which contain a small portion of phosphoric acid, which combines with the metal in the state of phosphorus. Red-short iron, which is much less common than the other, contains arsenic; but it is probable that the other semi-metals may produce the same effect. *Note of the authors.*—Probably the volatile metals may most eminently conduce to this effect. N.

† See *Philos. Journal*, I. 328.

serves to separate it, while the other portion combines with the iron itself, and gives it the qualities of steel.

The disposition of the hearth or fire-place, and the position of the tuyer or nozzle of the bellows, are two objects which require much attention. In order to obtain iron, the fire-place must be larger than for steel, and the tuyer must be inclined so as to direct the blast to the surface of the iron. The hearth is to be filled with charcoal, and the crude iron placed thereon to the level of the upper part of the tuyer. The heat is applied moderately, and by degrees, in order that the iron may not flow, but be kept in the state of a paste. It is to be stirred occasionally with the rake, frequently brought into the direction of the blast, and the scoriæ must from time to time be taken out.

To produce steel, a bed of small or powdered charcoal is to be laid round the fire-place, which must be moistened and rammed down. Light fusible scoriæ are added; the tuyer is usually more inclined, and the fusion more hastily urged, in order that the metal may flow, and immediately sink beneath the scoriæ, which are not removed till the end of the operation.

The same processes are not every where followed: but a little attention serves to show that they are all founded on the same principles; namely, that in producing steel the coaly principle of the iron is not burned, whereas in the production of bar iron the operation is so conducted as to burn that principle. We shall proceed to mention some instances.

In Styria, where good steel is made, the cast iron is reduced into thin plates, which are fused in the refinery. The ordinary masses or loupes, which have been suffered to cool in the furnace, instead of having been drawn off, are also fused for steel, of which they have begun to assume the character during their maceration in the pot, which, with this intention, was lined with charcoal, and wherein they were covered with the scoriæ. The plates as well as these masses, which are previously divided into smaller pieces, are refined with the precautions necessary to determine the formation of steel.

A circumstance which contributes much to the goodness of the steel is, that after it has been drawn out by the hammer, the bars are thrown red-hot into water, and afterwards broken in pieces for the purpose of separating the perfect steel from that which is of the nature of iron. The hard steel is also separated from the soft. Packets or trusses of these are made up, consisting of twelve or fifteen pieces each, observing that the two outer pieces are soft steel. These trusses are welded together, and drawn out again to a small size; by which means the quality of the steel is rendered considerably more uniform. The greatest quantity of German steel, as well as that which is most esteemed, is made in Carinthia. Their processes deserve to be particularly attended to. We shall here give a short account of the particulars from Hassenfratz, who made his observations on the spot, and has communicated them to us.

The crude iron is reduced into thin plates, or leaves, when it is drawn from the smelting furnace. For this purpose a mould, or hemispherical cavity, is prepared before the furnace. It is formed of the scoriæ reduced into very fine powder, and wetted to make them adhere together.

The work is then opened with an iron bar, in order that the scoriæ may flow into the mould, and dissipate its moisture. These are in the next place taken out, and the metal itself is suffered

to flow at first in a small stream, and afterwards more speedily. The aperture is enlarged in proportion as it flows out, and at last the scoriæ fall on the iron, and cover it in the mould. The furnace is then again closed, and the blast renewed. Water being thrown on the scoriæ which occupy the upper portion of the mould, they become fixed, and in this state are removed. A second portion of water is then thrown on the naked surface of the metal, which congeals to a small depth. The thin congealed plate is taken off, and a second aspersions of water is made, which affords a third plate. In this manner the process is continued, until as much of the metal is converted into plates as can be effected during the fluidity of the mass.

At some works the iron is melted in a particular furnace from the pig, for this purpose; but this second operation is evidently wasteful both of time and fuel.

The plates are intended to be made into either iron or steel.

In the process for making bar iron, the first operation consists in roasting the plates on a hearth, upon which they are arranged; a passage being formed with bricks, in order that the wind of the bellows may be directed from one extremity to the other. They are then covered with charcoal, and urged strongly with the bellows. The plates by this roasting, which destroys the charcoal of the cast iron, begin to assume the qualities of bar iron, after which they are carried to the finery furnace. The body of this furnace is more capacious than that which is intended for steel. The iron is covered with charcoal and scoriæ, and the tuyer is inclined so that the blast strikes on the plates of metal. When the fusion is complete, the scoriæ are let out, the mass is frequently turned to expose it to the blast, and, lastly, the process being completed, the iron is conveyed to the hammer.

If the object be to form steel, the furnace made use of is more contracted and deep. It is lined with pulverized charcoal, moistened and rendered solid by beating. The plates are disposed therein, and covered with scoriæ and charcoal. The position of the tuyer is nearly horizontal, in order that the stream of air may strike the fuel, and not the metal. When the metal begins to assume the solid state, the coal is taken off, the scoriæ are suffered to flow out, and scales and fragments of steel are driven by hammering into the soft mass.

The piece is afterwards melted a second time with the same precautions as before; and when the metal is thought to be sufficiently refined, the scoriæ are drawn off, and the mass is conveyed to the hammer to divide it into several pieces, which are to be separately forged out.

We see that all these operations are directed to the means of destroying the charcoal of the crude iron, when bar iron is wanted; but when steel is required to be made, the metal is not only preserved from the contact of the air, but the vessel is lined with charcoal, in order that, by its contact with the fused matter, it may supply any portion of that principle which may be wanting.

In the foregoing process there are two fusions of the metal. In the latter it is not only completed by the second fusion, but it is rendered more homogeneous. This method is excellent, and is perhaps the only means by which an exceedingly good steel can be had.

The other part of the process is worthy of much attention, namely, the reduction of the crude iron into plates. When bar iron is wanted, these plates roast with more facility

on account of the great surface they present to the air. And when steel is wanted, they are more readily fused, and sink beneath the scoriæ, which prevents the charcoal of the iron from being consumed by the action of the air. On the contrary, they absorb what might have been wanting from the lining of the vessel, which is prepared in such a manner as to support itself without being consumed through the whole of the operation.

When the steel has congealed in the furnace, it is taken out and divided into several portions more or less considerable, which are carried to the hammer. Here a separation is made of such portions as are not reduced into steel, but iron, and which occupy the surface of the pieces. Each piece is drawn out into bars, which are reduced into other smaller bars of different dimensions, by separating the softest parts from those which are more hard.

For steel of a superior quality, several bars of the soft and hard kinds are united by welding and forging. The hardest are placed in the middle.

We have shown that, in order to obtain steel from crude iron, it is necessary to have an iron abounding with coal; but there is an excess which is hurtful. The black crude iron, which contains too much coal, affords a steel so brittle as to be of no use. This kind of steel becomes fixed with more difficulty than good steel. When the workman perceives this symptom, he may prevent the bad effect by adding a certain quantity of old iron fragments, which deprives the too steely metal of its excess of coal, and, by incorporating with it, produces an uniform mass of good steel. When the crude iron is of such a nature as to afford brittle steel, it is usual to mix in the refining furnace a quantity of another kind of crude iron, which may modify its quality.

Though iron and steel are distinguishable by very striking qualities, there is, nevertheless, a point of contact at which they are confounded: the softest steel may be considered as a very hard iron, and, in fact, the several kinds of iron differ in hardness by the same principle which constitutes steel. They all retain a small portion of charcoal, which escapes the operation of refining. Those which contain the least are under like circumstances more flexible, soft, ductile, and susceptible of acquiring by the action of the hammer that fibrous form which constitutes what is called the grain of iron. Hence it is that different kinds of bar iron are sometimes obtained from the same crude iron, though the operation is apparently the same. It is sufficient for this effect that the inclination of the tuyer be changed.

#### *Concerning Steel obtained by Cementation.*

THE steel of cementation is formed by means of a cement, with which bars of forged iron are surrounded in a case disposed in the middle of a furnace, where they are subjected to a strong heat.

We must repeat that the good quality of the iron is a condition indispensable for obtaining good steel. It is of importance to choose the best kind; and the English, who almost exclusively prepare the steel of cementation, retain for that object all the iron of Roslagia, which is the best iron of Sweden, and for which they pay a high price.

It is not enough that the iron should contain no noxious principle. It is also necessary that it should be forged with care, and its parts well united. For if it contain flaws or clefts



clefts within the bars, they become much more perceptible when the iron has acquired the nature of steel; and it would not be practicable to unite them perfectly, because the parts of steel are much less disposed to weld and adhere together than those of iron. We have had convincing proofs in our own experiments, that the irons of France of good quality, such as those of the ci-devant Berry, afford only a bad steel when cemented in the state they usually are delivered in from the forge; but the same iron, after having been carefully forged and hammered, formed steel equally good with that which had been made at the same time from an excellent iron of Sweden. In another experiment the steel prepared with iron of the ci-devant comté de Foix, which had been well forged, produced steel of a quality equal to that which had been obtained in the same operation with Swedish iron.

Hence it results, 1. That the best Swedish iron owes its property of forming good steel, less to any particular quality of the ore than to the care with which it is forged and submitted to the action of the hammers: 2. That we possess in France irons capable of affording good steel, provided care be taken that they be well forged; but the mere neglect in this operation may prove fatal to a plan in other respects well conducted.

The first attention which ought therefore to be paid in the attempt to manufacture steel is to procure good iron, to examine whether it be well forged; and in case this operation has not been sufficiently attended to, it must be forged and made sound before it is subjected to cementation.

It has long been supposed that the cement proper for the steel-making process ought to contain saline, inflammable, fat or sulphureous parts, which were supposed to penetrate the iron and change it into steel. Hence have arisen many pretended secrets, which have diverted the attention of those who have engaged in undertakings of this kind under the guidance of ignorant pretenders. There are no secrets in the composition of the cement. The English use no material but the charcoal of wood reduced to powder; and in fact the only essential condition is, that the iron should become impregnated with the very substance of the charcoal uniformly to its centre.

When the bars or plates of iron which are to be converted into steel are ready for the furnace, they are cut to the length of the case or crucible in which the cementation is to be made. A bed of charcoal powder, sifted through a coarse sieve, and slightly wetted, is laid in the bottom of the case. Upon this is placed a row of iron bars at a little distance from each other. This first layer of iron is then covered with a bed of charcoal powder, which fills the spaces within the bars, and rises to the height of half an inch above them. A new range of bars is then laid and covered with charcoal as before, and in this successive way the operator proceeds till the case is full. The last row is covered with charcoal powder, over which is laid a bed of sand to cover its surface entirely, and prevent its being destroyed by combustion. The sand must be moistened and well pressed together in the form of a roof, rising higher than the sides of the case, so as to be several inches thick in the middle.

When the preparation or charging of the case is finished, the furnace is disposed for making the fire, which is gradually increased, and must be kept up for a longer or shorter time according to the quantity of steel, and consequently according to the size of the case. At Newcastle, where between twenty-five and thirty thousand weight of steel is cemented in two cases contained in a furnace, the operation lasts five days and five nights. At one  
of

of the extremities of the furnace as well as of the case, it is usual to form a hole, by which a bar may be taken out when it is supposed that the cementation may be sufficiently advanced. The workman judges from the colour, and the blisters on its surface, whether the steel be perfect. If he cannot depend on his judgment in this respect, trial is made of this steel by hardening and breaking it. If the cementation have not penetrated as far as the centre, it is easy to distinguish, by the fibrous fracture, that part which still retains the nature of iron.

When the steel is taken out of the cementing furnace, its surface is covered with inequalities and blisters, whence it is called blister steel. In this state its fracture presents very large facets, and resembles brittle iron of a bad quality. Before it is brought to market it is usually forged into flat bars seven or eight lines broad; after which it is suffered to cool in the air without plunging it in water. By this treatment its grain becomes much closer.

As the extremities of the bars thus converted into steel are usually flawed and less perfect, they are cut off and forged together in faggots. This steel is used for instruments of agriculture. If the fire have not been sufficiently active, or kept up for the proper time, the bars are not cemented to the centre; whence they become of unequal hardness, especially if they be not very carefully forged. When the fire has been too intense, the steel becomes too brittle, and difficult to be managed on account of its having dissolved too large a quantity of charcoal. Yet it is impossible to establish any rule for the management of the fire, because it must vary according to the form and size of the furnace, the number and thickness of the bars, and the nature of the fuel.

The form and magnitude of the furnaces vary considerably in the different works where iron is cemented. The objects to be aimed at are, to give the furnace a degree of solidity which shall enable it to resist a great number of operations, to cause the flame and heat to circulate equally on all sides of the case, and to produce the greatest quantity of heat with the smallest expence of fuel.

One very important observation respecting the extent of dimensions which may be given to these furnaces is, that no advantage, or at least very little, in the consumption of fuel is obtained by enlarging these dimensions, because it is necessary that the whole of the heat should be suffered to dissipate at the end of each operation. The case is very different in other manufactories where the accumulated heat may serve for successive processes; for the whole of such fuel as is employed in raising the temperature to the necessary degree in cases of interruption would be entirely lost.

Prudence demands, that he who is desirous of improving or extending the arts should not blindly be led away by the seduction of projects. It is proper to begin the operations on a small scale, in order to render the practice familiar, before furnaces of a certain magnitude are constructed\*.

*(To be concluded, with Annotations, in our next.)*

\* Reference is here made in the original to drawings of furnaces; one contrived by Jars for cementing three or four hundred weight of steel; another of the furnace at Newcastle; another heated by wood, together with some designs relative to the manipulations at Carinthia. None of these, however, are annexed to the memoir. N.

## IV.

*Observations Chemical and Economical on various Subjects\*.*

1. **M. TROMSDORFF**, Professor at Erfurt, has observed that sulphurated hydrogenous gas takes fire and burns with a strong flame by means of the nitrous acid.

2. **M. Linck**, Professor at Rostoc, finds that three parts of nitrous gas, and two of hydrogen gas, obtained by sulphuric acid and iron, are scarcely or not at all diminished when exposed to day-light over water. Common air is not more diminished by this admixture kept a long time : but the mixture itself of these two gases is diminished by the addition of new portions of nitrous gas. **M. Linck** concludes from this observation, that part of the oxygen of the nitrous gas combined with the hydrogen and formed water, and that the remaining oxygen and azote formed a mixture similar to the air of the atmosphere.

**Citizen Vauquelin** made a similar remark ten years ago on nitrous gas placed over a solution of the hydro-sulphuret of lime. The diminution of the gas was considerable.

3. The urine of animals which feed on vegetables does not contain phosphoric acid, but an acid of a vegetable nature. On this passage **C. Vauquelin** remarks, that **Rouelle** announced this fact twenty years ago, and that **C. Fourcroy** and himself have lately proved it by a great number of experiments, which have moreover shewn that the acid which supplies the place of the phosphoric in these animals is the benzoic.

On this occasion it may be remarked, that the vegetable kingdom affords phosphorus ; that the bones which are produced and grow in animals feeding only on vegetable food contain this substance ; whence it appears probable that their urine is not at all times divested of phosphoric acid. **M. Giobert** in his memoir on phosphorus (*Annales de Chimie*, xii. 23.) affirms that the urine of horses is nearly as proper for his process as that of men ; that is to say, that it affords nearly as much phosphorus. But he does not positively relate any experiment to this effect.

4. A German correspondent of the work before us affirms, that the blowing machines or bellows for the iron works in the Hartz, are cubical boxes of wood, which are more easily made than the iron cylinders made use of in England, and that they are very advantageous in the use. From various accounts there is reason to think, that the density and velocity of the stream of air from our iron cylinder worked by a steam engine are greater than has ever been afforded by the wooden bellows or other blowing engines formerly in use. The height to which this air will support a column of water is between six and seven feet, or at a medium near six inches of mercury.

5. Attempts have been made in Saxony, and particularly at Freyberg, to increase the quantity of water made use of to move the machines in that country. It has been found convenient to establish pumps worked by the wind, to raise a portion of the water which passes through the galleries, or is collected in the works. It is not intended to make use of this inconstant first mover to clear the mines of water, but simply to fill the reservoirs which work the hydraulic machines, at the same time that the secondary advantage of assisting to keep the mines clear is obtained. By this means the power of the wind, changeable as it is, may be rendered steadily useful. The celebrated Leibnitz first thought of this con-

\* *Journal des Mines*, No. 29.

trivance, and endeavoured, though at that time without success, to carry it into effect. I apprehend that his reservoir may not have been sufficiently capacious. Attempts have been made in England, to render the action of small streams more steady and constant by the intervention of a reservoir, which should retain a large portion of the water when wet weather rendered it plentiful, and supply the defect at such times as the water failed for want of sufficient rain.

6. An experiment has been made in the large way at the salt-work of Artern, situated in the circle of Thuringia, dependent on the electorate of Saxony, on the possibility of obtaining sea salt merely by the heat of the sun, after having brought the salt water to as high a degree of concentration as the process of graduation is capable of affording. This salt work was the first establishment of the kind in Saxony, by Mr. Borlach, to whom the undertakings of this nature are so much indebted; and it will probably have the honour of being the first in which this new process shall succeed. Experiments on a smaller scale have already afforded the highest hopes of success. Those which have been attempted in the large way, though at the end of the warm weather, have afforded encouraging results.

For this purpose a number of vessels of wood have been placed in a field upon posts, at the height of five or six feet from the ground. They can be covered or uncovered in an instant by a moveable roof made of thin boards, accordingly as the weather is clear or rainy. Though the summer was nearly over when the experiments began, salt was obtained in this manner by the mere heat of the sun; and this salt was much purer, and of a more lively and agreeable taste, than that which is obtained by evaporation in boilers. There is every reason to expect that the whole of the salt which can be obtained at these works, will be separated in this manner without the use of any combustible. A great number of these cases are accordingly prepared, to give all the necessary activity to this method of operating next year.

Citizen Charles Coquebert, in a note on this subject, remarks that the celebrated Haller published, in the Memoirs of the French Academy for the year 1764, a set of experiments on the evaporation of salt-waters made at the works in the canton of Berne, of which he was the director; and adds, that the experiments are interesting, but that the economical calculations are grounded on such erroneous foundations, that they would serve only to mislead those who from the reputation of the author might use his calculations relative to any undertaking in the large way.

## V.

*An Analysis of the Earthy Substance from New South Wales, called Sydneia, or Terra Australis.*  
By CHARLES HATCHETT, Esq. F.R.S.\*

IN 1796, the Right Hon. Sir Joseph Banks, P.R.S. favoured me with a specimen of the Sydneia, which had been lately brought to England. A portion of this I soon after examined,

\* From the Philos. Transf. 1798. The introductory part of Mr. Hatchett's paper concisely states the results of Wedgwood's paper (from which an extract is copied in Philos. Journ. I. 405);—that Professor Blumenbach, in

ed, in a cursory manner, by muriatic acid, but did not obtain any precipitate when water was added to the filtrated solution.

Upon mentioning this circumstance, and expressing a desire to examine this substance with more accuracy, Sir Joseph Banks, with his usual readiness to promote every scientific enquiry, not only permitted me to take specimens from different parts of the box which contained the earth already mentioned, but (that every doubt might be obviated) gave me about 300 grains which remained of the identical substance examined by Mr. Wedgwood.

Upon these the following experiments were made; and, to distinguish them, I shall call the first, No. 1, and that examined by Mr. Wedgwood, No. 2.

## S E C T. II.

### *Analysis of the Sydneia, No. 1.*

THE Sydneia, No. 1, is in masses and lumps, of a pale greyish white, intermixed with a few particles of white mica, and also occasionally with some which are of a dark grey, resembling graphite or plumbago.

It easily crumbles between the fingers, to a powder nearly impalpable, which has rather an unctuous feel.

Small fragments of vegetable matter are also commonly found intermixed with it; and the general aspect is that of an earthy substance which has been deposited by water.

*Experiment 1.*—400 grains were put into a glass matrass, and one quart of distilled water being added, the whole was boiled to one-fourth.

The liquor was then filtrated, and a portion being examined by the re-agents commonly used, afforded no trace of matter in solution. The remainder was then evaporated, without leaving any residuum.

*Experiment 2.* About 200 grains of the earth, rubbed to a fine powder, were put into a glass retort, into which I poured three ounces of concentrated pure muriatic acid. The retort was placed in sand, and the acid was distilled, till the matter in the retort remained dry. Two ounces of muriatic acid were again poured on it, and distilled as before, till only one fourth remained. The whole was then put into a matrass, which was placed in an inclined position, so that, when the earth had subsided, the liquor might be decanted without disturbing the sediment.

When it had remained thus for 12 hours, the acid was carefully poured into a glass vessel: but, as I observed that it was not so perfectly transparent as before it had been thus employed, I suffered it to remain 24 hours, but did not perceive any sediment. Half of this liquor was diluted with about twelve parts of distilled water, and, after a few hours, a very small quantity of a white earth subsided.

In his *Handbuch der Naturgeschichte*, p. 567, 568, mentions that he had examined a portion of this earthy substance, by means of muriatic acid after the manner of Wedgwood, and obtained a slight precipitate;—that M. Klaproth had also examined it, whose results he likewise gives (for which see the page of our Journal last mentioned);—and that the identity of the subjects examined by Wedgwood and Klaproth had been much questioned by me. The author then proceeds to relate the history and particulars of his own analysis, of which the words of the text are an exact copy. N.

This however did not appear to me to be a precipitate caused by a change in the chemical affinities, but rather an earthy matter which had been suspended in the concentrated acid, and afterwards deposited, when the liquor was rendered less dense by the addition of water. To ascertain this, I poured the remaining portion of the concentrated liquor on a filter of four folds: it passed perfectly transparent, and, although diluted with twenty-four parts of water, it remained unchanged, and as pellucid as before. I now filtrated the former portion, and added it to that already mentioned.

It was then evaporated to dryness, and left a pale brownish mass, which was dissolved again, by digestion, in the smallest possible quantity of muriatic acid.

Water was added, in a very large proportion, to this solution, without producing any effect; I then, by prussiate of potash, precipitated a quantity of iron, which was separated by a filter.

The clear solution was then saturated with lixivium of carbonate of potash, and a white precipitate was produced, which was collected andedulcorated. This, when digested with diluted sulphuric acid, was dissolved; and the superfluous acid being driven off by heat, boiling water was poured on the residuum, and completely dissolved it.

To this solution some drops of lixivium of potash were added, and, by repeated evaporations, the whole formed crystals of alum.

From the above experiment it appeared, that the muriatic acid had only dissolved some alumine and iron; but, in order to satisfy myself more completely in respect to the component parts of this substance, I made the following analysis.

*Analysis.* A. 400 grains were put into a glass retort, which was then made red-hot during half an hour. Some water came over, and the earth afterwards weighed 380.80 grains, so that the loss amounted to 19.20 grains. The greater part of this loss was occasioned by the dissipation of the water imbibed by the earth; to which must be added, the loss of weight caused by the combustion of a small portion of vegetable matter.

B. The 380.80 grains were rubbed to a fine powder, and being put into a glass retort, 1470 grains of pure concentrated sulphuric acid were added. The retort was then placed in a small reverberatory, and the fire was gradually increased, till the acid was distilled over: it was then poured back on the matter in the retort, and distilled as before, till a mass nearly dry remained.

On this, boiling distilled water was repeatedly poured, until it no longer changed the colour of litmus paper, and was devoid of taste. The undissolved portion was then dried, and made red-hot; after which it weighed 281 grains.

C. I now mixed the 281 grains with 300 grains of dry carbonate of potash, and exposed the mixture to a strong red heat, in a silver crucible, during four hours. The mass was loose, and of a greyish white: it was softened with water, and, being put into a retort, sulphuric acid was added to a considerable excess. The whole was then distilled to dryness; and when a sufficient quantity of boiling water had been added, it was poured on a filter, and the residuum was well washed: it was then made red-hot, and afterwards weighed 274.75 grains.

D. The solutions of B and C were added together, and were much reduced by evaporation. Pure ammoniac was then employed to saturate the acid, and a copious loose precipitate

pitate of a pale yellowish colour was produced; which, collected,edulcorated, and made red-hot, weighed 103.70 grains.

E. The filtrated liquor of D was again evaporated; and carbonate of potash being added, a slight precipitation of earthy matter took place; which, by the test of sulphuric acid, proved to be some alumine which had not been precipitated in the former experiment: this weighed 1.20 grain.

F. The 103.70 grains of D were completely dissolved when digested with nitric acid, excepting a small residuum of siliceous earth, which weighed 0.90 grain.

G. The nitric solution was evaporated to dryness, and a second portion of the same acid was added, and in like manner evaporated. The residuum was then made red-hot, and digested with diluted nitric acid, which left a considerable portion of red oxyde of iron. The solution was again evaporated, and the residuum, being treated as before, again deposited some oxyde of iron, much less in quantity than the former.

The whole of the oxyde was then heated with wax in a porcelain crucible, was taken up by a magnet, and weighed 26.50 grains.

H. The nitric solution of G was saturated with ammoniac, and a loose white precipitate was formed; which,edulcorated and made red-hot, weighed 76 grains.

I. These 76 grains were dissolved when digested with diluted sulphuric acid; and, when the excess of acid had been expelled by heat, the saline mass was dissolved in boiling water. To this solution I added some lixivium of potash, and, by gradual and repeated evaporations, obtained the whole in regular octoedral crystals of alum.

K. The 274.75 grains of C now alone remained to be examined. They appeared to consist of siliceous earth, mixed with the dark grey shining particles already mentioned; but, as I shall describe, in the following experiments, the process by which these were separated, I shall now only say that they amounted to 7.50 grains.

L. The earth with which the abovementioned particles were mixed weighed 267.25 grains. This earth was white, and arid to the touch: when melted with two parts of soda, it formed a colourless glass; and with four parts of the same it dissolved in water, and formed a *liquor silicum*: it was therefore pure siliceous earth or silica.

The substance here examined was composed therefore of the following ingredients:

	Grains.
Pure siliceous earth or silica	{ F. 0.90
	{ L. 267.25
Alumine	{ E. 1.20
	{ H. 76
Oxide of iron	G. 26.50
Dark grey particles	K. 7.50
Water and vegetable matter	A. 19.20
	<hr/> 398.55

The foregoing analysis was repeated several times, and always with similar results; excepting that, as I had taken the specimens from different parts of a large quantity, I found that the proportions of the ingredients were not constantly the same: that of the siliceous earth,

earth, for example, was sometimes greater, and the alumine and iron proportionably less. Some specimens were also nearly or totally destitute of the dark grey shining particles: in short, every circumstance was such as might be expected from a mixed substance, which, from the nature of its formation, cannot have the ingredients in any fixed proportion\*.

As this substance agreed in its general characters, for the greater part, with that described by Mr. Wedgwood, and as it was indisputably brought from the same place, there appeared every reason to believe that the nature of both was the same; but, to obviate as much as possible any doubt or objection, I determined to repeat the experiments, and the analysis, on that portion which remained of the identical substance examined by Mr. Wedgwood, and which from that period had been reserved by Sir Joseph Banks, who kindly favoured me with it for this purpose.

### S E C T. III.

#### *Analysis of the Sydneia, No. 2.*

THIS substance, as has already been mentioned, consists of a white transparent quartzose sand, a soft opaque white earth, some particles of white mica, and a quantity of dark lead-grey particles, which have a metallic lustre.

The Sydneia, No. 2, appears chiefly to differ from No. 1, by being more arenaceous, and by a larger proportion of the dark grey particles. Many experiments, similar to those made on No. 1, already described, were made on this substance, with pure concentrated muriatic acid; but, as none of these afforded any appearance of a precipitate by the means of water, I do not think it necessary to enter into a circumstantial account of them, and shall proceed therefore to the analysis.

A. 100 grains were exposed to a red heat, in a glass retort, and, after half an hour, were found to have lost in weight 2.20 grains.

B. The 97.80 grains which remained were mixed with 300 grains of dry carbonate of potash, and the mixture was exposed to a strong red heat, in a crucible of silver, during three hours.

When cold, the mass was softened with water, and was put into a glass matrass. I then added three ounces of pure concentrated muriatic acid, and digested it for two hours in a strong sand heat. Boiling water was then added; and the whole being poured on a filter, the residuum wasedulcorated, dried, and made red-hot: it then weighed 85.50 grains.

C. The filtrated solution was evaporated to one fourth; and pure ammoniac being added, a precipitate was formed, which, after a red heat, weighed 10.70 grains.

D. One ounce of muriatic acid was poured on the 10.70 grains, in a matrass, which was then heated. The whole of the 10.70 grains was dissolved, excepting a small portion of siliceous earth, which weighed 0.30 grain.

\* The description given by Mr. Klaproth convinces me that his experiments were made on a portion of this substance. Moreover, when my late friend Mr. Haidinger was in London, I gave him some of this earth for his collection; so that, whether Mr. Klaproth made his experiments on that which had been received by Mr. Haidinger from Sir Joseph Banks, or from myself, it is not less certain that he operated on that which might be regarded as the genuine *Sydneia*.

E. The



E. The muriatic solution was then reduced, by evaporation, to about one fourth; to which I added a large quantity of distilled water, which did not however produce any change. I then gradually added a solution of pure crystallized prussiate of potash, and heated the liquor till the whole of the iron was precipitated; after which, ammoniac precipitated a loose white earth, which,edulcorated and made red-hot, weighed 7.20 grains. The iron precipitated by the prussiate may therefore be estimated at 3.20 grains.

F. The 7.20 grains of the white earth were digested with sulphuric acid, and, after the excess of acid had been expelled by heat, boiling water was poured on the saline residuum. The solution was then gradually evaporated, with the addition of a small portion of lixivium of potash, and afforded crystals of alum, without a trace of any other substance.

G. I now proceeded to examine the 85.50 grains of B. These appeared to consist of siliceous earth, or fine particles of quartz, mingled with a considerable quantity of the dark grey shining particles.

Mr. Wedgwood was of opinion that these were a peculiar species of plumbago or graphite. Professor Blumenbach, on the contrary, regards them as molybdæna: and Mr. Klaproth believes them to be *eisenglimmer* or micaceous iron ore.

When rubbed between the fingers, they leave a dark grey stain, and the feel is unctuous, like that of plumbago, or molybdæna: the traces which they make on paper also resemble those of the abovementioned substances, but the lustre of the particles approaches nearer to that of molybdæna.

In order therefore to determine whether or not they consisted totally or partially of molybdæna, I put the 85.50 grains into a small glass retort, and added two ounces of concentrated nitric acid. The retort was then placed in a sand heat, and the distillation was continued till the matter remained dry. The acid was then poured back into the retort, and distilled as before; but I did not observe that the grey particles had suffered any change, nor were nitrous fumes produced, as when molybdæna is thus treated.

To be more certain, however, I digested pure ammoniac on the residuum; and, having decanted it into a matrafs, I evaporated it to dryness, without perceiving any vestige of oxide of molybdæna, or indeed of any other substance.

It was evident, therefore, that molybdæna was not present; and, as the general external characters and properties corresponded with those of plumbago, I was inclined to believe that these were particles of that substance, and not micaceous iron, as Mr. Klaproth imagined. To determine this, the following experiment was made:

H. 200 grains of pure nitre in powder were mixed with the 85.50 grains, and the mixture was gradually projected into a crucible, made strongly red-hot. A feeble detonation took place at each projection; and, after a quarter of an hour had elapsed, the crucible was removed.

When cold, the mass was porous and white, without any appearance of the dark grey particles. Boiling water was poured on it; and the whole being put into a matrafs, one ounce of muriatic acid was added, and digested with it in a sand heat. By evaporation it became gelatinous: it was then emptied on a filter, and, being well washed, dried, and made red-hot, weighed 75.25 grains.

The appearance of this was that of a white earth, and to the touch. When melted with

with two parts of soda, a colourless glass was formed; and, with four parts of the same, it was soluble in water, and produced *liquor silicum*: it was therefore pure siliceous earth.

I. The filtrated liquor was saturated with ammoniac; and, upon being heated, a few brownish flocculi were precipitated, which, when collected and dried, weighed 0.40 grain. This precipitate was dissolved in muriatic acid, and was again precipitated by prussiate of potash, in the state of Prussian blue.

The liquor from which the flocculi of iron had been separated was then examined, by adding carbonate of potash, and, lastly, by being evaporated to dryness; but it no longer afforded any earthy or metallic substance: so that, by the process of detonation with nitre, the 85.50 grains afforded 75.25 grains of pure siliceous earth, with 0.40 grain of iron; and, as the dark grey substance was destroyed, excepting the 0.40 grain of iron above mentioned, and as 9.85 grains of the original weight of 85.50 grains were dissipated, there can be no doubt but that this substance, amounting to 10.25 grains, was carburet of iron or plumbago; especially as some experiments which I purposely made, on that from Kewick in Cumberland, were attended with similar results.

It is also evident, that these particles could not be *eisenglimmer*, or micaceous iron, as nitre has little or no effect on that substance, when projected into a heated crucible.

In a subsequent experiment on the same, the crucible was removed immediately after the last projection; and I then observed that an effervescence, with a disengagement of carbonic acid, took place, upon the addition of the muriatic acid, as is usual when pure plumbago is decomposed by nitre, and that less of the gelatinous matter was formed by evaporation.

The cause of this difference was evidently the duration of the red heat; for, in the first instance, the alkali developed by the decomposition of the nitre had time to unite with the siliceous earth, so as, when dissolved, to form *liquor silicum*; but, in the second experiment, a portion of alkali remained combined with the carbonic acid, produced by the carbon of the decomposed plumbago.

The produce of 100 grains by this analysis was,

Silica	-	-	-	{ D. 0.30
				{ H. 75.25
Alumine	-	-	-	F. 7.20
Oxide of iron	-	-	-	E. 3.20
Graphite or plumbago	-	-	-	I. 10.25
Water	-	-	-	A. 2.20
				<hr/>
				98.40

Mr. Wedgwood says, that sulphuric acid cannot dissolve the precipitated earth, and has but little effect on the mixed substance, even when distilled to dryness; but, from the preceding experiments, I had reason to believe that the aluminous earth and iron would be separated by reiterated distillation; I therefore repeated the analysis in the following manner.

#### Second Analysis of the Sydneia, No. 2.

A. 100 grains of the earth were put into a glass retort, upon which 400 grains of pure concentrated sulphuric acid were poured. The retort was placed in a small reverberatory, and

and the fire was continued till a dry mass remained. 400 grains of the acid were again poured in, and distilled as before. Upon the dry mass boiling water was poured, and the whole was then emptied on a filter, andedulcorated. The residuum, after a red heat, weighed 87.75 grains, and consisted of siliceous earth, mixed with some mica, and with particles of plumbago.

B. The filtrated solution, by ammoniac, afforded a precipitate, which weighed 9.50 grains; and, being examined, as in the former experiment, yielded 6.50 grains of alumine, and 3 grains of oxide of iron.

The plumbago was separated from the siliceous matter, in the manner already described, and amounted to about 10 grains.

By this analysis I obtained,

				Grains.
Silica and mica	-	-	-	77.75
Alumine	-	-	-	6.50
Oxide of iron	-	-	-	3
Plumbago	-	-	-	10

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97.25

It appears therefore that the Sydneian earth, when treated with fulphuric acid, is capable of being for the greater part decomposed; and Mr. Wedgwood probably did not succeed, because his process was in some respect different, or that the distillation was not sufficiently repeated.

I have not thought it necessary to be more circumstantial in the account of this second analysis, as the operations were similar to those of the former.

#### S E C T. IV.

THESE experiments prove, that the earthy substance called *Sydneia*, or *terra australis*, consists of siliceous earth, alumine, oxide of iron, and black lead or graphite.

The presence of the latter appears to be accidental, and it probably was mixed with the other substances at the time when they were transported, and deposited, by means of water; for this appears evidently to have been the case, from the general characters of this mixed earthy substance.

The quartz and mica, which are so visible, indicate a granitic origin; and the soft white earth has probably been formed by a decomposition of feldt spar, such as is to be seen in many places, and particularly at St. Stephen's in Cornwall. The granitic sand which covers the borders of the *Mer de Glace*, at Chamouni, in Savoy, also much resembles the *terra australis*, excepting that the feldt spar is not in a state of decomposition: in short, the general aspect, and the analysis, concur to prove, that the *Sydneia* has been formed by the disintegration and decomposition of granite, or *gneiss*.

Mr. Wedgwood's experiments are so circumstantial, that had I only examined the earth last brought to England, I should have supposed, with Mr. Nicholson, that I had operated on a different substance; but, as I had an opportunity to examine, by analysis, a portion of the same earth on which Mr. Wedgwood made his experiments, and as I received it from

Sir

Sir Joseph Banks, the same gentleman who had furnished Mr. Wedgwood with it, no suspicion can be entertained about its identity.

Some of the experiments which I have related, and which prove that some of the finer earthy particles remained suspended in the concentrated muriatic acid, and were precipitated when the acid was diluted with water, appear in some measure to account for the mistake which has been made, in supposing that a primitive earth, before unknown, was present; but this alone will not account for many of the other properties mentioned by Mr. Wedgwood: such as,

1st. The repeated and exclusive solubility in the muriatic acid, and subsequent precipitation by water.

2dly. The butyraceous mass which was formed by evaporation. And

3dly. The degree of fusibility of the precipitated earth.

These, indeed, I can by no means explain, but by supposing that the acids used by Mr. Wedgwood were impure. This supposition appears to be corroborated by a passage in Mr. Wedgwood's paper, where he says, "Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all, (only the usual blueishness arising from the iron always found in the common acids.)\*" Now if (as it seems from this expression) Mr. Wedgwood employed the common acids of the shops, without having previously examined and purified them, all certainty of analysis must fall, as the impurity of such acids is well known to every practical chemist†: but whether this was the cause, or not, of the effects described by Mr. Wedgwood, I do not hesitate to assert, that the mineral which has been examined does not contain any primitive earth, or substance possessing the properties ascribed to it, and consequently, that the Sydneian genus, in future, must be omitted in the mineral system.

## VI.

*The Method of making strong Artificial Magnets. By M. COULOMB†.*

I SHALL here present the methods which I have found successful in constructing artificial magnets of very great force at a moderate expence. \* \* \* \* When a steel rod or plate is required to be rendered magnetic, and two bars are used for this purpose, it is obvious that it must be of advantage to cause the poles of these bars to act in conjunction with each other. This has given rise to the method of the double touch. Fig. 1, Plate IV. shows the former practice of this method. If the bars be required to be impregnated, the two bars SN, S'N', were placed vertically at the distance of seven or eight lines from

\* Philosophical Transactions, vol. lxxx. part ii. p. 313.

† It appears from Wedgwood's paper, that nearly one fifth part of the mineral was taken up by muriatic acid, and that his solution was reckoned to have about six grains of the soluble matter to three ounces of the acid. From the experimental process of boiling, it seems probable that the actual quantity of acid made use of, bore an higher proportion to the matter taken up. If we admit the supposition of impurity in Wedgwood's acid, the quantity of matter precipitable by water was probably less than two grains in the ounce: and after rejecting the Sydneian earth, as we undoubtedly must, it may perhaps be an object worthy of enquiry to determine what that substance was in which Wedgwood observed the peculiar properties related in his paper. N.

‡ Journal de Physique, xliii.

each other, more or less according to their force; the points S and S' representing the south, and the points N, N' the north poles. In this situation the two bars were moved from one end to the other of the bar n s.

M. *Æpinus* has remarked, that in this method the centre of action of the two magnets NS, N'S' being necessarily placed at some distance from their extremities at the point u for example, the action on the points of the bar n s comprehended between the two bars is made very obliquely, and consequently does not communicate as much magnetism as the subject is capable of receiving. Whence, instead of placing the two bars upright in this operation, M. *Æpinus* advises that they should be inclined, as in fig. 2; and in this position moved from one end of the bar to the other.

I have, in fact, found by the magnetic balance described at the beginning of this memoir\*, that the method of M. *Æpinus* is preferable to the other; but I have also observed, that it does not give the needles the perfect saturation of magnetism, and that frequently when the needle is of considerable length, several poles are formed on the intermediate parts, the action of which is indeed small, but nevertheless perceptible. I attribute these to the particular action of each magnet, which tends to produce on the points passed over by the magnets an effect contrary to that which is desired. In our figure 2, the pole S for example being placed upon the needle, tends at the same time to give to the point q, which is placed under the bar, the same kind of magnetism as at the point u; that is to say, on the hypothesis of two magnetical fluids capable of moving towards the extremities of the needle, if the point u be drawn towards the point n, the point q which is near it will be drawn towards the point s, after this point q shall have been passed by both magnets. In my hypothesis, in which the magnetic fluid is not capable of moving, except in the integrant parts, the molecules u and q, which are near each other, tend to become magnetized in opposite directions; and must produce a diminution of magnetism towards the extremities of the needle, where the magnetic fluid must be most condensed; a circumstance which may produce several poles in very long needles, as is proved by experience. This observation, which necessarily results from the accurate measures afforded by my experiments, obliged me to depart from the method of M. *Æpinus*. The following is that which, after various trials, has proved by the magnetic balance to be the most advantageous.

In my operation I use four very strong magnets impregnated by a first process, which I shall presently describe. I place my two strongest magnets (fig. 3.) NS, NS, on an horizontal plane in one right line, at such a distance that they may be a few lines nearer to each other, than the length of the needle n s intended to be magnetized. I afterwards take the two magnets N'S', and inclining them as in the method of *Æpinus*, I place them first on the middle of the needle, or with their poles nearly in contact. I then draw each magnet, without changing its inclination, to the extremity of the needle, and repeat this operation five or six times on each face of the needle. It is clear that in this operation the poles of the needle n s remain fixed and invariable at the extremities of the needle, by means of the two strong magnets NS on which it rests. The effect produced by these can only be

\* The excellent and well known method of Coulomb of measuring the forces of electricity and magnetism, is by suspending an horizontal lever or bar by a fine wire at the point of suspension; which bar being turned causes the wire by its torsion to exert a determinable force against the action intended to be measured. N.

augmented by the action of the two superior magnets, which concur in magnetizing all the particles of the needle in the same direction.

As the needle *n s*, placed between the two large magnets in the preceding operation, acquires by the joint action of the four bars a degree of polarity which is more than it can preserve when separated from them, it follows that at the moment of this separation the needle loses part of the magnetism it derived from those forces, and that its magnetism diminishes until the magnetic action of the whole needle on each of its parts is in equilibrio with the coercive force. Hence, upon separating the needle from the magnets, it is found to be saturated with magnetism.

I have found likewise, that in this method of magnetizing, there is a greater certainty of giving to both surfaces of needles, intended to determine the magnetic meridian, an equal degree of magnetism; a circumstance deserving of the greatest attention in the construction of compasses, if the needle be suspended with its broadest surface parallel to the horizon.

#### *The Construction of Artificial Magnets.*

I TAKE fig. 4. thirty bars of steel hardened and tempered to the temper of a spring \* 5 or 6 lines broad, 2 or 3 lines thick, and 36 inches long. The blades of fencing foils, such as are found in the shops, make pretty good magnets. English sheet steel † (*la tôle d'acier d'Angleterre*) cut into pieces one inch wide, hardened and lowered to spring temper, is preferable. When each compound magnet is to contain no more than 15 or 20 pounds of steel, it is sufficient to make the bars 30 or 36 inches long.

I magnetize each bar singly, according to the method already described. I then take two rectangular parallelopipedons of very soft iron, well polished, 6 inches in length, between 20 and 24 lines broad, and 10 or 12 lines thick ‡: with these two parallelopipedons represented fig. 4. at *N* and *S*, I form the armor of my magnet by enveloping one extremity of each parallelopipedon with a stratum of my magnetic bars, so that the extremities of the parallelopipedons may project beyond the extremities of the bars 20 or 24 lines, and the other part may be enveloped by the ends of the set of bars. On this first layer of steel bars of 3 or 4 lines thick, I place a second which is three inches shorter than the first, so that the first projects beyond the second 18 lines on each side. The whole is secured at the ends by two binding pieces of copper, which press the bars close together, and prevent the armor from escaping.

Fig. 4. represents two artificial magnets composed according to the method just described. *N* and *S* are the extremities of the two iron parallelopipedons. The two other extremities are inclosed by the bars. Each magnet thus compounded is solidly connected

\* By other experiments, the author has ascertained that the magnetism acquired by steel is least when it is either absolutely hard or annealed by a white heat, and greatest when it has been hardened and annealed by a very obscure red heat; from which middle term it diminishes either way, whether the bar be harder or softer. *N*.

† We have two sorts at least in the London market. The common sort is sold retail at 6d. or 7d. per lb. the finer, under the name of cast-steel (which I suppose it to be) is sold at one shilling per lb. and deserves its price. I have not been able to procure sheet steel thicker than 1-8th of an inch. *N*.

‡ All the weights and measures in this paper are French; but as extreme precision is no where implied, it was unnecessary to reduce them. *N*.

together by the copper pieces marked a, b, a', b'. The pieces of contact A, R, join the opposite poles of the magnets.

Experience has shewn me, that with an apparatus of this form, each part weighing 15 or 20lb. a force of 80 or 100 pounds will be required to separate the pieces of contact; and that when an ordinary needle of the compass is placed on the two extremities of our two compound bars, fig. 4, they become magnetized to saturation without its being necessary to rub them with the upper pair of magnets. It is scarcely necessary to observe, that when magnets of greater force are desired, it is necessary, in proportion as the number of bars is increased, to augment their length also, and the dimensions of the parallelopipedons of iron which serve for the armor. It would be easy to ascertain the different dimensions which the magnets ought to have, in a manner sufficiently accurate for practice, from the laws of magnetism; and the position of the centre of action of the bars of steel, of different lengths and thickness, which we have explained in the course of this memoir.

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## VII.

*On the Separation of Argillaceous Earth from Magnesia. By Mr. F. ACCUM.*

To Mr. N I C H O L S O N.

SIR,

THE separation of argillaceous earth from magnesia, when both are combined in one substance, as is very often the case in mineral bodies, has been hitherto considered as one of the most difficult operations in the whole analysis of artificial and natural compounds. The experimental chemist, who is practically employed in the investigation of mineral substances, must be sensible of the difficulties which unavoidably accompany the present method of operation; and must likewise allow, that notwithstanding the utmost care, accuracy and skill, it is scarcely possible to avoid uncertainty in his conclusions and results. For these reasons, I beg leave to observe, that the above object may be accomplished to the utmost precision in the following manner: When both these earths are in a solution of muriatic acid in a perfectly saturated state, the argillaceous earth may be totally separated by means of carbonate of ammoniac; as this will only disengage that earth, and the magnesia will be retained in solution; which may then be separated by adding solution of pure potash or soda to the remaining fluid.

I am, Sir, your humble servant,

Haymarket, No. 17.

FREDERICK ACCUM.

## VIII.

*Extracts from the Manuscripts of Leonard de Vinci. With Remarks, by J. B. VENTURI, Professor of Natural Philosophy at Modena, Member of the Institute of Bologna\*.*

I. **O**N the descent of heavy bodies combined with the rotation of the earth, De Vinci shows by a figure, that a body let fall from an eminence will continue perpendicularly over the same spot, notwithstanding the rotation of the earth, and consequently infers that it will describe a spiral line. (It is an ellipsis.)

It was at the commencement of the 16th century that the works of Nicolas de Cusa were printed, in which that author endeavoured to renew the ancient doctrine of the motion of the earth, though in a confused and metaphysical method. The writing of de Vinci is nearly of the year 1510, and shows that this notion was in a state of discussion in the minds of discerning men before the time of Copernicus. It has been asserted, that Regiomontanus supported this doctrine; but he opposes it directly in a writing preserved by Schoner, and in his commentaries on the Almagest †. The doctrine of the motion of the earth was publicly maintained for the first time at Rome, in 1533, by Widmanstæt ‡, who affirmed that he had learned it from Copernicus. The work of this last did not appear till 1543. Vinci was the only man at that time who was sufficiently acquainted with mechanics to apply the theory of combined motion to the fall of heavy bodies, an application of which the honour was assumed by Gassendi in the last century §. In this state the opinion of philosophers remained, until D'Alembert demonstrated, that heavy bodies projected towards the zenith ought not to fall exactly at the place whence they set out. A similar idea has been taken up in my country. The tower Asinelli in Bologna is about three hundred feet high. A ball exactly round being let fall from this height to the earth, ought to deviate nearly six lines from the perpendicular. J. B. Guilielmini made the experiment in 1792 with great care. He could not avoid some aberrations, of which the mean result, however, confirmed the truth of a fact which had before been demonstrated by astronomy and mechanics ||.

II. Concerning the earth divided into fragments.—L. de Vinci affirms, that if the earth were cut into fragments, and dispersed through the surrounding space, a single fragment being let fall would be carried to the common centre, which it would pass to a nearly equal distance on the opposite side, and return again nearly to the place whence it set out; and in this way the vibrations would continue:—that if all the fragments were suffered to fall at different times, they would meet, strike, and break each other, and a tumultuous

\* From the “*Essai sur les Ouvrages Physico-Mathématiques de Léonard de Vinci*,” of which notice is taken in *Philos. Journal*, I. 599. I have abridged the passages in many instances. All the remarks in the text are by Professor Venturi.

† Schöneri Opera, pars secunda, cart. 127. Regiomontan. in Almagestum, l. p. conclus. 5.

‡ Marini Anchiatri Pontificii, tom. 2, pag. 351.

§ De motu impresso a motore translato. Paris 1642.

|| De diurno terræ motu experimentis confirmato. Bonon. 1792.—An experiment of this nature was proposed to the Royal Society, and a discourse read thereon by Robert Hooke, who inferred that the ball would in our latitude fall to the S. E. This, on trial about December 1679, proved to be the case. See Ward's *Lives of the Gresham Professors*, p. 184. N.



commotion would be produced in the atmosphere, which would continue for years, until all the parts were united about the common centre\*.

III. Concerning the earth and the moon.—That the earth is a star; that the scintillation of the stars is an affection of the eye †; that the earth performs the same office of illumination to the moon, as the moon to the earth, but with contrary phases; that the earth in lunar eclipses does not receive light by reflection from the moon, *nor the moon from the earth* in solar eclipses; that the obscure illumination of the dark part of the moon at the beginning of the first and end of the last quarters is produced by reflection of solar light from the earth.

IV. On the action of the sun upon the ocean.—Our author affirms that the heat of the sun causes the waters of the sea to rise in an eminence beneath the equator, from which they flow on all sides, as may be observed in water heated over a fire; and that the aqueous eminence following the sun in the diurnal motion is carried through about a thousand miles per hour.

De Vinci has here applied the same principle to the waters of the ocean, as Halley has since used to explain the trade winds in the atmosphere ‡.

V. The ancient state of the earth.—When the water of rivers deposited its mud upon the marine animals living near the shore, this mud impressed itself upon the animals themselves. When the sea afterwards retired, this mud became petrified all round, and within the shells of testacea which it had penetrated. These are found in various places; and most of the shell-fish petrified in the mountains have their shells entire, particularly those of the greatest age and hardness. In answer to a supposed objection, that the influence of the stars may have formed these shells in the mountains, the author requires to be shewn any place where this operation of the stars is in actual progress to form shells of different ages and species in the same place. And how upon that system it can be explained why the gravel has become indurated in strata at different heights in the mountains. This gravel, continues he, has been transported thither from various places by the currents of rivers. It was formed of fragments of stone, of which the corners have been worn down by the frictions, blows, and falls they have undergone in the water which rolled them to their present situation. And how can this system explain the great number of different kinds of leaves bedded in stone near the tops of mountains? and the Alga, a marine plant, intermixed with shells and sand, and petrified in a mass with sea crabs broken and confounded with the same shells.

\* In this section it is evident, as Professor Venturi remarks, that our author has a clear comprehension of the inertia of matter. Another step in the process of reasoning would have given a revolution, to his falling fragment, in an appropriate orbit. The vibration of a body in a cavity through the centre of the earth to the antipodes, and the retention of the moon in its orbit by the combination of the projectile and gravitating forces are stated in a passage of some length by Plutarch, in his treatise *De placitis philosophorum*, as I well remember, but have not the work at hand to quote. N.

† On this subject it may be remarked in favour of scintillation being a consequence of the irregular density of the air, and not an affection of the eye, 1. That it is less on lofty mountains, and countries where the air is almost constantly serene. 2. That in the same star it is less, the greater the altitude. And 3. That, contrary to a general observation, it may be observed through a telescope, provided the disc of the star be enlarged by deranging the focal adjustment. N.

‡ And very lately again applied to the sea by Count Rumford. *Philos. Journal*, I. 573. N.

The sea changes the equilibrium of the earth. The oysters and other shells which are formed in the mud of the sea, attest the change which the earth has undergone about the centre of the elements. Large rivers always wear away the earth which they detach by friction from their beds. This corrosion discovers to us many banks of shells heaped together in different layers, and the shell-fish have lived in the same place when the waters of the sea covered them. These banks in the course of time have been covered by other strata of mud of different thicknesses; so that the shells have been bedded in the mud heaped above them in such a manner as to rise above the surface of the water. At the present time these beds are at the height of hills and mountains, and the rivers, by wearing them away, discover the strata of shells at their summits. Here then is a portion of the earth become lighter, which continually rises while the opposite parts approach nearer and nearer to the centre of the earth; and that which was formerly the bottom of the sea is now become the summits of the mountains.

When a river forms banks of mud or sand, and afterwards quits them, the water that runs from these masses shews the manner in which the mountains and valleys may by degrees be formed in a soil rising from the bottom of the sea, though this ground might at first have been nearly plain and uniform. The water which flows from this land elevated from the bottom of the ocean, begins to form currents at the lower parts, and excavates the beds of rivulets which receive the fluid from the neighbouring parts. The rivulets, afterwards fed by the rain waters, become broader and deeper every day, and are converted into torrents passing through ravines: they unite into rivers, and by continually wearing away their banks they convert the land between them into mountains. The rains have incessantly swept and degraded these mountains. The elevated rock remains surrounded by the air; the earth of the summit and its sides has descended to its base, and, by raising the bottom of the sea which surrounded that base itself, has forced it to retire to a distance.

Vinci is here the first among modern philosophers who maintained that the greatest part of the continents have formerly existed at the bottom of the sea. We cannot reject this doctrine, which is proved by every geological observation; but we do not yet discern the means of reconciling the successive transportation of the sea, on the surface of the globe, with the laws of gravitation. L. de Vinci offers an explanation which may well deserve some examination. It is different from the opinion of Bernier, who has ascribed a motion to the centre of gravity, without changing the arrangement of the solid parts of the earth. But the profound geometer, who has lately traced the system of the world, agrees that the observations of Bouguer and Maskelyne on the attraction of mountains do not entirely determine the density of the interior part of the earth\*. We may therefore still suppose at present, as Vinci does in his writings, that the fluid mass in our globe is equal, or may be even greater than the solid portion: or, if this hypothesis be not admitted, it will be sufficient if we suppose that there are several detached solid masses in our globe, so that each continent may be considered as a small part of the total mass of the earth. Whence the mass of matter transported by the rain during the lapse of centuries from the summits of mountains to the bottom of the sea, may be sufficiently great, in proportion to the mass of each continent, to cause those continents to emerge more and more above the surface, or

\* Exposition du Système du Monde, par P. S. La Place.

to cause them to rise on one side, while they become immersed on the other, until at length some great shock may reverse them entirely, or cause some new masses to rise from the bottom of the sea. This will be, it may be said, an hypothetical notion; but it is a notion which shews that de Vinci contemplated the cause of the phenomenon in a way sufficiently agreeable to the mechanism of gravitation. No philosopher since his time has yet thought of a more satisfactory explanation\*.

VI. Concerning flame and air.—Where flame is produced, a current of air takes place around, which is necessary to preserve and augment the flame. The stronger the motion of the air, the more brilliant the flame and the greater the heat. Fire incessantly destroys the air which nourishes it; and it would produce a vacuum, if other air did not rush in and fill it. When the air is not in a state proper to receive the flame, neither flame nor any terrestrial or aerial animal can live. No animal can live in a place where flame cannot live.

Smoke is produced in the centre of the flame of a candle, because the air which enters into the composition of the flame cannot penetrate to its interior part. It is arrested at the surface of the flame, which it condenses; by becoming the aliment of the flame it is transformed into it, and leaves a void space, which is successively filled by other air.

\* I must confess that the difficulties of this theory do not appear to be removed either by the observations of the author or his learned commentator. I apprehend that the hypothesis requires or asserts, that some natural process should take place to raise the land above the surface of the water, which was originally beneath that surface; and the assumed principle is, that it shall rise because lighter than an equal mass of water. The specific gravities of mineral substances are totally repugnant to this notion of floating continents, unless we suppose them to be hollow, which is by no means rendered probable to the required degree. Neither do we know of any facts which shew that the longitudes and latitudes of places on the several continents are liable to change, as must be the case if each continent were a separate mass capable of librating on a comparatively narrow base of support, as seems to be implied in one part of Professor Venturi's annotation. It appears however to be well ascertained, that the sea has formerly surmounted the level of high, and perhaps the highest mountains; and from the regularity of disposition which in many instances is observable (*Philos. Journal*, I. 221.) it may be doubted whether the shock of a comet (*Philos. Journal*, II. 42.) and the supposed change of the earth's axis of rotation are sufficient to account for its subsequent depression. I think it is scarcely disputable, that the whole of the solid mass of the earth is connected together, and that the fluid portion occupies the cavities to which it can have access in this solid. If we suppose the globe of the earth to have been formerly enveloped by a sea, whose surface was between two and three miles higher than the surface of the present ocean, or even much less elevated, the simple enquiry will be, what has become of it? It cannot have assumed the elastic state; for we know the medium weight of the atmosphere, which corresponds with little more than a shell of water covering the earth to the depth of about thirty-three feet. We are therefore led to the following objects of investigation: 1. Is the earth solid throughout, or does it afford natural cavities into which the water may, in the course of ages, have found its way? 2. As a very large part of the present surface of the earth has been thrown up by the chemical process of volcanic combustion, and these processes must have left cavities beneath the surface, it may become a matter of computation to determine the solid contents of volcanic product above the present level of the sea, in order to ascertain the quantity of depression the sea would have suffered on the supposition of its having sooner or later flowed into those cavities. 3. If, upon careful enquiry into these facts, it should be ascertained, or rendered highly probable, that the primitive sea has in a great measure disappeared by flowing into cavities in the solid mass of the earth, would not the globe of the moon afford an instance in confirmation of such a process? For the obscure parts of that globe which were at first thought to be seas are observed to be cavities, probably the beds of ancient seas. It seems likely that the component parts of the moon (whatever may be those of the other planets) are nearly the same as of the earth. Yet its atmosphere as well as its surface indicates a great want, if not the total absence of water. Has it required less time for the lunar sea to be absorbed?—Much more might be said: but on an object where facts are wanting, and conjectures plentiful, too much has perhaps been said already. N.

Muschen-

Muschenbroek and most of the philosophers of the present century have attributed scarcely any other function to the air than that of compressing the caloric or fuel in a state of combustion, and blowing away the ashes. Chemistry has lately proved what Vinci had before discerned, and Mayow and Hooke had suspected towards the end of the last century.

VII. and VIII. On Statics.—The author explains the theory of the oblique lever, the inclined plane, and the general principle of virtual velocities in machines.

Vinci and Galileo both observed, that the descent of heavy bodies is made more speedily through an arc of a circle than by an inclined plane; but their proofs are imperfect in certain respects. It has since been demonstrated that the cycloid is the curve of quickest descent. Nevertheless, I find that there is a minimum of time of descent in a circular arc, which may be determined in a synthetical method of considerable simplicity, by means of the following theorem.

An arc of a circle which does not exceed  $60^\circ$  is a curve of speedier descent than any other curve which can be drawn within the same arc:—and the arc of  $90^\circ$  is a curve of speedier descent than any other curve which can be drawn without the same arc\*.

IX. Concerning water drawn from a canal.—The quantity of water which issues from a canal through a given aperture, may vary from many causes. 1. From the greater or less height of the water of the canal above the aperture. 2. From the greater or less swiftness of the water along the side or bank, in which the opening is made. 3. From the greater or less convergence of the sides of the aperture. 4. The greater or less thickness of the side of the canal. 5. Whether the aperture be circular, square, triangular, or oblong. 6. Because the aperture is more or less obliquely situated with regard to the bearing or direction of the side. 7. Or more or less inclined to the horizon. 8. Whether the opening be in a convex or concave part of the bank. 9. Whether there be cavities or prominences in the bed of the canal opposite the aperture. 10. Whether the air do or do not insinuate itself into the current of water which issues forth. 11. Whether the water at its emission fall freely in the air, or is conducted away by an open trough, or by a pipe closed all round. 12. Whether this conducting pipe have a greater or less diameter with regard to the quantity of water which descends in the tube itself. 13. Whether the same conducting pipe have a greater or less length in its descent. And 14. Whether the bore of this pipe be equal or rough, straight or curved.

It must be admitted here, as well as elsewhere, that De Vinci, though possessed of a mind so powerful as to discern the true causes of natural phenomena, had not the advantage of modern analysis to ascertain and demonstrate the quantum of action of these causes. But, on the other hand, it must be allowed that hydraulics even in our time has not yet perfectly and accurately determined all the laws according to which the fourteen circumstances pointed out by Leonardo affect the quantity of water emitted through an aperture of a given magnitude.

X. Concerning circular eddies, or whirls of water.—The author marks the effect of the inertia of bodies which produces a centrifugal force in curve-lined motions. He remarks that the cavity produced in water by a whirling motion is least at the bottom, because there the

\* For the demonstration I must refer to the Essay, p. 19. N.

pressure is greatest; and that, contrary to the observable motion in a wheel, the parts nearest the axis in the present case move the swiftest \*.

XI. On vision.—The inversion of images formed in the darkened chamber by rays admitted through a small hole in a plate of metal are explained by a diagram, and applied to the phenomena of vision. It is also remarked, that the apparent magnitude of remote objects may be enlarged by rendering the pencils of light more convergent. This is shewn by a figure, but the means are not perspicuously explained.

XII. On military architecture.—Under this title the author gives directions for constructing military works; and considers their relative properties with regard to attack and defence. He exhibits, as his commentator remarks, a sketch nearly complete of the art of fortification, and, the attack and defence of places, at the commencement of the sixteenth century. We observe platforms, buttresses or counterforts, ravelins, the glacis surrounding the ditch, trenches, advantages of the ricochet, mines and countermines, and in some designs of de Vinci there are even counterguards. It must however be remarked, that Leonardo was much superior to the engineers of his time; in proof of which his observations in this section may be compared with what Machiavel delivers on the same subject in his Art of War, and what Albert Durer has written upon fortification †.

It is pretended that the bastions of Verona ‡ were the first which were constructed with right-lined faces, flanked in the modern fashion; and the merit of this invention has been ascribed to San Michel. The bastions of Verona were however constructed after the death of Leonardo, who had explained the necessity of that disposition, and has marked embrasures in each flank of the bastion in one of his figures.

Geufs, in his *Théorie de l'Art du Mineur* ||, reproaches Vallière for having robbed Peter Navarro of the glory of the invention of mines, to attribute it without foundation to Francis Georgi §. Vallière may be defended by the testimony of Biringuccio, who was the countryman and contemporary of Georgi. He expressly says ¶, “that it was this engineer who, being consulted by Navarro respecting the means of taking the castle of Naples, proposed and executed the mines, and that the glory remained with the commander General Navarro, because renown disposes itself on the side of great men.” But, in fact, the invention of mines with gun-powder is more ancient than either of these personages. The Genoese executed them in 1487 \*\*. The manuscript N° 7239 of the French National Library was written by an Italian who lived at the commencement of the 15th century. The author explains very clearly (fol. 32) the manner of taking a castle situated on a mountain, by the operation of mines ††.

XIII. Concerning certain instruments.—1. The proportional compasses. Its centre is moveable; and, as the author remarks, it may be used for the proportions of incommensurable quantities, and also to form an oval which shall have a given ratio to a given

\* Professor Venturi has treated this subject in his *Recherches Expérimentales*, &c. p. 58, for an account of which work see our Journal, I. 525.

† De urbibus arcibus castellis, fol. Paris, 1535.

|| In 8vo. Mastricht 1778. § 21.

¶ Pyrotechnia, liv. 10. cap. 4.

†† Mr. Venturi gives a more particular account of this manuscript and its contents in the notices at the end of the present treatise.

‡ Maffei Istoria di Verona, part. 3, cap. 4.

§ Folard, tom. iii.

\*\* Guicciardini Istoria, lib. vi.

circle. M. Venturi informs us, that from a drawing in the manuscript it does not appear to have been different from the instrument now in use. 2. An instrument to shew the constitution and density of the air, and when the weather inclines to rain. From the figure it appears to have been a balance loaded at each end, and provided with a graduated circle to shew its changes of inclination. M. Venturi thinks that one of the weights in equilibrio was a body proper to become charged with the humidity of the atmosphere. This does not very well agree with the consideration of density, though it must be confessed that the equal magnitudes of the two weights in the drawing do not give much countenance to the notion of its having been a statical baroscope. 3. A leathern case or clothing for a diver in the pearl fishery. 4. A piece (*bâton*) which locks alternately in the teeth of a crown wheel to operate like the verge of the balance in clocks; but for what purpose is not said\*.

XIV. Two chemical processes.—1. A flaming ball composed in the following manner:—Take the charcoal of willow, nitre, brandy, resin, sulphur, pitch, and camphor. Mix the whole together over the fire. Plunge a woollen cord in the mixture, and form it into balls, which may afterwards be provided with spikes. These balls being set on fire, are thrown into the enemy's vessels. It is called the Greek fire, and is a singular composition, for it burns even upon the water. Callinicus the architect taught this composition to the Romans (of Constantinople), who derived great advantage from it, particularly under the emperor Leo, when the Orientals attacked Constantinople. A great number of their vessels were burned by means of this composition.

2. Oil fit for painting. Choose the most perfect nuts, take off the shell, soak them in a vessel of water to separate the skin; then leave the kernels in clear water, which is to be changed as often as it becomes turbid, which may be six or eight times. After a certain time, the nuts on being stirred will be found to mix with the water, and form a solution resembling milk. Expose this in plates to the open air, and the oil will rise to the surface. In order to obtain this oil very clean and pure, provide cotton wicks, one end of which is to be dipped in the oil, while the other part passes over the border of the vessel, and descends into the neck of a glass phial two inches below the surface of the liquor in the plate. The oil will by degrees filtrate along the wick, and pass very fine into the phial, all the impurities remaining in the plate. All the oils of grain and of nuts are equally clear. It is the manner of extracting them which renders them foul.

The composition of the Greek fire here given by Vinci, is found nearly in the same words in Baptista Porta†, whence it appears that both authors derived their information from the same source. The modern discoveries in chemistry respecting combustion have disclosed the whole secret of compositions which burn without access to the atmosphere, but by means of oxygene afforded from nitre. The balls described by Frezier‡, which shine on the surface of water, are nothing else but the Greek fire.

XV. Concerning method.—It is always of advantage to the understanding to acquire

\* The interesting commentary or note on this last passage has already been given, for the most part, at page 51 of the present number.

† Mag. Natur. lib. xii. cap. 2.

‡ Des feux d'artifices, edit. de 1747, p. 355. See also p. 332.

knowledge whatever may be its nature; we may afterwards choose the good, and reject the useless. (Qu.?)

Theory is the General; Practice is the Army.

Experiment is the interpreter of the contrivances of nature. This interpreter never deceives us. Our judgment sometimes misleads itself in expectation of consequences which experiment refuses to verify.—We must recur to experiment, and vary the circumstances until we have deduced general rules, for it is experience which affords the true rules.—But you may ask, What is the advantage of these rules? I reply, that they direct our researches into nature, and the operations of art. They prevent us from deceiving ourselves or others by the promise of results which cannot be obtained.

There is no certainty in sciences to which some part of the mathematics cannot be applied, or which do not depend in some manner upon mathematical knowledge.

In the study of sciences which depend on the mathematics, those who do not consult nature, but authors, are not the children of nature. I would call them her grand-children. Nature, in fact, is the sole leader of true genius. But such is the folly of men. He who chooses rather to learn from the original source, instead of applying to authors, becomes an object of derision for his singularity\*.

## IX.

*Observations and Experiments on the Formation of Sulphate of Soda, or Glauber's Salt, in Salt Waters, at a Temperature beneath the Freezing Point of Water, and upon an easy Method of disengaging all the deliquescent Salts. By M. GREN †.*

THE influence of temperature upon the established laws of mutual decomposition of the neutral and middle salts, is a circumstance very essential to be observed. Many salts are not decomposed above the temperature of freezing water, though very well beneath that temperature. Such, for example, are the sulphate of alumine and the muriate of soda; the sulphate of magnesia and the same muriate.

The formation of Glauber's salt during congelation in salt water, wherein previous analysis did not discover it, had long since engaged the attention of the author. He concluded that the sulphate of soda owed its origin to the mutual decomposition of sulphate of lime and muriate of soda, at a temperature below zero. A more accurate analysis of these waters after the severe cold of 1794 and 1795, proved that he was mistaken, and that the sulphate of soda had been formed by the double decomposition of the muriate of soda and the sulphate of magnesia. The excessive cold of 1785 had already exhibited this decomposition to Scheele, and he had shown that sulphate of soda was produced from a mixture of two parts of the solution of sulphate of magnesia, and one part of the muriate of soda, exposed to the temperature of ice.

\* Professor Venturi intends to publish as speedily as circumstances will permit, in three complete treatises, the whole works of De Vinci on mechanics, hydraulics, and optics. N.

† The memoir of M. Gren was addressed by himself in manuscript to the Council of Mines in France, who published it in the xxviii<sup>th</sup> Number of the Journal des Mines. Citizen Descotils abridged the same in the xxiv<sup>th</sup> volume of the Annales de Chimie, page 121. The above paper is a translation from his abridgment. N.

M. Gren was convinced by experiment, that the sulphate of magnesia in salt waters forms at a sufficient degree of cold all the sulphate of soda which can then be afforded, and of which they did not before contain the smallest particle. He endeavoured, but constantly without success, to reproduce the muriate of soda, and the sulphate of magnesia, by an elevation of temperature. So that salt waters which contain sulphate of magnesia change their nature; and their composition is no longer the same when they have been subjected to some degrees of cold below the freezing point; and a subsequent increase of heat will not restore their former state.

The quantity of muriate of magnesia in salt waters which contain sulphate of magnesia, is increased by a temperature beneath zero. It is therefore productive of a real loss to suffer the salt water to remain in the basons during the winter, because they will afterwards afford a muriate of soda of a bad quality, and the sulphate of soda which is obtained will not compensate for this disadvantage. It appears likewise that the hope of concentrating sea water by frost is chimerical.

It was an important question to be resolved in the art of making salt, what might be the means of separating the deliquescent salts even before the evaporation. In this respect M. Gren has succeeded. His processes are founded on the decomposition, well known to chemists, of muriate of lime by sulphate of soda, and muriate of magnesia by caustic lime.

If the water contain only muriate of lime with the muriate of soda, the sulphate of soda is sufficient: if there remain muriate of magnesia and sulphate of soda, lime will suffice. In this case muriate of lime is formed, which afterwards decomposes the sulphate of soda by double affinity.

If the water contain the muriate and sulphate of magnesia without sulphate of soda, it will be necessary to employ lime and sulphate of soda at the same time.

The same substances must also be employed, if the water contain the muriates of lime and magnesia together; which then entirely exclude the sulphates of soda and of magnesia.

Lastly, if the sulphate and the muriate of magnesia be present with the sulphate of soda, lime only need be employed, unless the quantity of the sulphate of soda be found insufficient. In this case a portion of the salt must be added.

These processes introduce no foreign salt into the waters, because they are already saturated with sulphate of lime, which does not render the muriate of soda more impure, because it separates in the state of schlot. They have the advantage to clear it of the sulphates of soda and magnesia, which alter the purity of the muriate of soda, and render it bitter. Besides which, in the practice, no mother water remains. The whole of the fluid may be evaporated to the last drop, and all the salt extracted, which will constantly be pure marine salt. The directors of salt works will easily perceive how much profit will thus be obtained with regard to time and fuel consumed in the evaporation and drying of the salts. But this is merely a secondary profit, greatly inferior to the principal advantage of obtaining a pure salt not deliquescent, but capable of resisting the impression of the air, and of being preserved and transported without loss.

The strongest objection against the use of these processes may arise from the necessity of purchasing sulphate of soda; and this objection would be very well founded, if the salt waters were not for the most part of such a nature as to afford it themselves at a small expence; that is to say, by their maceration at a temperature below zero. It would be sufficient



cient to leave a certain quantity in the basons exposed to the cold during the winter. It would not indeed be proper to extract the common salt from these waters, after having separated the sulphate of soda, because they would then contain too great a quantity of muriate of magnesia. It would be more advantageous to add sulphate of iron to prepare the sulphates of soda and of magnesia.

With regard to such waters as do not contain any sulphate of magnesia, which is very rare, it would still be of advantage to purify them by these means; because the water must be very bad, and highly charged with deliquescent salts, to require more than fifty or seventy-five pounds of sulphate of soda for thirty quintals of salt obtained by evaporation.

In order to make use of this method, it is proper to be well acquainted with the principles which enter into the composition of the water intended to be purified. It then will be known whether lime alone, or sulphate of soda, or both substances together, ought to be used. Immediate experiment must afterwards determine what quantity ought to be used to insure the most perfect success.

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## X.

*A Report made to the Council of Trade and Mines in Spain, on a new Kind of Wood for Dyeing, named Paraguan. By D. DOMINIQUE GARCIA FERNANDEZ, Inspector of Coinage\*.*

**I**N compliance with the order of the Supreme Council of Commerce and Mines I have undertaken a chemical examination of the wood known at Guiana by the name of Paraguan; which I have carried as far as appeared necessary to ascertain the nature of the wood, and the advantages to be derived from it. In the first place I observed that the bark, the wood properly so called, and the leaves of paraguan afford different colours. The leaves do not deserve notice, because they produce only a false and disagreeable colour. My researches were chiefly directed to the bark, which is the part most deserving of attention. But my remarks upon the bark may also be extended to the wood; for, though this affords a different colour, it presents nearly the same phenomena as the bark itself.

When the bark is boiled in water it affords a coloured extract, which, when exposed to the action of the sulphuric, muriatic or nitric acid, resists their agency for a longer time than brazil or logwood does. The colour may be revived by means of alkalis, after it has been destroyed by combination with acids.

Vinegar, lemon juice and tartar render this colour more brilliant. They give it a fine rose colour; whereas these acids entirely destroy the colours of brazil and logwood.

The fecula of the bark of paraguan fixes and attaches itself to wool, cotton, and silk. The colour is brighter on silk than on wool, and brighter on wool than on cotton.

The same fecula dried is afterwards soluble in alcohol. It communicates to it a tinge similar to that afforded by cochenille.

By mixing alum with a very concentrated decoction of the same substance, a kind of

\* Translated from the Spanish by Cit. Venturi, *Annales de Chimie*, XXIII. 320.

lac is obtained, which is neither so bright nor so beautiful as is obtained from cochénille by the same process.

The same decoction mixed with nut galls afforded me a precipitate of a faint rose colour. The infusions of brazil wood and logwood mixed with the infusion of galls assume a deeper and browner colour. Ours, on the contrary, becomes brighter, and assumes a pale tinge of the rose, or some other similar and delicate colour.

It must be confessed that the colour obtained from paraguatan has not the force of that of cochénille. But it is superior to those of madder, brazil wood, and logwood. For it resists vinegar, lemon juice, and tartar. Soap itself does not destroy it so quickly as it does those of brazil and logwood.

Our bark likewise affords the advantage, that, by employing it in certain doses, and giving a due preparation to silk, we may obtain various shades of rose and poppy colour, which cannot be had by means of carthamus but by difficult processes, long washings, alkaline mixtures, and other embarrassing manipulations.

By examination of the external appearance of a piece of the wood of paraguatan, it appears to me to be the same tree which Francis Correal \* says he observed in the province of Popayan, not far from Guiana. The same author relates, that this tree is different from that of brazil; that the trunk is of the thickness of a man's thigh; that its bark is filled with longitudinal indentations; that the wood and bark are of a fine red; and that the Indians use this wood mixed with a red earth to dye their cotton garments.

The colour obtained from paraguatan does not resist the action of light. No colour can withstand this test. It is enough that our colour withstands it longer than those of brazil and logwood. I must not conceal, that these two trees afford the colouring matter in greater abundance than the paraguatan.

From these facts I consider the paraguatan as one of the most valuable productions which America furnishes to Spain. It may be advantageously employed in the art of dyeing throughout Europe. It is to be wished that enquiry might be made at Popayan, in order that the earth and the wood mentioned by Correal might be procured. It is also desirable that the governor of Guiana should collect all the information which can be procured respecting the paraguatan, and transmit the same to us with other samples of the wood, the leaves and the flowers of this tree, to enable us to determine its species.

The knowledge of this wood begins to be extended. For I have lately received a portion of its bark, and a red matter which an Englishman, named *Milnes*, has obtained from Guiana. It is to be presumed that this matter is the same as is mentioned by Francis Correal in his travels.

#### SCIENTIFIC NEWS.

THE administration of finances and contributions of the French Republic in Italy, having at its disposal a great number of the most valuable pieces of sculpture, painting, and marbles†, arising from confiscations made of (the property of) the enemies of the French

\* Voyage aux Indes Occidentales, p. 420, of the French translation printed in 1722.

† *Marbres*, which I understand to denote tablets, statues, bas-reliefs, and every other work formed of marble. N. Republic,

Republic, being desirous of speedily disposing of all these master-pieces of art, has charged citizen Hubert (who is) attached to the administration of finances in quality of artist, to offer them for sale to individuals, or societies. The lovers of the arts, academies of painting, sculpture, and the sciences, will have a matchless opportunity of acquiring the treasures of art, of which catalogues are in the hands of the whole world, and descriptions in all the publications of travels through Italy.

It will therefore suffice to offer for sale the marbles of the famous Villa Albani, the galleries of the Princes Albani, Braschi, and other monuments, of which a catalogue will be given, with estimates of price made by artists, upon which a considerable abatement will be allowed. Bills (payable) in gold or silver on neutral countries, such as Florence, Venice, Vienna, and even Switzerland, will be accepted. Purchasers belonging to nations at war with France will have passports for transporting whatever they may purchase, whether by land or by sea, and for embarking the same at the ports of Rome and Civita Vecchia. The agents belonging to the same inimical powers may, on their approach to Italy, procure all the necessary passports to come with all security to treat and choose themselves the objects which may suit their wishes, by addressing Citizen Hubert, rue de la Croce, at Rome. Speed and ready money are the best means to be used in this business. Feb. 28th, 1798.

The foregoing advertisement was sent by the therein mentioned Hubert to Mr. Trevor, the British Minister at Turin, and by him transmitted to Sir Richard Worsley in London, where it arrived on the 27th of March. I obtained it of the Right Hon. Sir Joseph Banks, Bart. It is unnecessary for me to make any comment, either political or otherwise, on its contents.

*Account of the Society of Civil Engineers.* [Concluded from p. 48.]

THE same period gave rise also to an association of some gentlemen employed as above-mentioned. They often met accidentally, prior to that union, in the houses of parliament and in courts of justice, each maintaining the propriety of his own designs, without knowing much of each other. It was however proposed by one gentleman to Mr. Smeaton, that such a state of the profession, then crude and in its infancy, was improper; and that it would be well if some sort of occasional meeting in a friendly way was to be held, where they might shake hands together and be personally known to one another. That thus the sharp edges of their minds might be rubbed off, as it were, by a closer communication of ideas, no ways naturally hostile; and might promote the true end of the public business upon which they should happen to meet in the course of their employment, without jostling one another with rudeness too common in the unworthy part of the advocates of the law, whose interest it might be to push them on perhaps too far in discussing points in contest.

Mr. Smeaton immediately perceived the utility of the idea, and at once embraced it. In March 1771, a small meeting was first established on Friday evenings, after the labours of the day were over, at the Queen's Head tavern, Holborn. And from a few members at first, it soon increased, so that in the space of twenty years they amounted to sixty-five and upwards. But of these there were only about fifteen who were real engineers employed in public works, or private undertakings of great magnitude.

Among these we find the names of Yeoman, Smeaton, Grundy, Milne, Nickalls, Jessop, Golborne, Whitworth, Edwards, Joseph Priestley, Major Watson, Boulton, Whitehurst, Rennie, Watt, and some others. The other members were either amateurs, or ingenious workmen and artificers connected with and employed in works of engineering.

This

This association declared itself a society, and a register was kept of the names and numbers of its members. Conversation, argument, and a social communication of ideas and knowledge in the particular walks of each member, were at the same time the amusement and the business of the meetings.

In this manner, sometimes well attended, and at other times not so, as the members were dispersed all over England, the society proceeded until May 1792, when it ceased to exist by mutual consent of the principal members.

Some untoward circumstances in the behaviour of one gentleman towards Mr. Smeaton, gave rise to the disunion. No one was ever more obliged than that gentleman (who is now deceased) to Mr. Smeaton for promoting him in business, and many essential offices in life. The offence given was done away by an apology at the desire of the company, and by the good-nature of Mr. Smeaton; but the remembrance of it had an effect on all present.

Afterwards it was conceived and intended to renew this society in a better and more respectable form. Steps were taken for that purpose, and Mr. Smeaton agreed to be a member. But before the first meeting could be held, he was no more. He died the 28th of October 1792, and their first meeting was in April 1793.

It was conceived it would be a better plan that the members should dine together at a late hour after attendance in parliament, and pass the evening in that species of conversation which provokes the communication of knowledge more readily and rapidly than it can be obtained from private study or books alone.

The first meeting of this new institution, *The Society of Civil Engineers*, was held on the 15th of April 1793, by Mr. Jessop, Mr. Mylne, Mr. Rennie, and Mr. Whitworth.

The constitution was agreed on, and afterwards acceded to by all. That there should be three classes in the society. The first class as ordinary members, to consist of real engineers, actually employed as such in public or in private service. The second class as honorary members, to consist of men of science, and gentlemen of rank and fortune, who had applied their minds to subjects of civil engineering, and who might, for talents and knowledge, have been real engineers, if it had been *their good fortune* to have it in their power to employ others in this profession: and also of those who are employed in other public service, where such and similar kinds of knowledge are necessary. And the third class as honorary members also, to consist of various artists, whose professions and employments are necessary and useful to, as well as connected with, civil engineering.

The meetings are held at the Crown and Anchor in the Strand, every other Friday during the session of parliament. And the list of members are: Of the

First class. Ordinary members. William Jessop, Robert Whitworth, John Rennie, F.R.S. Ed. Robert Mylne, F.R.S. James Watt, F.R.S.—L. and Ed. James Golborne, Sir Thomas H. Page, Knt. F.R.S. John Duncombe, Captain Joseph Huddart, F.R.S. Henry Eastburne, William Chapman, M.R.I.A. James Cockshutt.

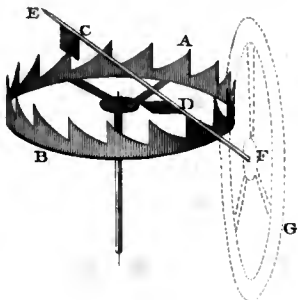
Second class. The Right Hon. Sir Joseph Banks, Bart. P.R.S. Knight of the Order of the Bath, &c. Sir George A. Shuckburgh Evelyn, Bart. F.R.S. General Bentham, Joseph Priestley, Esq. Doctor Charles Hutton, F.R.S. Henry Oxendon, Esq. The Right Hon. the Earl of Morton, F.R.S. John Lloyd, Esq. F.R.S. Rt. Hon. Ch. Greville, Esq. F.R.S.

Third class. William Faden, geographer; Jesse Ramsden, F.R.S. instrument-maker, &c. John Troughton, instrument-maker, &c. John Foulds, mill-wright, &c. Samuel Phillips, engine-maker; Samuel Brooke, printer; John Watté, land-surveyor, &c.

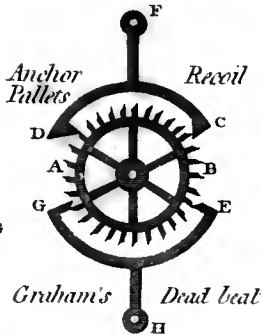
# Escapements.

*Philos. Journal, Vol. II, Pt. III, facing p. 96.*

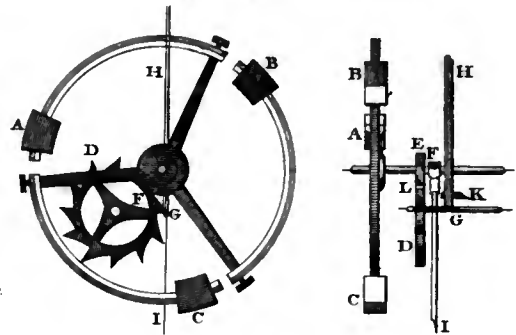
*Fig. 1. Crown Wheel*



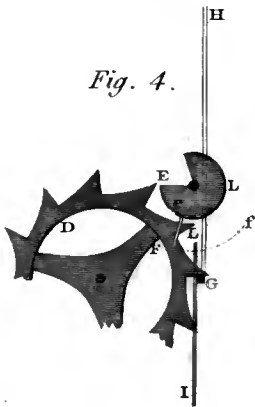
*Fig. 2*



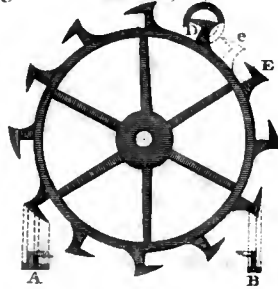
*Fig. 3. Free or detached*



*Fig. 4.*

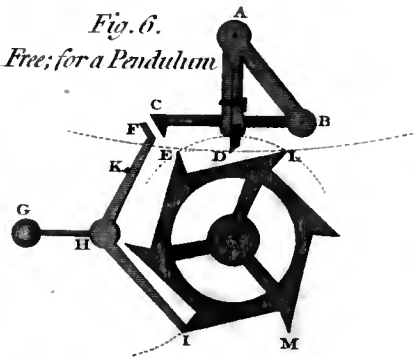


*Fig. 5. Horizontal*

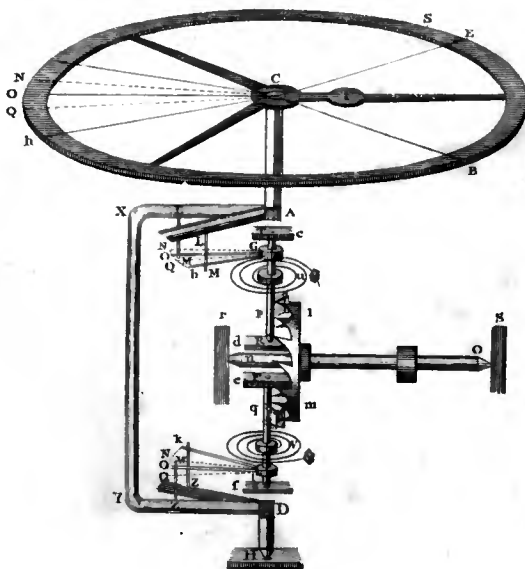


*Fig. 6.*

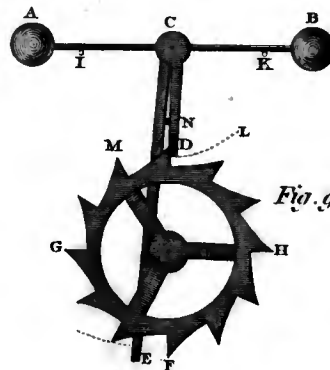
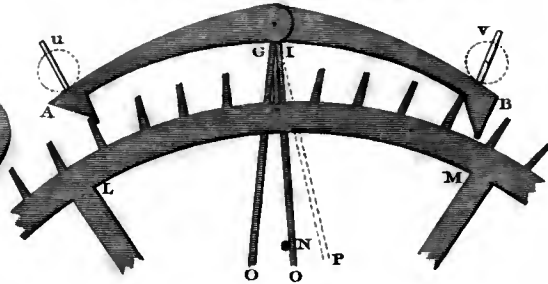
*Free; for a Pendulum*



*Fig. 7. Free or detached by Mudge*



*Fig. 8. Mudge's Esc. for a Pendulum*



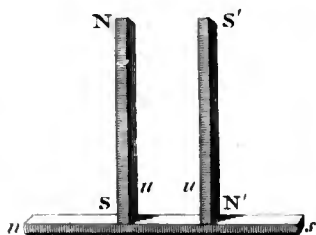
*Fig. 9. W.N.*

*Handwritten text, likely bleed-through from the reverse side of the page.*

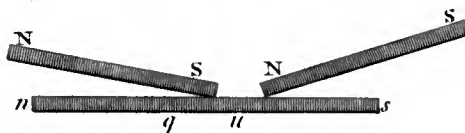


# *M. Coulomb's method of exciting Magnetism.*

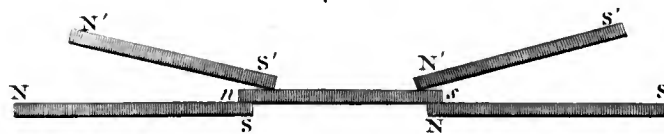
*Fig. 1.*



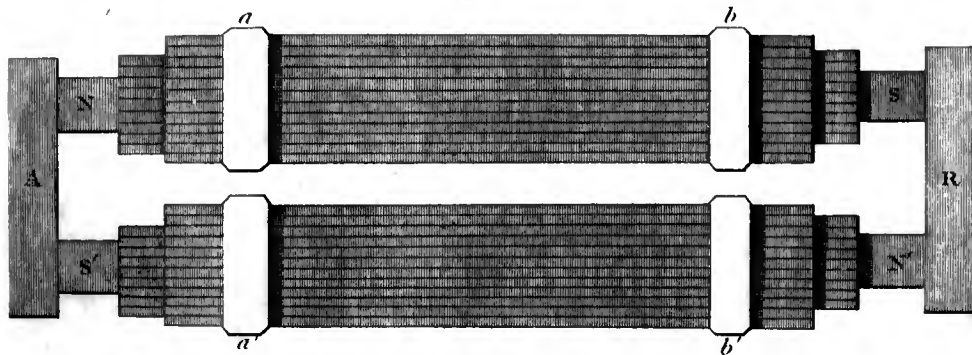
*Fig. 2.*



*Fig. 3.*



*Fig. 4.*







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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JUNE 1798.

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ARTICLE I.

*Abstract of a Memoir on Camphor and the Camphoric Acid, read to the First Class of the National Institute of France. By BOUILLON LA GRANGE\*.*

CAMPHOR, of which the relation, the co-existence, and almost the identity with the volatile oils, have long since fixed the attention of chemists, has appeared to me to deserve a particular examination. I was desirous of confirming the discovery of Mr. Kosegarten on the property of camphor to become converted into a peculiar acid by the action of nitrous acid. The enquiries to which this undertaking has led me, have not only confirmed his discovery, but have likewise presented various facts of immediate interest to the vegetable analysis.

Camphor has long been an object of chemical attention. Several chemists have supposed that it exists in all the aromatic plants, particularly those which contain abundance of volatile oil; but as the camphor obtained from most of these plants constantly retains the odour of the plant which afforded it, there is reason to think, according to the remark of Citizen Fourcroy, that it is combined with the volatile oils of these vegetables. I hope to clear up this fact as soon as I shall procure a sufficient quantity of recent volatile oils; for my experiments have shewn that these afford the greatest quantity.

Proust, who has made experiments relative to this object, has merely indicated a process

\* Annales de Chimie xxiii. 153.

for obtaining the camphor of several volatile oils; but he has neither described its character, nor shewn in what respect it might differ from ordinary camphor.

The experiments we are about to describe may perhaps hereafter afford some light on the nature of this singular substance.

We know that the earths, the saline earthy substances, and the alkalis, have no action upon camphor; but as chemists had hitherto employed the alkalis in combination with carbonic acid, it was essentially necessary to try the action of the pure alkalis.

I shall not here enlarge upon the various experiments I have made. It will be sufficient to remark, that I have obtained no satisfactory result. The camphor is always volatilized. It seems as if a small quantity was dissolved; but it is sensible only to the smell, for I have not been able to ascertain its presence by any other means.

The acids dissolve camphor. The nitric acid is most commonly employed for this purpose, to form a medical preparation known by the improper name of oil of camphor.

I shall not attempt to explain the inconveniencies and injudicious method of preparing this oil. It is known that it is impossible to use it in any vehicle whatever without regenerating the camphor; consequently there remained to medical practitioners no other means but of employing this oil alone, or the camphor united to alcohol. But if we examine the action of this oil on the animal economy, it will be immediately seen that it cannot be exhibited alone.

These considerations engaged me in the search for the means of obtaining camphor in the fluid state, without the addition of an acid. Though the result of my experiments has not been successful, it will nevertheless be useful to science in fixing our notions respecting the nature of this substance.

## SECTION II.

### *Processes for obtaining the Decomposition of Camphor.*

TAKE one part of camphor and six of clay. Let the clay be pulverized in the dry state, and sifted through a fine sieve; on the other hand, the camphor is to be pulverized by means of alcohol, and the whole is then to be mixed together in a mortar. When the mixture is well made, add a small quantity of water, not too much, but enough to admit of forming the matter into a paste, which is to be made into balls of the size of an olive. Place these on a hair sieve to dry in a stove.

When the balls are perfectly dry, let them be introduced into a retort, which is to be placed on a sand bath, with a receiver containing some distilled water, and well luted at the neck. In this disposition of the apparatus the retort must be gradually heated, and kept for several hours in this state, after which the heat is to be raised.

If the heat be not well managed, it sometimes happens that part of the camphor sublimes to the neck of the retort. This however is no reason for discontinuing the process, which must be carried on so long as any oil is seen to come over. The fire is then to be put out; and when the apparatus is cold, the receiver may be unluted, and the sublimed camphor, as well as the residue in the retort, must be taken out. The whole is then to be pounded in a mortar, and the operation recommenced as before, until, even by a considerable degree of heat, neither sublimation nor oil appears.

*A second*

*A second Process.*

The foregoing operation may be conducted in another manner, namely by reducing the two substances into a very fine powder, and introducing them into the retort.

This method, though apparently more speedy, is in fact much less so than the foregoing; for the camphor rises with much more facility, and the distillation must be oftener repeated, which renders the operation much more tedious.

*Third Process.*

Alumine is likewise a medium which may be used with advantage.

The alumine is prepared by means of ammoniac; and it is afterwards washed with distilled water, in order to obtain it as pure as possible. It is then slightly dried.

Take two parts of alumine, or three, when it is wetted, to one part of camphor. The camphor is first to be pulverized, and the alumine afterwards added. The whole may be formed into a paste with water, or otherwise the pulverulent mixture is to be introduced into the retort; but I prefer the first method, as I have already remarked. A receiver containing some distilled water is then to be adjusted. The pneumatic apparatus may also be adapted. Distillation is then to be made by a gentle heat.

By either of these processes a volatile oil of a golden yellow colour comes over into the receiver, and floats at the surface of the water. We shall proceed to describe its characters.

The distilled water placed in the receiver is not only necessary to prevent the loss of oil, which would attach itself to the interior surface, but likewise to absorb a small portion of camphoric acid which is formed during the operation. In fact this water is very aromatic and penetrating: it reddens the tincture of turnsol, and effervesces with the alkaline carbonates. The acid itself, at the end of a certain time, crystallizes on the sides of the vessel.

After the operation is completed, the matter remaining in the retort is of a very deep black colour.

Four ounces of camphor, or 122,284 grammes, may produce

    Volatile oil 12 gros, or 45,856 grammes;

    Carbonic acid 8 gros, or 30,571 grammes;

Carbonated hydrogen gas; carbonic acid; camphoric acid, each in proportions which I have not been able to determine, on account of the volatility of the camphor.

SECTION III.

*An Examination of the Oil obtained from Camphor in the foregoing Experiments.*

THE oils obtained by the different processes have no difference in their qualities.

This oil has an acrid burning taste, and leaves a perception of coolness on the tongue. Its smell is aromatic, resembling that of thyme or rosemary. Its colour golden yellow. When exposed to the open air it partly evaporates, and there remains a brown thick yellow matter of a very sharp taste, but somewhat more bitter, which at length entirely evaporates.

With the caustic alkalis it affords an homogenous mixture, soluble in water, and which has all the characters of soaps made with volatile oils.

Alcohol dissolves it entirely; and if distilled water be added to the solution, the mixture becomes milky, but affords no precipitate.

When brought into contact with the oxygenated muriatic acid, this oil becomes very white, but no precipitate appears.

It therefore differs from that obtained by the nitric acid; because, when mixed with water, with alkalis, or with the oxygenated muriatic acid, camphor is regenerated.

If the muriatic acid gas be passed through the oil of camphor prepared by the nitric acid, its colour immediately becomes of a rose red, and the instant afterwards yellow. This last colour does not vary until towards the end of the operation.

The oil itself does not acquire new properties.

## SECTION IV.

### *Examination of the Matter found in the Retort.*

THE matter which remains in the retort after the operation, is, as we have remarked, of a beautiful black, and, when examined as to quantity, it is found to weigh more than the alumine alone.

It consists of carbone very accurately mixed with the alumine, and accordingly the powder is extremely fine.

In order to separate the carbone from the alumine, and leave it in a state of purity, the alumine must be saturated with an acid. For this purpose the sulphuric acid is to be applied in the diluted state. This acid dissolves the alumine, and leaves the carbone disengaged, which may be separated and dried upon the filtre. The sulphat of alumine may be obtained by concentrating the fluid in crystals, if a small portion of alkali be added.

The intimate combination of the carbone with alumine led me to suspect that this method might probably be advantageous to obtain the carburet of alumine, a preparation hitherto attempted without success, and consequently unknown to chemists.

I therefore thought it necessary to repeat the experiment.

I took an indeterminate quantity of this kind of carburet of alumine, which I placed under a glass vessel filled with distilled water, which vessel was inverted over another vessel filled with water.

At the end of a certain time bubbles of air were disengaged, which rose to the upper part of the vessel, and gradually disappeared. The water in the vessel became depressed, and was replaced by two gases, one of which had all the characters of the carbonic acid gas, and the other was inflammable, and proved to be hydrogen.

This experiment consequently proves, that a small portion of the water was decomposed, its oxygen having seized the carbone, and formed carbonic acid, while the hydrogen became disengaged.

Notwithstanding the accuracy of this experiment, it is insufficient to prove that an intimate combination had taken place; or, in a word, that a true carburet of alumine had been formed. Nevertheless, until experiments shall have established the characters which ought

ought to distinguish an earthy carburet, particularly that of alumine, I shall apply this name to distinguish the result of the new product obtained by the decomposition of camphor.

We cannot have any further doubt respecting the existence of carbone in camphor; but we should render this object much more interesting, if we could prove our assertion by synthesis. Vegetables do not, however, in all cases, permit us to imitate Nature in her processes. It frequently happens, that the best products of Art are but imperfect approximations to natural compounds.

I do not, however, despair of our forming camphor hereafter by means of a volatile oil. Various essays on the contrary allow me to assert that it is possible to effect this.

A few experiments which follow will render this assertion probable.

## SECTION V.

### *Processes for carbonizing a Volatile Oil.*

ONE ounce, or 30,571 grammes of carburet of pot-ash, or pot-ash fused with charcoal, and 4 ounces, or 122,284 grammes of volatile oil of turpentine, or other volatile oil, were taken. These two substances were put into a matras, and the whole was macerated by a gentle heat. The oil acquired a very deep black colour; its smell was changed and became more aromatic. If in these experiments the oil thus changed be poured into distilled water, the fluid becomes white, and a precipitate is formed. If the liquor be left in contact with the atmospheric air, a few drops of oil rise and float at the surface, but the liquor continues white. From a persuasion that this combination had formed a saponule, I saturated the pot-ash with an acid, and no true oil was separated, but it remained suspended in the water, which was become opaque and white.

As the addition of pot-ash rendered my experiment uncertain, I repeated it in another manner.

By the advice of Cit. Guyton, I passed hydrogen gas (obtained by the action of sulphuric acid upon zinc) through oil of turpentine, into which I had put the powder of charcoal. I was careful to keep the oil constantly hot, in order to promote the solution of the coal by the hydrogen gas.

In proportion as the gas was disengaged, the fluid became deeper coloured; and, instead of continuing limpid, it became yellow, and at last brown. If the pneumatological apparatus be adapted to the vessel containing the oil, hydrogen gas is collected together with a small portion of carbonic acid.

The oil by this treatment has therefore taken up a small quantity of carbone and hydrogen, as appears by the difference between its properties and those of pure oil; viz. 1. It is deeper coloured: 2. Its smell is more agreeable, (*suave*): 3. And, when mixed with water, this fluid becomes turbid, and the oil does not separate but by exposure to the air, which leaves the water constantly opaque.

It must be admitted that these trials afford no satisfactory result. But I have no doubt but that a continued set of experiments on this object would afford decided conclusions, or at least some elucidations of utility to the progress of science.

[The remainder in our next.]

## II.

*Instructions concerning the Manufacture of Steel, and its Uses. By VANDERMONDE, MONGE, and BERTHOLLET. Published by Order of the Committee of Public Safety.*

[Concluded from Vol. ii. p. 70.]

*Of Cast Steel.*

**C**AST Steel is produced by fusion of natural steel, particularly that of cementation. The fluid state assumed by the metal in this operation causes the flaws and veins to disappear, and renders the whole mass more uniform.

According to the description which Jars has given us of the manner in which this operation is performed at Sheffield, all kinds of fragments of broken steel are used. The furnace is of the same kind as that of the brass-founder, but much smaller, and supplied with air by a subterraneous communication. At the mouth of the furnace, which is square, and level with the earth, there is an opening against a wall where a chimney is carried up. These furnaces contain only one large crucible nine or ten inches high, and six or seven in diameter. The steel is put into the crucible with a flux, the composition of which is kept secret: and the crucible itself is placed on a round brick standing on the grate. Coak is placed round the crucible, and the upper part of the furnace is filled with it. It is then set on fire; and the upper opening of the furnace is entirely closed by a covering formed of bricks, bound together with iron.

The crucible remains five hours in the furnace before the steel is perfectly fused. Several operations are afterwards made. Moulds formed of two pieces of cast iron, which fit together, and form an octagonal or square cavity, are prepared for casting the steel, which is afterwards hammered out in the same manner as blister-steel, but with less heat and more care, because of the danger of breaking it.

Chalut, officer of artillery, has made experiments on the flux which is best adapted for making cast steel. He is convinced that every kind of glass may be used as a flux, except that which contains lead or arsenic.

The steel being broken into small pieces, is to be covered with the glass. The cover of the crucible must then be put on, and the heat urged to the greatest degree of the brass-founders furnace.

It appears that an extraordinary hardness is sometimes required to be given to cast steel, and that this effect is produced by mixing coaly matter with the flux, to saturate the steel, and give it the highest degree of hardness. It is probable that certain instruments are manufactured by some process of this kind; such as cylinders and laminating rollers, of which the hardness is very great, and the grain perfectly uniform through the whole mass:—but on this subject we have nothing to offer but conjectures.

One of the greatest difficulties we find in this country (France) in the fusion of steel, is to procure good crucibles. The art of pottery, which is truly important in every one of its parts, is that which, of all others, the most strongly solicits our industry.

*Concerning the peculiar Properties of the different Kinds of Steel.*

CAST steel may be considered as the most perfect for all such instruments as require a beautiful polish and uniform hardness. It is exempt from the flaws, dull spots, and veins, which are more or less abundant in other steel. This steel ought to be chosen for such tools as require to be hard and well polished; such as burnishers, watchmakers' tools, lancets, razors, and ornamental steel-work; but it has the inconvenience of not being welded\* with iron, and is brittle. It is more difficult to treat in the fire, and is necessarily much dearer than common steel, because it is obtained by fusion of that very article.

This kind of steel is highly valuable to the arts which promote luxury; but we must direct our attention to the steel which forms the hatchet, the file, the sabre, and the hammers of gun-locks.

Steel of cementation sometimes approaches to cast steel in purity, when excellent iron has been used, and particularly when it has been well forged; but in general it presents some threads and veins, is less uniform in its texture, and does not become equally hard. This steel may be used for most articles of cutlery, edge-tools, laminating rollers, hammers, small springs, files, &c. It is not easily welded to iron.

This steel is not only used for a great variety of objects, but it may be used in different proportions in the stuffs (*Etoffes*†) which are used when a material is required which shall be less subject to break, as in the fabrication of large springs, scythes, sabres, &c.

The material called stuff (*Etoffe*) is a compound of iron and steel, several plates of which are forged and welded together, to afford a substance partaking of the properties of both. The iron seems to give flexibility to the steel, while this communicates its hardness and elasticity to the iron. It appears that the art of uniting plates of steel and iron, and of conjoining them together, is the principal requisite in the perfection of Damascus steel.

Natural steel is much less uniform than the steel of cementation. When it is polished, its surface is usually rendered unequal by scabrous places, flaws or veins. It is easy to discover veins of iron with the point of the graver, so that it may be considered as a natural composition; whence it is found, that cutting tools made of this steel are less subject to fail in the edge, and that it bears heating a second time very well; having, as the workmen say, more body, and being more easy to forge.

In general, according to our eminent cutler Perret, it is adviseable to use the English cemented steel for fine and delicate work, and sometimes even cast steel; but for strong and heavy work, the preference is to be given to German steel, which is the natural steel, because it has more body and tenacity.

Upon the whole, it appears to us that the steel of cementation is most worthy of being attended to; because it is easy, at a small charge, to establish manufactories on a confined scale, and with the desired expedition; and because this steel may, without difficulty, be used in any kind of work. Fellow citizens, let us convey to our forges those expensive balustrades and railing, which have nothing to defend; and if we find them to possess the qualities of good iron, let us convert them into steel.

\* For the method of welding cast-steel to iron, see *Philos. Jour.* L. 575. N.

† I am not acquainted with the appropriate English term. N.

*The Method of trying Steel.*

THE different properties belonging to each kind of steel render the proofs of their quality difficult even to skilful workmen. Cast steel will be thought of a very bad quality by him who is not in the habit of working it; as will likewise the steel of cementation by the workman who is commonly employed upon German steel. The grain of the fracture is a deceitful indication, because its fineness varies with its hardness; nevertheless good steel ought always to exhibit an even grain. Cast steel ought to take a good polish, and not be too brittle: steel of cementation ought to afford gravers capable of resisting percussion without notching or turning in the edge; natural steel ought to weld with ease to iron, and make good edge-tools.

There are circumstances in which it is of advantage to ascertain whether a manufactured article be formed of steel or iron without injuring it. The means of doing this is of the more consequence to us, because certain unprincipled contractors have delivered sabres with blades of pure iron, to which a slight degree of rigidity had been given in the fabrication; a circumstance which has induced the Committee of Public Safety to publish the directions we shall here transcribe, and to oblige its agents, entrusted with the reception of every kind of arms of steel, to submit them to this proof.

“If a drop of nitrous acid be placed upon the surface of polished iron, and, after having left it two minutes, water be thrown thereon, this last fluid will carry off the acid with all its contents, and there will remain merely a white spot, or the colour of iron newly cleaned.

“If the same operation be made on a plate of polished steel, the acid likewise seizes the iron, but does not act on the coaly part. This last falls down during the solution, and forms a black spot, which the projection of the water does not remove, and which even remains for a considerable time, on account of its adhesion.

“In order to succeed in this operation, a diluted acid must be used, because the coaly principle does not adhere to the surface, but in proportion to the slowness of the solution, and the weakness of the effervescence.

“If pure or rectified nitrous acid be not at hand, the aquafortis of commerce may be used, taking care to dilute it in a certain degree.

“The drop of acid must be conveyed with a stick of glass, or other material, which is not itself attacked, and cannot afford any thing capable of changing the result.

“The smallest drop suffices. It ought rather to be spread out, than kept together, that it may mark a larger surface. The stopper of a very small bottle, in which the acid is kept, answers this purpose very well.

“After having made this proof two or three times on iron and steel compared with each other, the operator will be enabled to speak with certainty respecting the difference of the metals.”

Artists have long since availed themselves of a method of this kind to distinguish Damascus blades. These blades, as we have observed, are composed of the steel and iron intimately mixed. By this proof, says Perret, they present serpentine veins, some of a whitish grey, others of a deep grey, and others blackish, which are known by the name of the Flowers of Damascus.

We



We have remarked that cast iron, sufficiently abounding with charcoal, assumes the appearance of the white crude iron when it is cast in plates or too suddenly cooled. To ascertain its nature, nothing more is necessary than to clean its surface; and by the more or less grey or dark colour of the spot produced by nitrous acid, a judgment may be formed of its quality.

VANDERMONDE, MONGE, BERTHOLLET.

*Annotations upon the Report concerning Steel.*

1. NOTWITHSTANDING the great light which has been afforded by the labours of Rinman, Bergman, Vauquelin, and other chemists, together with the learned authors of the foregoing paper, we have still much to learn respecting the causes of the several qualities of steel to be found in the market. Much of this information might probably be obtained by diligent enquiry, and the usual methods of examination. It is yet to be shewn by experiment, whether pure iron united with carbon constitutes the best steel; or whether any additional ingredient, such as phosphorus, manganese, or other metallic matter, may not be of advantage. If the former position be true, the best steel-iron will be that which is the purest; and the best cement will be the simplest coaly matter. On the contrary, if the latter suppositions should be well founded, the goodness of our steel will depend, not only upon the component parts of the bar iron, but likewise on the adaptation of the cement proper for its conversion. Mr. Duhamel, in the *Encyclopédie*, article *Acier*, p. 462, found that the charcoal which had been used in making steel was not fit to be used again, by reason of the slowness of its operation. In this case it seems probable that, though enough of carbone remained, yet some more volatile ingredient had been expended in the first process. Our authors have well explained the advantage of the iron being well forged and found throughout, previous to its exposure to the cementing process.

2. The grey crude iron may be considered as iron saturated with as much plumbago or carburet as it can hold in solution at a very elevated temperature. When it is suffered to cool slowly, as in soft casting, the plumbago appears to separate by hasty crystallization through the whole mass, as may frequently be observed in its fracture, and as has been shewn by plunging a cold bar of iron into the fused metal, and withdrawing it covered with the carburet which precipitates upon it. When this iron is poured into a cold mould of metal, or suffered to run to a great distance from the aperture of reception, in sand, or otherwise if a bar of this metal be heated to whiteness, and plunged in water, it becomes very hard; more so than steel tools of any description are usually made, and is very white, and closer grained in its fracture. The hardness must depend upon circumstances in its aggregation, to explain which we possess no data; but I apprehend the whiteness to be an evident consequence of the union between the iron and the plumbago continuing to be nearly as intimate as it was at the white heat. It is very probable that, in all cases of the hardening of other steels containing less carbone, a similar effect may take place.

3. The laminating rollers, concerning which our authors offer nothing but conjecture, are made of this iron cast in sand, or metallic moulds of considerable thickness. I am informed by founders who make them, that the external hard part does not penetrate to a greater depth than about three quarters of an inch; and that the hardness is not greater than that of a good graver. The process of turning them in the lathe, in fact, shews this.

For they are turned by an extremely slow motion, with a tool selected from the general stock in the work-shop, because the greater number even of good gravers will not cut it.

4. Those who forge steel know that it is very easily degraded in the fire. If a small piece, for example, half a cubic inch, of grey crude iron be put into a common fire, and kept red-hot for about half an hour, and at the expiration of that time the heat be suddenly raised to whiteness by the bellows; the internal or steely part will break its way through the external crust, which is converted into common iron, and rendered much less fusible. This crust, or hollow shell, may then be taken out of the fire. All steel becomes degraded in the same manner, even by very careful heating. Cast-steel, drawn into small bars, exhibits cloudy lines and veins on its surface when tried by an acid, which no doubt have been produced during the heating and forging.

5. Cast-steel being made out of broken tools of every kind, cannot of itself possess a larger dose of plumbago than the average quantity contained in those steels. But the English cast-steel is more fusible and more tender under the hammer than German steel, or the steel of cementation; which circumstances appear to indicate that it contains more plumbago: and the truth of this induction is confirmed by its exhibiting a much darker spot than other steels, when tried by an acid. Chalut did not therefore make this kind of steel when he used glass only for his flux. It cannot be doubted but that the flux of our manufacturers must contain charcoal, at least. If it be animal coal, which is most probable, it will also contain phosphorus; an ingredient to which the superiority of this coal, in case-hardening, is probably owing.

6. Tenacity and hardness are very frequently considered as if they were one and the same quality with regard to implements and tools, though they are certainly very distinct properties. Tenacity is the opposite to friability or brittleness; hardness is the opposite to softness. It is probable that iron is more tenacious than steel, and it is certain that soft steel is more tenacious than hard. Where tenacity and no considerable degree of hardness are wanting, as in springs, the inferior steels, or the compounds of steel and iron, will afford a more safe, and consequently in many instances more useful article; but where hardness and tenacity are both required, the leading quality of the steel must be its uniformity. Cast steel is preferred in England to every other kind, not only for polished steel-work and the best cutting tools, but likewise for cold chisels, and the hard gravers for turners in metal; in both which last I have observed, by considerable experience, that the common opinion is well founded.

### III.

*An Enquiry concerning the Source of the Heat which is excited by Friction\*. By BENJAMIN, COUNT of RUMFORD, F.R.S. M.R.I.A.*

**I**T frequently happens that, in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of nature; and

\* From the Philosophical Transactions, 1798.—Read January 25, 1798.

very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life, has oftener led, as it were by accident, or, in the playful excursions of the imagination put into action by contemplating the most common appearances, to useful doubts and serviceable schemes for investigation and improvement, than all the more intense meditations of philosophers in the hours expressly set apart for study.

It was by accident that I was led to make the experiments of which I am about to give an account; and though they are not, perhaps, of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in superintending the boring of cannon in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat which a brass gun acquires in a short time in being bored, and with the more intense heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

The more I meditated on these phenomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of heat; and to enable us to form some reasonable conjectures respecting the existence or non-existence of an *igneous fluid*: a subject on which the opinions of philosophers have in all ages been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the heat actually produced in the mechanical operation above mentioned?

Is it furnished by the metallic chips, which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent heat and of caloric, the *capacity for heat* of the parts of the metal so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the heat produced.

But no such change had taken place; for I found, upon taking equal quantities by weight of these chips, and of thin slips of the same block of metal, separated by means of a fine saw, and putting them at the same temperature (that of boiling water) into equal quantities of cold water (that is to say, at the temperature of  $59\frac{1}{2}^{\circ}$  F.), the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion into which the slips of metal were put.

This experiment being repeated several times, the results were always so nearly the same,

that I could not determine whether any, or what change had been produced in the metal, in regard to its capacity for heat, by being reduced to chips by the borer\*.

From hence it is evident, that the heat produced could not possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment.

Taking a cannon (a brass six pounder) cast solid, and rough as it came from the foundry, (see fig. 1, plate V) and fixing it (horizontally) in the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see fig. 2.) I caused its extremity to be cut off, and, by turning down the metal in that part, a solid cylinder was formed,  $7\frac{3}{4}$  inches in diameter, and  $9\frac{8}{10}$  inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon,) by a small cylindrical neck, only  $2\frac{1}{2}$  inches in diameter, and  $3\frac{8}{10}$  inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length, (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in fig. 2; as also in fig. 3, where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small round hole, (see d, e, fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

\* As these experiments are important, it may, perhaps, be agreeable to the Society to be made acquainted with them in their details. One of them was as follows:

To 4590 grains of water, at the temperature of  $59\frac{1}{2}^{\circ}$  F. (an allowance or compensation, reckoned in water, for the capacity for heat of the containing cylindrical tin vessel, being included) were added 1016 $\frac{1}{2}$  grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of  $210^{\circ}$  F. When they had remained together 1 minute, and had been well stirred about by means of a small rod of light wood, the heat of the mixture was found to be  $= 63^{\circ}$ .

From this experiment, the specific heat of the metal, calculated according to the rule given by Dr. Crawford, turns out to be  $= 0.1100$ , that of water being  $= 1.000$ .

An experiment was afterwards made with the metallic chips, as follows:

To the same quantity of water as was used in the experiment above mentioned, at the same temperature, (viz.  $59\frac{1}{2}^{\circ}$ ), and in the same cylindrical tin vessel, were now put 1016 $\frac{1}{2}$  grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature ( $210^{\circ}$ .) The heat of the mixture, at the end of 1 minute, was just  $63^{\circ}$ , as before; consequently the specific heat of these metallic chips was  $= 0.1100$ . Each of the above experiments was repeated three times, and always with nearly the same results.

The

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were  $385\frac{3}{4}$  cubic inches, English measure, and it weighed 113.13 lb. avoirdupois, as I found on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected \*.

*Experiment No. 1.*

THIS experiment was made in order to ascertain how much heat was actually generated by friction, when a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder, that the pressure against it was equal to the weight of about 10,000 lb. avoirdupois, the cylinder was turned round on its axis (by the force of horses) at the rate of about 32 times in a minute.

This machinery, as it was put together for the experiment, is represented by fig. 2. W is a strong horizontal iron bar connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel,  $0.63$  of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely,  $3\frac{1}{4}$  inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (m), which kept it in its place. The area of the surface, by which its end was in contact with the bottom of the bore of the cylinder, was nearly  $2\frac{1}{4}$  inches. This borer, which is distinguished by the letter n, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just  $60^{\circ}$  F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was  $\frac{1\frac{1}{2}}{100}$  of an inch in diameter, and  $3\frac{1}{4}$  inches in length, was introduced into the hole made to receive it, in the side of the cylinder; when the mercury rose instantly to  $130^{\circ}$ .

Though the heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended

\* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal about two feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorn kopf*, (the head of the cannon to be thrown away,) and which is represented in fig. 1.

This additional projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle, where, without this precaution, the metal would be apt to be porous or full of honeycombs.

from the axis of the cylinder to near its surface, the heat indicated by it could not be very different from that of the mean temperature of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it. Thus,

At the end of 4 minutes, the heat, as shown by the thermometer, was 126°

After 5 minutes, always reckoning from the first observation, 125

At the end of 7 minutes, — — — — 123

12 — — — — 120

14 — — — — 119

16 — — — — 118

20 — — — — 116

24 — — — — 115

28 — — — — 114

31 — — — — 113

34 — — — — 112

37½ — — — — 111

And when 41 minutes had elapsed — — — — 110

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment, and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 113 lb. of gun-metal at least 70 degrees of Fahrenheit's thermometer, and which, of course, would have been capable of melting 6½ lb. of ice, or of causing near 5 lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of a change of its capacity for heat?

As the weight of this dust (837 grains Troy) amounted to no more than  $\frac{1}{348}$ th part of that of the cylinder, it must have given off 948 degrees to raise the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of heat to have produced that of the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, is not sensibly changed by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces by means of a borer that is less sharp.

If the heat, or any considerable part of it, were produced in consequence of a change in the capacity for heat of a part of the metal of the cylinder, as such change could only be superficial, the cylinder would by degrees be exhausted, or the quantities of heat produced, in any given short space of time, would be found to diminish gradually in successive experiments. To find out if this really happened or not, I repeated the last-mentioned experiment several times with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of heat actually given off.

Finding so much reason to conclude, that the heat generated in these experiments, or excited, as I would rather choose to express it, was not furnished at the expence of the latent heat or combined caloric of the metal, I pushed my enquiries a step farther, and endeavoured to find out whether the air did or did not contribute any thing in the generation of it.

#### Experiment No. 2.

AS the bore of the cylinder was cylindrical, and as the iron bar (m), to the end of which the blunt steel borer was fixed, was square, the air had free access to the inside of the bore, and even to the bottom of it where the friction took place by which the heat was excited.

As neither the metallic chips produced in the ordinary course of the operation of boring brass cannon, nor the finer scaly particles produced in the last-mentioned experiments by the friction of the blunt borer, showed any signs of calcination, I did not see how the air could possibly have been the cause of the heat that was produced; but, in an investigation of this kind, I thought that no pains should be spared to clear away the rubbish, and leave the subject as naked and open to inspection as possible.

In order by one decisive experiment to determine whether the air of the atmosphere had any part or not in the generation of the heat, I contrived to repeat the experiment under circumstances in which *it was evidently impossible for it to produce any effect whatever*. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the access of the external air to the inside of the bore of the cylinder was effectually prevented. In fig. 3. this piston (p) is seen in its place: it is likewise shown in fig. 7. and 8.

I did not find, however, by this experiment, that the exclusion of the air diminished in the smallest degree the quantity of heat excited by the friction.

There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was however desirous to remove. The piston which closed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety by means of collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it occasioned, by the friction of the piston? And as the external air had free access to the extremity of the bore where it came in contact with the piston, is it not possible that this air might have had some share in the generation of the heat produced?

*Experiment*

*Experiment No. 3.*

A QUADRANGULAR oblong deal-box, (see fig. 4.) water-tight,  $11\frac{1}{2}$  English inches long,  $9\frac{1}{8}$  inches wide, and  $9\frac{1}{8}$  inches deep, (measured in the clear,) being provided, with holes or slits in the middle of each of its ends just large enough to receive the one, the square iron rod to the end of which the blunt steel borer was fastened, the other, the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above by a wooden cover or lid moving on hinges) was put into its place; that is to say, when by means of the two vertical openings or slits in its two ends, (the upper parts of which openings were occasionally closed by means of narrow pieces of wood sliding in vertical grooves,) the box (*g, b, i, k*, fig. 3.) was fixed to the machinery in such a manner that its bottom (*i, k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident from the description, that the hollow metallic cylinder would occupy the middle of the box without touching it on either side, (as it is represented in fig. 3.) and that on pouring water into the box and filling it to the brim, the cylinder would be completely covered and surrounded on every side by that fluid. And farther, as the box was held fast by the strong square iron rod (*m*), which passed in a square hole in the centre of one of its ends, (*a*, fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the experiment I had projected in the following manner:

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel-borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being closed at the same time by means of the circular piston, through the centre of which the iron bar passed.

This being done, the box was put in its place; and the joinings of the iron rod and of the neck of the cylinder, with the two ends of the box, having been made water-tight by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of  $60^{\circ}$ ) and the machine was put in motion.

The result of this beautiful experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had in contriving and managing the complicated machinery used in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water and touching the outside of the cylinder, that heat had been generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm.

At the end of one hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. avoirdupois, or  $2\frac{1}{4}$  wine gallons,) that its temperature had been raised no less than 47 degrees, being now  $107^{\circ}$  of Fahrenheit's scale.

When



When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was found to be raised to 178°.

At 2 hours 20 minutes, it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED.

It would be difficult to describe the surprise and astonishment expressed in the countenances of the bystanders, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could justly be considered as surprising in this event, yet I acknowledge, fairly, that it afforded me a degree of childish pleasure, which, were I ambitious of the reputation of a grave *philosopher*, I ought most certainly rather to hide than discover.

The quantity of heat excited and accumulated in this experiment was very considerable; for, not only the water in the box, but also the box itself, (which weighed 15½ lb.) and the hollow metallic cylinder, and that part of the iron bar which, being situated within the cavity of the box, was immersed in the water, were heated through 150 degrees of Fahrenheit's scale; viz. from 60 degrees (which was the temperature of the water and of the machinery at the beginning of the experiment) to 210 degrees, the heat of boiling water at Munich.

The total quantity of heat generated may be estimated with some considerable degree of precision as follows:

Of the heat excited there appears to have been actually accumulated,

In the water contained in the wooden box, 18½ lb. avoirdupois, heated 150 degrees; namely, from 60° to 210° F.

In 113.13 lb. of gun metal (the hollow cylinder) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated 12½ lb. of water the same number of degrees.

In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for heat to 1.21 lb. of water.

N. B. No estimate is here made of the heat accumulated in the wooden box, nor of that dispersed during the experiment.

Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours 30 minutes, might have been heated 180 degrees, or made to boil.

Quantity of ice-cold water which, with the given quantity of heat, might have been heated to 180°, or made to boil.

In Avoirdupois weight,  
lb.

15.2

10.37

1.01

---

26.58

From the knowledge of the *quantity* of heat actually produced in the foregoing experiment, and of the *time* in which it was generated, we are enabled to ascertain the *velocity of its production*, and to determine how large a fire must have been made, or how much fuel

must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of heat in the same time.

In one of Dr. Crawford's experiments, (see his *Treatise on Heat*, p. 321) 37lb. 7oz. troy = 181920 grains of water were heated  $2\frac{1}{8}$  degrees of Fahrenheit's thermometer, with the heat generated in the combustion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or  $14693\frac{1}{8}$  grains of water heated 1 degree, or  $14693\frac{1}{8} = 81.631$  grains, heated 180 degrees with the heat generated in the combustion of 1 grain of wax.

The quantity of ice-cold water, which might have been heated 180 degrees with the heat generated by friction in the before-mentioned experiment, was found to be 26.58lb. = 188060 grains; and, as 81.631 grains of ice-cold water require the heat generated in the combustion of 1 grain of wax to heat it 180 degrees, the former quantity of ice-cold water, namely, 188060 grains, would require the combustion of no less than 2303.8 grains (=  $4\frac{8}{10}$  oz. troy) of wax to heat it 180 degrees.

As the experiment (No. 3.) in which the given quantity of heat was generated by friction lasted 2 hours 30 minutes = 150 minutes; it is necessary, for the purpose of ascertaining how many wax candles of any given size must burn together, in order that, in the combustion of them, the given quantity of heat may be generated in the given time, and consequently *with the same celerity* as that with which the heat was generated by friction in the experiment, that the size of the candles should be determined, and the quantity of wax consumed in a given time by each candle in burning equably should be known.

Now, I found by an experiment made on purpose to finish these computations, that when a good wax candle of a moderate size,  $\frac{3}{4}$  of an inch in diameter, burns with a clear flame, just 49 grains of wax are consumed in 6 minutes. Hence it appears, that 245 grains of wax would be consumed in 30 minutes; and that to burn the quantity of wax (= 2303.8 grains) necessary to produce the quantity of heat actually obtained by friction, in the experiment in question, and in the given time (150 minutes), nine candles burning at once would not be sufficient; for 9 multiplied into 245 (the number of grains consumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax necessary to be burned, in order to procure the given quantity of heat, was found to be 2303.8 grains.

From the result of these computations it appears, that the quantity of heat produced equably, or in a continual stream (if I may use that expression), by the friction of the blunt steel bore against the bottom of the hollow metallic cylinder, in the experiment under consideration, was greater than that produced equably in the combustion of 9 wax candles, each  $\frac{3}{4}$  of an inch in diameter, all burning together, or at the same time, with clear bright flames.

As the machinery used in this experiment could easily be carried round by the force of one horse (though, to render the work lighter, two horses were actually employed in doing it), these computations show farther how large a quantity of heat might be produced by proper mechanical contrivance, merely by the strength of a horse, without either fire, light, combustion, or chemical decomposition; and, in a case of necessity, the heat thus produced might be used in cooking victuals.

But

But no circumstances can be imagined in which this method of procuring heat would not be disadvantageous; for more heat might be obtained by using the fodder necessary for the support of a horse as fuel.

As soon as the last-mentioned experiment (No. 3) was finished, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the scaly metallic powder, which had been produced by the friction of the borer against the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about  $8\frac{2}{3}$  oz. troy.

As this quantity was produced in  $2\frac{1}{2}$  hours, this gives 830 grains for the quantity produced in *half an hour*.

In the first experiment, which lasted only half an hour, the quantity produced was 837 grains.

In the experiment No. 1, the quantity of heat generated in *half an hour* was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180 degrees, or cause it to boil.

According to the result of the experiment No. 3, the heat generated in *half an hour* would have caused 5.31lb. of ice-cold water to boil. But in this last-mentioned experiment, the heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two experiments.

It remains for me to give an account of one more experiment which was made with this apparatus. I found, by the experiment No. 1, how much heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the experiment No. 2, I found that the quantities of heat generated were not sensibly diminished when the free access of the air was prevented; and, by the result of No. 3, it appeared that the generation of heat was not prevented or retarded by keeping the apparatus immersed in water. But as, in this last-mentioned experiment, the water, though it surrounded the hollow metallic cylinder on every side, externally, was not suffered to enter the cavity of the bore (being prevented by the piston), and consequently did not come into contact with the metallic surfaces where the heat was generated; to see what effects would be produced by giving the water free access to these surfaces, I now made the

#### *Experiment No. 4.*

THE piston which closed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the result of this experiment that renders it necessary for me to be very particular in my account of it. Heat was generated as in the former experiments, and to all appearance quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the last. The only circumstance that surprised me was, to find how little difference was occasioned in the noise made by the borer in rubbing against the bottom of the bore of the cylinder, by filling the bore with water. This noise, which was very grating to the ear, and sometimes almost insupportable, was, as nearly as I could judge of it, quite as loud and as disagreeable

agreeable when the surfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers, namely, What is heat?—Is there any such thing as an *igneous fluid*?—Is there any thing that can with propriety be called caloric?

We have seen that a very considerable quantity of heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux *in all directions*, without interruption or intermission, and without any signs of diminution or exhaustion.

From whence came the heat which was continually given off in this manner in the foregoing experiments? Was it furnished by the small particles of metal detached from the larger solid masses on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for in three of these experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident; *first*, because this water was continually *receiving heat* from the machinery, and could not at the same time be *giving to* and *receiving heat from* the same body; and, *secondly*, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place (which indeed could not reasonably have been expected), one of its compound elastic fluids (most probably inflammable air) must at the same time have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but though I frequently examined the water to see if any air bubbles rose up through it, and had even made preparations for catching them in order to examine them if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process going on in the water.

Is it possible the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before mentioned; for heat was continually going off or *out of the machinery*, by both these last passages, during the whole time the experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction in these experiments appeared evidently to be inexhaustible.

It is hardly necessary to add, that any thing which any *insulated* body or system of bodies can continue to furnish *without limitation*, cannot possibly be a *material substance*; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing capable of being excited and communicated in the manner the heat was excited and communicated in these experiments, except it be motion.

I am very far from pretending to know how, or by what means or mechanical contrivance, that particular kind of motion in bodies which has been supposed to constitute heat

is excited, continued, and propagated; and I shall not presume to trouble the Society with mere conjectures, particularly on a subject which, during so many thousand years, the most enlightened philosophers have endeavoured but in vain to comprehend.

But although the mechanism of heat should in fact be one of those mysteries of nature which are beyond the reach of human intelligence, this ought by no means to discourage us, or even lessen our ardour, in our attempts to investigate the laws of its operations. How far can we advance in any of the paths which science has opened to us, before we find ourselves enveloped in those thick mists which on every side bound the horizon of the human intellect? But how ample and how interesting is the field that is given us to explore!

Nobody, surely, in his sober senses has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal NEWTON enabled to make, merely by the investigation of the laws of its action!

The effects produced in the world by the agency of heat are probably *just as extensive*, and quite as important, as those which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are in all cases determined by laws equally immutable.

Before I finish this paper I would beg leave to observe, that although, in treating the subject I have endeavoured to investigate, I have made no mention of the names of those who have gone over the same ground before me, nor of the success of their labours; this omission has not been owing to any want of respect for my predecessors, but was merely to avoid prolixity, and to be more at liberty to pursue without interruption the natural train of my own ideas.

*Description of the Figures. Plate V.*

FIG. 1. shows the cannon used in the foregoing experiments, in the state it was when it came from the foundry.

Fig. 2. shows the machinery used in the experiments No. 1. and No. 2. The cannon is seen fixed in the machine used for boring cannon. *W* is a strong iron bar (which, to save room in the drawing, is represented as broken off); which bar being united with machinery (not expressed in the figure) that is carried round by horses, causes the cannon to turn round its axis.

*m* is a strong iron bar, to the end of which the blunt borer is fixed, which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating heat by friction.

Fig. 3. shows on an enlarged scale the same hollow cylinder that is represented on a smaller scale in the foregoing figure. It is here seen connected with the wooden box (*g, h, i, k*) used in the experiments No. 3. and No. 4. when this hollow cylinder was immersed in water.

*p*, which is marked by dotted lines, is the piston which closed the end of the bore of the cylinder.

*n* is the blunt borer seen sideways.

*d, e*, is the small hole by which the thermometer was introduced, that was used for ascertaining the heat of the cylinder. To save room in the drawing, the cannon is represented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is represented broken off at *m*.

Fig.

Fig. 4. is a perspective view of the wooden box, a section of which is seen in the foregoing figure. (See *g, h, i, k*, fig. 3.)

Fig. 5. and 6. represent the blunt borer *n* joined to the iron bar *m* to which it was fastened.

Fig. 7. and 8. represent the same borer with its iron bar, together with the piston, which, in the experiments No. 2. and No. 3. was used to close the mouth of the hollow cylinder.

## IV.

*An Attempt to discover the Genuineness and Purity of Drugs and Medical Preparations.*

*By FREDERICK ACCUM.*

To Mr. NICHOLSON.

SIR,

OF all the possible frauds hitherto practised by mercenary traders, there is none more intimately connected with the welfare of mankind than the art of making or counterfeiting drugs and adulterating genuine medicines.

This unprincipled art is brought to such perfection, that spurious articles are every where to be found in the market, made up with such dexterity, that not only the mere merchant and drug broker, but even the man of skill, is sometimes deceived.

The influence of this practice has indeed been observed by medical men of science, and the rulers of this art have thought it their duty to appoint a court of examiners to investigate the goodness of drugs and medicines in the different chemists' and apothecaries' shops in this metropolis.

How far this great work is accomplished, or to what extent the whole undertaking does either honour and credit to the sagacity of these learned, liberal, and upright examiners, or affords justice to the public, I shall leave to the determination of every individual who has once only been honoured with their visit.

But as moral duty should stimulate every individual to offer all the assistance in his power to suppress or check such detestable practices, I have ventured to recommend that method which, if judiciously executed, will contribute in no small degree to the public good in this respect. If these sketches should tend in any measure to put the unwary on their guard, I shall think myself amply rewarded, and shall not fail to extend them from time to time, and lay them before the public with some confidence.

No. 17, Hay-market,

May 1st, 1798.

FRED. ACCUM.

## EXAMINATION OF SALINE SUBSTANCES.

*Sulphuric Acid.*

THE vitriolic acid kept in our shops under the name of oil of vitriol, usually contains not only lead in solution, but likewise iron, copper, and sulphate of potash.

In order to discover these admixtures, a little of the acid must be diluted with distilled water and saturated with pure vegetable alkali, as by this means the copper, iron, and

lead become precipitated in the form of a more or less dark-coloured powder, which is to be separated by the filter, and washed with distilled water.

To analyse this precipitate, it is first treated with pure ammoniac, which soon acquires a blue colour if copper be present.—The blue supernatant fluid is to be separated, the remainder washed in distilled water, and then dissolved in pure muriatic acid.

In order to ascertain the presence of lead, the foregoing solution is mixed with an equal quantity of water impregnated with sulphurated hydrogen gas; and if a dark brown or black precipitate ensues, it is a sure sign that lead is present.

Iron that may happen to be in solution is exhibited by the admixture of a few drops of pure prussiate of potash or tincture of gall-nuts, which in the first case will produce a blue, and in the second a black precipitate.

Sulphate of potash, or the vegetable alkali, united to sulphuric acid is more difficult to discover, as for this purpose a certain quantity of the acid is to be perfectly saturated with carbonate of potash, or carbonate of ammoniac; to this solution a little tartareous acid is to be added, which by its superior affinity will decompose the vitriolated tartar, and indicate the vegetable alkali under the form of tartarite of potash separated from the fluid. If the quantity in the acid be not very small, as is seldom the case, it may also be discovered by the admixture of strong ardent spirit; for by this means a quantity of sulphate of potash will be separated in the form of a white powdery precipitate.

The specific gravity of sulphuric acid, according to our pharmacopœia, should be to that of distilled water as 185 to 100.

It is void of smell—perfectly colourless—and boils at 546° of Fahrenheit's thermometer.

Equal parts of vitriolic acid and water, hastily mixed together, should produce a heat nearly equal to that of boiling water.

#### *Nitrous Acid\*.*

THE nitrous acid of commerce (or double aqua fortis as it is commonly called) always contains either sulphuric or muriatic acid, and often both.

The vitriolic acid may be discovered by the addition of a few drops of a solution of nitrate of barytes, or acetite of lead, which form insoluble compounds and fall down. The muriatic acid is manifested by the addition of a solution of nitrate of silver. In this case muriate of silver is formed and precipitated.

Its specific gravity should be to that of distilled water, as 155 to 100.

If it is of an orange yellow colour, and emits a vast quantity of elastic fluid, known by the name of nitrous gas, which forms yellow fumes in our atmosphere. Mixed with water it develops a considerable degree of heat †.

\*The instruction for preparing nitrous acid according to our pharmacopœia is erroneous, as the proportion of sulphuric acid prescribed is much too small: it consequently leaves a considerable quantity of nitrate of potash behind undecomposed. A.

† Pure nitrous acid is deprived of a portion of nitrous gas by the application of heat. It is then colourless, and constitutes nitric acid. Exposure of nitric acid to light expels oxygen, and leaves the acid with that redundancy of azote which constitutes the nitrous or yellow acid. Nitric acid must therefore be kept in the dark, or else in bottles quite full. N.

*Muriatic.*

*Muriatic Acid\*.*

COMMONLY called spirit of salt, always contains iron, and frequently sulphuric acid and copper.

The sulphuric acid is detected either by the addition of barytes, or acetite of lead, and the results are the same as in the foregoing experiments.

Copper is investigated by superfaturating this acid with pure ammoniac, which in this case will immediately be tinged of a perceptible blue.

Iron is manifested by previously uniting this acid with carbonate of potash and treating it then with tincture of gall-nuts, or prussiate of potash. The first will produce a black, and the latter a blue precipitate.

The specific gravity of muriatic acid should be, 1,70 to 1,000.

Pure muriatic acid is totally destitute of colour.

*Acetous Acid.*

THE concentrated acetous acid commonly met with contains sulphuric acid, with which it is either fraudulently adulterated, or with which it is impregnated by a careless and slovenly management during the process for obtaining it.

We often find this acid contaminated by tartareous acid, and not unfrequently by copper or lead. The presence of sulphuric acid is here also discovered by the addition of barytes, or acetite of lead, in the manner already mentioned.

Tartareous acid is detected by saturating the acetous acid with vegetable alkali, in consequence of which a tartarite of potash becomes separated in the form of a white powder.

Copper is discovered by saturating the acid with carbonate of ammoniac, and lead becomes manifested by mixing it with water impregnated with sulphurated hydrogenous gas. In the first case a blue, and in the second a black precipitate will be formed.

The specific gravity of the most concentrated acetous acid should be to that of water as 1,050 to 1,000. It is colourless, and of a penetrating smell.

*Distilled Vinegar.*

VINEGAR, if not distilled in glass vessels, but in a still with a pewter head, always contains lead in solution.

To discover this, equal quantities of vinegar, and water impregnated with sulphurated hydrogenous gas, are mixed together, which mixture, if the vinegar is free from lead, will remain unaltered; whereas, on the contrary, the smallest quantity of this metal will produce a black precipitate.

We frequently meet with distilled vinegar adulterated with sulphureous acid, the presence of which is detected by means of nitrate of barytes, or acetite of lead, as mentioned before.

\* The proportion of sulphuric acid prescribed by our royal college of physicians for obtaining this acid is much too great, as 18 or 20 ounces are quite sufficient for a total decomposition of the quantity of muriate of potash ordered.



The best distilled vinegar has a pleasant taste and fragrant smell, is perfectly colourless, and twelve parts of it require one of dry vegetable alkali to neutralise it.

*Boracic Acid.*

ON account of the high price which this acid bears, we often meet with it intentionally adulterated with Venetian talc, asbest, &c.

Genuine boracic acid is soluble in five times its quantity of boiling ardent spirit, and the solution when set on fire burns with a green flame. In water it dissolves tardily; when submitted to the blow-pipe it fluxes to a perfect transparent glass if genuine; it vitrifies earth and stones. If it be added in small quantities to tartarite of potash, it transforms it into a very soluble salt. The best boracic acid is exhibited in small hexangular scaly crystals, of a shining silvery white colour. It discovers only a slight acid taste, and does not deliquesce in the air.

Its specific gravity is 1,480.

*Tartareous Acid.*

TARTAREOUS acid may very easily be contaminated with sulphuric acid, either fraudulently or in consequence of a faulty preparation.

The admixture of sulphuric acid is soon discovered, if a small quantity of the tartareous acid be dissolved in distilled water, and a few drops of a solution of acetite of lead are added to it: by this means a white precipitate is afforded, which, by the addition of a few drops of pure nitric acid, will be entirely redissolved if no sulphuric acid is present. The presence of this acid is still more readily manifested by adding to the tartareous acid dissolved in distilled water a few drops of a solution of nitrate of barytes, in consequence of which sulphate of barytes will immediately be generated.

Pure crystallized tartareous acid does not change by exposure to air; it is very soluble in water, and possesses a grateful acid taste.

*Acid of Amber.*

WE find this acid wonderfully adulterated sometimes with sulphuric acid and all its combinations, sometimes with tartareous acid, and now and then with muriate of ammoniac.

The sulphuric acid is discovered by means of nitrate of barytes, or acetite of lead.

The tartareous acid is discovered by means of vegetable alkali; for, if this acid be present, a quantity of tartarite of potash will be formed.

Muriate of ammoniac is discovered with respect to one of its component parts (the muriatic acid), by the solution of nitrate of silver; and with respect to the other (the ammoniac) by adding to the aqueous solution of this salt a little vegetable alkali, and, after heating the mixture, holding over it a stopper moistened with acetic acid; white fumes will be formed, which indicate that ammoniac is present.

Pure acid of amber is a crystalline white salt, of an acid taste, soluble in twenty-four times its weight of cold water, but in eight when boiling hot, and is volatilized on an ignited iron, leaving neither ashes nor any other residue behind.

*Acid of Benzoin,*

WHICH is commonly known by the name of flowers of benjamin, is not liable to be easily adulterated.

The best flowers of benjamin are brilliant white, possess a peculiarly grateful smell, are totally soluble in boiling water and ardent spirit, and leave no residue or ashes when laid upon a red-hot iron, or on ignited coals.

## V.

*A Model proposed for the Construction of a Satellitian; or Instrument for explaining the Phenomena of Jupiter and his Satellites, with an Account of its Use. By the Rev. W. PEARSON, of Lincoln\*.*

SINCE the doctrine of brightness has been particularly attended to by Mr. Herschell in his observations of the heavenly bodies, this celebrated astronomer has been able, by means of the excellence of his telescopes, to announce to the world, what his predecessors could only conjecture, not only that the satellites of Jupiter have each a rotatory motion on their axes, but also the exact time of each rotation. In Part II. of the Philosophical Transactions of the Royal Society of the last year [1797], is an article in which Mr. Herschell has proved, I think, in a satisfactory manner, that each of these four satellites has, like our moon, just one rotation in every periodical revolution round its primary, viz. The first or nearest, in 1 day 8 hours and  $26\frac{6}{10}$  minutes; the second, in 3 days 13 [by a typographical error made 18] hours and  $17\frac{2}{10}$  minutes; the third, in 7 days 3 hours and  $59\frac{1}{10}$  minutes; and the fourth, in 16 days 18 hours and  $5\frac{1}{10}$  minutes.——This discovery of a rotation in each revolution renders the analogy so striking between the Jovian, as a detached part of the solar system, and that of our own earth and moon, that we can hardly any longer doubt whether or not Jupiter be inhabited. Such a consideration, in my opinion, renders the Jovian portion of the solar system a subject which merits more particular attention than is usually allotted to it in lectures upon astronomy; and on this account I have contrived the model of an instrument of a simple construction for the purpose of explaining the different phenomena thereof, which, for want of a more appropriate name, I shall take the liberty of calling a satellitian. Other instruments to answer the same purpose may have been constructed and used; but as I have no knowledge of any, except that Mr. Rowley's grand orrery is said to represent Jupiter's satellites moving by wheel-work†, I feel myself warranted in presenting to the public a plan and description of the constituent parts of this detached instrument, and of the method of using it, in hopes that it may prove not unacceptable to the cultivators of science, or at least that it may suggest to some more ingenious artist the means of making a more eligible one.

\* Communicated by the author.

† A machine for this purpose is also described in Harris's Lexicon Technicum. N.

In figure 1 of plate VI. is exhibited a lateral view of the wheels of this instrument, which are calculated for producing the mean motions of the four satellites, and contained in the brass box ABCD,  $8\frac{1}{10}$  inches long within, 4 broad, and  $1\frac{1}{10}$  deep. Figure 2, EF, represents the cover of this box, on which the different faces for the hour, week, and month-hands are graduated and silvered: the small concentric wheels marked 8, 18, 27, and 39 in fig. 1, are all fixed on the same revolving axle, placed on the point H, which carries the hour-hand on its upper end once round in 24 hours; but the larger corresponding wheels, 134, 129, 96, and 69, are fixed on the ends of as many separate tubes or hollow axles of brass, which just turn easily round within one another, and are supported by a shoulder in the perpendicular fixed stem that holds Jupiter over the point S; the innermost being the longest, and the next to the innermost the next in length, as in a common planetarium: these tubes have each a broad ring of a proper diameter to slide a little way upon their upper ends, to which are riveted the crooked arms which support the satellites; and the rings are turned round the tubes in the adjustment of the satellites to their relative situations: the tubes also have shoulders each for the support of its next superior wheel, and are kept in their places by the lower edges of the rings.

All the parts of the satellitian are laid down in the plate in their proper proportions and full size, according to the following table, from which an artist of but little ingenuity may easily construct it, provided he is furnished with, or has access to, a cutting engine for making the wheels, the plate of which is properly divided for the required numbers. An easy method for dividing a plate is laid down in Mr. Ferguson's "Select Exercises," in substance thus: As all the teeth in any wheel are to  $360^\circ$ , so are any odd number of teeth, to the number of degrees to be marked out, and divided into that odd number; after which the remaining even number may be very easily divided by continued halves.

*Table of Dimensions \*.*

Wheels	Diam. in Inches from the Pitch Line.	Distance of the Satellites from Jupiter in Diameters of him.
134	2,8	1. Satellite $2\frac{5}{8}$ diameters.
129	2,6	2. do. — $4\frac{1}{2}$ do.
96	2,3	3. do. — $7\frac{1}{6}$ do.
69	1,9	4. do. — $12\frac{2}{3}$ do.
8	0,2	
18	0,4	From S to H $1\frac{1}{2}$ inches.
27	0,7	From H to W 0,68 do.
39	1,1	From W to P 2,125 do.
56	1,2	
7	0,35	
73	3,9	

\* The wheels in the table are proportioned thus:—As the sum of the teeth in any two corresponding wheels is to the distance between their centres, or sum of their semidiameters, so is the number of teeth in each, taken separately, to its semidiameter: an allowance being afterwards made for the driving wheel being somewhat larger in this proportion than the driven one, to make them work more easily.—W. P.

Though the dimensions of the wheelwork here given are convenient for the construction of the satellitian, yet the box may be in the form of a square, an octagon, or a circle; though a parallelogram seems to be the most portable; but of whatever shape, there must be a contrivance for cramping or otherwise fixing it to a table when used.—The size of the ball, which represents Jupiter in the plate, is proportioned to the length of the arms which carry the satellites, and ought not to be larger; for an increased length of the arms would not admit the screen to approach near enough to prevent the divergence of the shadows.—The balls which represent the satellites should, however, be as diminutive as possible, so that they give a distinct shadow by candlelight. According to Mr. Herschell's observations, the third is the largest, the first and fourth nearly equal, and the second the smallest: their colours are, in his own words, as follows: the first is "white, more so sometimes than others;" the second "white, blueish, and ash-coloured;" the third "white, differently in different situations;" and the fourth "dusky, dingey inclining to orange, reddish, and ruddy at different times."

A motion is communicated to the machinery by means of a single endless screw, on an horizontal axle, lying across the box at the small black circle near 73, and working with the teeth of this wheel: a handle is put upon the projecting end of this axle, which cannot appear in the plate, at such a distance from the side of the box, as may prevent the hand of the person who turns it from touching any of the small balls, or intercepting the view of a small company of spectators.

As appendages to the satellitian, a small sliding screen of thin paper and an appropriate candlestick are necessary.—The mode of adjusting the screen is optional, and therefore requires no description.—The base of the candlestick, of which a sketch is given in miniature at fig. 3, consists of a circular plate of brass 4 inches diameter, divided into the 12 signs, and graduated in concentric circles near the extremity: in the centre of this stands a short perpendicular stem of a cylindrical form, which screws into a solid piece soldered on the surface of the plate, and has a socket proper for containing a candle or lamp at the upper end: on the low end of this stem turns a broad ring, into which an arm is riveted 3 inches long, which has a similar socket on its upper end, and opposite thereto a hand also riveted, which points to the graduated ecliptic opposite to the part on which the arm rests at all times.

In order to estimate the accuracy of the mean motions of the little balls in the satellitian, we must consider each pair of corresponding wheels as an improper fraction of a day; thus  $\frac{69}{39}$  of 24 hours is equal to 1 d. 18 h. 27 min. 41 sec.;  $\frac{27}{27}$  is equal to 3 d. 13 h. 20 min.;  $\frac{129}{128}$  is equal to 7 d. 4 h. 0 min.; and  $\frac{134}{8}$  equal to 16 d. 18 h. 0 min.; whence it appears that the error in one day's mean motion of the first satellite is not quite  $+ 37''$ ; of the second somewhat more than  $+ 37''$ ; of the third not quite  $+ 3\frac{1}{2}''$ ; and of the fourth about  $- 18''$ .—The first and second of these errors will amount to one hour's motion of each satellite in about  $97\frac{1}{2}$  days' use with one rectification; but the third and fourth will not be perceptible by the eye in several years, by reason of the slowness of the satellites' motions, as well as smallness of their daily errors. With regard to the calculation of the other wheels, the fraction  $\frac{56}{8}$  is equal to 7 days for the week-hand; and the compound one  $\frac{56}{8} \times \frac{73}{7}$  is equal to 73; but the spiral face on the cover has five lines divided into

365 equal parts, therefore five revolutions of the month-hand, viz.  $5 \times 73$ , will complete the year. The 365 days in the spiral are divided into calendar months, the initials of each of which stand immediately over the beginning of the first day of each; so that when the instrument is rectified for the month, day, and hour at any particular time, the different hands will preserve their respective situations to the end of the year, and so on from year to year, provided the month-hand be put back one day in the spiral at the latter end of February in every leap year. This putting back, however, will not in the least affect the motions of the satellites; for the hands, being put upon the circular ends of the axles, will turn in adjustment without giving motion to the wheels.

As the error in the first and second satellites will amount in one year to about three hours and forty-four minutes motion of each, they must also after that space be put to their right places again, which is very easily done thus: Turn the handle in a retrograde direction till all the satellites have come back 4 h. 44 min. then hold the first and second in that particular position while the handle is again turned to its original situation, and then let them be at liberty.—If this is done at a quarter of a year after rectification, the error will be somewhat less than an hour, and it may render a new rectification by the nautical almanac, as will be described hereafter, needless.

If at any time the letter pointed to in the week circle, and the corresponding day of the month in the spiral, in the first, second, third, fourth, or fifth line, as the case may be, be both attentively observed, it will appear by inspection for what particular time the satellitian stands rectified in a given year after it has been out of use; for, as the month-hand lies over the five lines, that particular line is the proper one, the numerical point of which answers to the initial of the day to which the week-hand points: for example, in the plate the rectification of the hands is for noon of Wednesday June 14, 1797. Without a regard to such an inspection, a new rectification would become necessary every time the instrument was to be used, and in some cases probably when the means of rectification might not be immediately attainable.

If the orbits of the satellites were not necessarily too large in the satellitian to be proportional to those of the earth and Jupiter, when the latter is placed at such a distance from them as that a candle in its place may produce a distinct shadow of each,  $5\frac{2}{3}$  times the distance between the two sockets, viz.  $15\frac{6}{10}$  inches, would be the proper distance of the central candle from Jupiter, in which position the angle at Jupiter in the satellitian subtended by the distance between the sockets, viz. 3 inches, would be equal to the parallax of the earth's annual orbit seen from Jupiter; but as the exact proportions of all their orbits cannot be preserved, a mechanical adjustment becomes requisite to preserve the due arrangement of the shadows upon the screen.

The rectification ought always to be very minutely attended to, and is thus effected: Fix the satellitian firmly on a table, and, setting the centre of the candlestick just *four feet* from Jupiter, place a steady burning candle, or rather a lamp, in the central socket, upon the same level with Jupiter in the instrument, and a similar one in the socket supported by the arm at the same height; by this means two shadows of Jupiter will fall upon the screen when placed behind the satellitian: slide the screen as near Jupiter as a revolution of the longest arm will suffer it to approach, which will be upwards of  $2\frac{1}{4}$  inches; and if the

the central candle and Jupiter form a right angle at the other candle, the two shadows will cover one another just so much as is represented in fig. 4. or fig. 6. ; but if this is not exactly the case, the candlestick must be brought nearer, or removed farther, till this appearance is produced ; and then the screen must be secured in its place, and the candlestick be suffered to remain. In the next place, with a pair of bow-compasses describe a circle coincident with that shadow which is occasioned by the central candle ; and within that, three other concentric circles, the largest of the three to touch the edge of the other shadow when the arm remains unmoved ; the next to touch the same when the arm is  $46^{\circ}$  from conjunction with Jupiter and the central candle ; and the smallest to touch the same when the arm is  $24^{\circ}$  from a similar situation.

Here then the central candle will represent the sun ; the outermost circle in fig. 4. or 6. the diametrical section of the sun's shadow at the orbit of the first satellite ; the second circle is supposed to represent the appearance of the same at the orbit of the second ; the third, the appearance at that of the third ; and the small one, the appearance at that of the fourth, as viewed in perspective from the earth ; whilst the candle carried by the arm will be the representative of the earth. Remove now the central candle, and Jupiter's remaining shadow will be his situation as viewed from the earth, coinciding with his real shadow, represented by the concentric circles, as far as the orbit of the second satellite, and then disuniting.

Hitherto the satellites themselves have been disregarded, and must next be placed in their proper situations by the help of a \* nautical almanac, thus : Look for the day, for which the satellitian is to be rectified, among the configurations given in the last page of every month, and the relative *apparent* situations of all the satellites, at the hour specified at the top, will be found to the right and left of the central cypher which represents Jupiter. When the numerical figure stands between the representative point of any satellite and Jupiter, the satellite is approaching him ; but when the point is put the nearer, it is receding from him : also, when a satellite is approaching on the right hand of Jupiter, or receding on the left, it is in its superior semicircle, and is placed above the centre of the cypher ; but when receding on the right, or approaching on the left, it is in its inferior one, and stands higher than the centre. The relative situations and direction of motion of each satellite being observed, put them all to their places as near as the eye can guess by the shadows compared to the points in the almanac ; then, having previously marked the greatest elongation of the fourth with little points on the screen, by means of a sector or diagram of similar triangles, make the distance of each shadow, from Jupiter's centre on the screen, bear the same proportion to the distance of each point from the centre of the cypher in the almanac, that the greatest elongation of the fourth on the former bears to the greatest elongation of the same in the latter : this will be attended with no difficulty.—The rectification for the *true* places of *mean motion* will however be the more accurate the

\* The rectification may be made most accurately by calculating the time of a conjunction of each satellite, *mean or apparent*, as may be required, by the help of Wargentin's Tables, in any given month : but every reader cannot be supposed to have such Tables ; and therefore the configurations are here substituted as affording a more general, as well as familiar method.—W. P.

smaller the grand equation of the satellites, which depends upon Jupiter's anomaly at the time of rectification.

The last most favourable time was on October 11, 1794, and the next least favourable will be on July 5, 1800: the former will recur on August 21, 1806, and the latter on May 17, 1812; and two of each will follow at nearly six years distance from each other in each revolution of Jupiter afterwards.

If the diameters of the cyphers in the nautical almanac, which are too small, were so far augmented as to bear an \* exact proportion to the greatest elongations of the satellites there exhibited, which appears to be not the case, their apparent places, measured by those diameters, might be easily ascertained without further trouble on any given day, as well as their true places of mean motion at the suitable times above specified.

The configuration fig. 7. is for half past six P. M. on October 11, 1794, where it appears, from what has been already remarked, that the first satellite is approaching Jupiter in its superior semicircle near its greatest western elongation; the second receding in its inferior; and the third and fourth receding each in its superior; the third being near its greatest eastern elongation. The corresponding places of the shadows on the screen, when the satellitian is rectified for this time, will appear as in fig. 8.

The shadows may be made to fall a little higher in their inferior semicircle than in their superior, by giving the instrument a small reclinacion with a thin wedge placed under the end next the candle, or by an adjusting screw pressing against the table, which effect is greatest with the fourth.

In this representation the satellites are very nearly in their true places of *mean* motion, as well as in their *apparent* places: therefore, if the satellitian be rectified for Saturday October 11, 1794, at half past six in the evening, by this latter configuration, and the handle be turned till the month-hand comes to the end of February 1796, before it be put back a day, and thence forward to the present time (1798), provided the first and second arms be also adjusted for their errors in motion, as already directed, the rectification will be more accurate for *mean* motion at present than if made by the almanac at any other time.

From 1794 to 1800 the difference between mean and apparent motion of the satellites, rejecting the smaller equations, is increasing; and from 1800, when it will be a maximum, to 1806, it will decrease, the apparent being faster than the mean, according to the year for which the satellitian is to be used: from 1806 to 1812 it will again increase, after which time a decrease will commence; and, in a little less than every six years, the increase and decrease will continue to be alternate for the time to follow.

Lastly: Put the index of the candlestick to the sun's place in the ecliptic, and the heliocentric longitude of Jupiter taken from a nautical almanac or White's Ephemeris, immediately between the central stem and Jupiter in the machine, by means of a thread stretched and tied to both, which will serve as an index, as well as a guide for the distance of the

\* The diameter of Jupiter is represented in the opposite extreme in the Encyclopædia Britannica, both in fig. 13. plate 62. and in fig. 177. plate 79. though it is said, in vol. ii. Part 2. p. 577. that "the orbits of Jupiter's moons are drawn in true proportion to his diameter" in the latter diagram, and are evidently intended to be such in the former. — W. P.

candlestick; and in this situation the representatives of the Sun, Earth, and Jupiter, as also of the satellites, will all be in their relative situations for exhibiting the general phenomena of the Jovian as a detached system.

But before I give a particular description of these phenomena as they will be exhibited by the satellitian, the reader will perhaps form a more accurate conception of the extent of its application, if a summary account be first given of those minute irregularities in the motions of the satellites, which no simple machinery can be supposed to represent.

The satellites of Jupiter were discovered by Galileo in January 1610, and called *Medicean stars*, in honour of Cosmo Medici great duke of Tuscany. This astronomer continued his observations upon them for 27 years, till unfortunately the loss of sight frustrated the fruit of his continued labours. In 1663 Borelli published a theory of the Medicean Stars, but had not acquired data sufficient for ascertaining the exact quantities and qualities of their motions.—After him Cassini, in the year 1668, favoured the world with “Tables of the Motions of Jupiter’s Satellites,” which were improved and edited by him again in 1693. These Tables have since that time been rendered still more accurate by Messrs. Hadley, Pound, Bradley, &c. as also by the French astronomers, and lastly by Wargentin the Swedish astronomer, whose best Tables are now used in calculating the immersions and emersions given in the nautical almanac.—The times of the mean periodical revolutions have been already mentioned; but they are subject to such inequalities of motion as require the following equations for ascertaining their apparent places: viz.

1. For the light which depends upon Jupiter’s eccentricity: 2. For the light which depends upon his change of place in his orbit: 3. For his anomaly: 4. For the mutual gravitation of the three first, the period of which is upwards of 437 days: 5. For a period of 12 years for the third, accruing from an unknown cause: 6. For a similar period for the fourth, accruing from its eccentricity: 7. For the variable inclination of the orbit of the second; and, 8. for apparent time.—The greatest or grand equation, which depends upon Jupiter’s anomaly, has been shewn to be different in different years: with the first satellite it is possible for it to amount to 1 h. 18 min. 16 sec.; with the second, to 2 h. 37 min. 12 sec.; with the third, to 5 h. 16 min. 32 sec.; and with the fourth, to 12 h. 20 min. 34 sec. at a certain time in some particular years above specified, though in others it may be nothing.

From measurements of the greatest elongations of these satellites, taken by a micrometer, it does not appear certain that their orbits are elliptical, except that of the fourth: the others, however, are by analogy supposed to be such, though the ellipses are so like circles that they may be considered as such, even in calculations, without any apparent error. The diameters of these orbits, when viewed from the earth, subtend but very small angles; viz. the first subtends  $3' 55''$ ; the second,  $6' 14''$ ; the third,  $9' 58''$ ; and the fourth,  $17' 30''$ .—The diameters of each of the satellites themselves, compared to that of Jupiter, seem not to be accurately known. Mr. Herschell’s observations make that of the first to be not quite  $\frac{1}{16}$  of a second; whereas, before his notice, they were each considered to subtend an angle more than double this quantity; for their diameters were estimated at  $\frac{1}{18}$  or  $\frac{1}{20}$  of Jupiter. Future observations must determine this point.



In observing the eclipses of Jupiter's satellites by his shadow, it has been found that the duration of an eclipse of each is longer at some times than at others: that in some instances a satellite passes through the centre of the shadow, which is supposed to be a line conical, but at others through only a chord of its circular section; hence an inclination of each of their orbits to that of Jupiter has been proved, and calculated to be as follows: Of the first, about  $2^{\circ} 55'$ , the ascending node being at rest near the middle of Aquarius; of the second, variable from  $2^{\circ} 50'$  (in 1668), to  $3^{\circ} 2'$  (in 1715), the ascending node being at rest about  $5^{\circ}$  of Aquarius; of the third, variable from  $3^{\circ}$  (in 1695), to  $3^{\circ} 24'$  (in 1765), its ascending node being about  $25^{\circ} 58'$  of Aquarius at this time (1798), and moving forwards eight minutes in a year; and of the fourth, about  $2^{\circ} 40'$ , which is very little variable, the ascending node being about the middle of Aquarius.

The apojove of the fourth is at about  $24\frac{1}{2}^{\circ}$  of Aries (1798), and moves forward about  $3^{\circ}$  in five years.

The duration of an eclipse is the greatest at the nodes and smallest at the limits: that of the first varies from 2 h. 16 min. to 2 h. 7 min. 40 sec.; of the second, from 2 h. 51 min. 20 sec. to 2 h. 13 min. 4 sec.; of the third, from 4 h. 35 min. 40 sec. to 1 h. 2 min. 32 sec.; and of the fourth, from 4 h. 46 min. to 0 h. 0 min. 0 sec.

The two opposite points of the ecliptic, cut by the plane of each orbit extended, are called the geocentric nodes; and a satellite appears to move in an exact straight line only when the earth is in one of those; for at other situations of the earth the track of each appears, though in a small degree, elliptical, and the more so the farther the earth is removed from their nodes: this is most apparent with the fourth, notwithstanding the nodes are all in the same signs, by reason of the greatness of its orbit; for, when near either limit, it entirely escapes an occultation, nor is eclipsed if removed above  $52^{\circ}$  from either node. This happens to be the case this year, and will continue so nearly, if not quite, throughout the next; and recurs for nearly two years and a half in every six.

The line in which the satellites appear is nearly horizontal, as on the screen, when Jupiter is on the meridian, but becomes the more oblique the farther he is removed therefrom.

The greatest part of these inequalities and peculiarities of motion, as well as their rotations, will not be attempted to be illustrated by the satellitian, but such phenomena only as are demonstrable to the eye of an observer independently of calculations, and which therefore may be considered as the most proper subjects of illustration by machinery.

After having given a description of the satellitian and its appendages; of the method of rectifying it for use; and of the principal minutiae relating to the motions of the satellites; I come now, in the last place, to particularize those phenomena to be illustrated, which a telescope of a moderate magnifying power for celestial objects will present to the observation of a spectator, and which afford a perpetual source of amusement to any person who is in possession of a good instrument. It is necessary however to make this previous remark, that if the telescope invert the object, the posterior surface of the screen must be viewed; but if it shew it direct, the anterior will be proper.

When the satellitian is properly rectified, the screen fixed and marked, and the candlestick adjusted with only one candle, the central one being removed, each turn of the

handle will produce one day's motion of every satellite; and a continuation of slow regular turns will produce a pleasing view of the following phenomena; viz.

1. The shadows will move in nearly a straight line.
2. Some in a direct and others in a retrograde direction.
3. The most distant will frequently appear the nearest to Jupiter.
4. Near Jupiter they will move the quickest, and slowest near their greatest elongation, where they become stationary for a short time.
5. Their greatest elongations will be a little before quadrature, when west; and after, when east of Jupiter.
6. Hence it will appear, that the superior portion of each orbit is greater than the inferior.
7. Whether to the east or west of Jupiter, their motions will be direct in the superior, and retrograde in the inferior, parts of their orbits.
8. When passing between Jupiter and the earth (candle) they will transit him.
9. When passing the same line in their superior semicircle they will suffer an occultation.
10. When passing through the four concentric circles they will be eclipsed.
11. The *mean* or *apparent* times, accordingly as the instrument may be rectified, of each of these phenomena, will be pointed out by the hour-hand, the name of the day by the week-hand, and the day of the month by the month-hand, in each year.
12. If the candlestick is adjusted frequently for the sun's place and Jupiter's heliocentric longitude, the solar shadow will appear to alter its situation to the right and left of Jupiter, as he approaches to or recedes from conjunction or opposition.
13. The reason will be evident, from the concentric circles, why an immersion or ingress into Jupiter's shadow, and a subsequent emerision or egress out of it, never both happen with the first and \* second satellites; nor with the third, if Jupiter is within  $46^{\circ}$  of opposition to, or conjunction with, the sun; nor with the fourth, if that distance is less than  $24^{\circ}$ .
14. It will be likewise evident why an immersion only is visible of the first and second satellites from a conjunction to an opposition (fig. 6.), why an occultation and eclipse may be coincident when at opposition (fig. 5.), and why an emerision only is seen from an opposition to a conjunction (fig. 4.).
15. If a proper reclination be given to the satellitian in this and on the next year, and also on every sixth and seventh year hence, the fourth satellite will neither be eclipsed nor suffer an occultation.
16. If a candle be now placed in each socket of different lengths, as is represented in fig. 3. the shadow of the central one to cover the concentric circles, there will be two rows of shadows on the screen above one another, one of which will represent the heliocentric, and the other the geocentric, places of the satellites.

\* An immersion and subsequent emerision may be seen of the second satellite, provided it be near one of its limits at the same time that Jupiter is near both his perihelion and quadrature with the sun; but this will very rarely happen.—W. P.

17. Hence it will appear, that an eclipse seen from the earth is an occultation seen from the sun.

18. That an eclipse is never visible at the sun.

19. And that the shadow of a satellite, as seen from the earth, falls on Jupiter sometimes before, sometimes after, and sometimes at its transit, according to the relative situations of Jupiter, the sun, and earth.

20. If the central candle only be suffered to remain, and the little balls themselves be viewed in an oblique direction, the reason will be apparent why the satellites are seen lunated from Jupiter :

21. And also Jupiter lunated when seen from them ;

22. But neither of them lunated when seen from the earth or sun.

23. It will also be evident why the sun is frequently eclipsed to the inhabitants of Jupiter by the shadows of his satellites.

24. And, lastly, why the satellites, as seen from the earth, do not eclipse one another.

These, and perhaps other phenomena not specified here, will be illustrated by the satel-  
lition in so distinct a manner, as to convey to a spectator, who may compare the apparent motions of the shadows to the real motions of the little balls, a clear conception of the relation that the apparent motions of Jupiter's satellites, as viewed from the earth with a telescope, have to their true circular motions, as viewed by the inhabitants of Jupiter. But though a clear conception may thus be formed by a spectator of the true and apparent motions of the satellites as they regard Jupiter himself, yet he must be informed that the *real track* in which they move round the *sun* along with Jupiter, the progressive centre of their detached system, is neither circular nor elliptical, but in a line which crosses Jupiter's track in a sinuose manner, more or less frequently as their periods are shorter or longer. As the velocity of the first and second satellites exceeds the velocity of Jupiter, they are not only apparently, but really retrograde in some part of every revolution ; on which account their tracks form loops which are alternately concave and convex towards the sun ; the concavity being greater than the convexity : but as the velocity of Jupiter exceeds that of the third and fourth, these are not really but only apparently retrograde at the inferior semicircles : on this account their tracks are always concave, except at their stationary points, which project and divide the concave spaces. These real tracks will easily be comprehended, if, while the satellites are in motion, Jupiter himself be also conceived to have a direct motion slower than the two first, but quicker than the two last.

If now, after what has been said, we conceive ourselves, like the philosophical poet \*, conveyed into the regions of Jupiter and his satellites, we shall with him find cause for adoring the power, wisdom, and goodness of the Almighty Creator ! When we consider that Jupiter, the diameter of which planet is more than ten times larger than that of our earth, has a rotation on its axis in the small space of nine hours and fifty-six minutes, we

\* ——— “ remote from day's all-cheering source,

“ Large Jupiter performs his constant course :

“ Four friendly moons with borrow'd lustre rise,

“ Bestow their beams benign, and light his skies.” *BAKER'S Universe.*

must perceive, that, without the assistance of some other luminary besides the sun, total darkness would be the lot of his inhabitants for nearly five hours in every ten.—Again, if we consider that Jupiter's year, or periodical revolution, contains 4332 d. 8 h. 51½ min. of our time, or 22936,43 + of his own days, and that he has no sensible change of seasons, by reason of the nearly perpendicular direction of his axis, the inclination of which is only 1° 20', we must see that the number of his days in his year might frequently be lost, without some intermediate remembrancers between those two very distant extremes: accordingly, the Omnipotent Father of the creation has provided a remedy for these inconveniences: he has furnished Jupiter with these four moons to cheer his inhabitants with light in the frequent absence of the sun's rays, and to present them with four different kinds of months, like our years, months, and weeks.

Of the first kind of months in Jupiter's year there are 244,97 +, each of which contains 9,36 + of his days; of the second kind there are 121,89 +, each containing 18,81 +; of the third kind there are 60,45 +, each comprising 37,94 +; and of the fourth kind there are only 25,85 +, each of which comprises 88,68 + of his days; so that each shorter month, particularly of the three first kinds, is very nearly *double* the next longer; and may be considered as exactly such, in counting time by divisions and subdivisions of months, by the help of intercalary days.

But besides these uses of Jupiter's satellites, and others, perhaps, which our limited capacities can never comprehend, there are three very considerable advantages, which the inhabitants of our globe possess, accruing from the observations which have already been made upon them; viz. an accurate knowledge of the parallax of the earth's annual orbit; of the real velocity of light; and of an easy method of ascertaining the longitude of places by land; all which are explained in the different books upon astronomy:—so true is it throughout the whole grand scale of nature, that no individual part is without its utility.

*Lincoln,*  
*March 10, 1798.*

## VI.

*Objections to the Opinion of Professor Spallanzani respecting the Cause of the Light of Natural Phosphori. Communicated to Mr. John Fabbroni, Sub-Director of the Royal Museum of Florence. By M. JOACHIM CARRADORI, M. D.\**

AT length the hypothesis of Gœtting is entirely destroyed, in consequence of the refutations of various celebrated authors, and among them Spallanzani. You may perhaps recollect that I foretold its short duration, and I pointed out to Brugnatelli, at its first appearance, several proofs of its falsity, which might be deduced from the very experiments it was built upon.

Though the refutation of Spallanzani † has not the merit of being the first, its excellence

\* Annales de Chimie, xxiv. 216.

† Chimico Esame del Cittadino Spallanzani. Modena. 1796.

cannot be disputed. I have read this short treatise with pleasure, from the many excellent and amusing points of knowledge it contains. But on perusing it with a certain degree of attention, I cannot avoid making several observations which the facts appear to demand.

It appears to me that Spallanzani supposes himself to be the first who observed that water has the property of absorbing oxygen from the air of the atmosphere. For at the 114th page of his book, he says: *Trovai pertanto che l'acqua e' un mezzo di decomporre l'arie, come lo sono il fosforo di Kunckel e i sulfuri alcalini; mà ella agisce con estrema lentezza.* "I found, however, that water is a medium for decomposing the air in the same manner as Kunckel's phosphorus, or the alkaline sulphurets; but it acts with extreme slowness."—But this fact was already known to Scheele (*Treatise of Air and Fire*), and consequently the honour of this discovery is his right. He was the first who observed that, by keeping a bottle of atmospheric air inverted over water for several days, the water gradually rises; the volume of air is diminished, and azotic gas, or, as it was then called, phlogisticated air, remains alone. It is surprising that Spallanzani should not be aware of this; or, if he knew it, that he should have omitted mentioning it.

I have very important facts to state against his theory of natural phosphori. It is long since I first observed that the phosphoric wood not only shines under water and under oil, but even in the barometric vacuum; which observations are related in the second volume of my theory of heat. If the phosphoric wood require air to enable it to shine, how can it continue to emit light under oil, where the vital air neither exists nor can have access? I must likewise remark the difference which Spallanzani has observed between the urinous phosphorus and that of wood; namely, that when the former is entirely surrounded by any pure mephitic air, it immediately ceases to give light; whereas the other being placed in pure azotic air continues to shine during six minutes, and does not entirely lose its light till half an hour afterwards.

When Spallanzani introduced phosphoric wood into vital air, or oxygen gas, how did it happen that he omitted to observe, whether by its shining in that fluid for a considerable time, there was no diminution of volume, as he observed that this diminution took place when the phosphoric flies (*luciole*) were placed therein?

The *lucioloni*, or glow-worms, as well as the *luciole*, shine under oil. I have observed a luminous fly continue to shine perfectly for a quarter of an hour in the barometric vacuum.

Spallanzani found that the phosphorus of these flies shines much more in oxygen gas than in the air of the atmosphere, and is totally extinguished by air not capable of maintaining combustion. But how can we explain their shining under oil for hours together? The fact is certain, because I have repeated the experiment with many variations; sometimes by putting the phosphoric flies entire beneath the oil, and sometimes the phosphorus only detached from the insect, and even crushed.

This observation must be well known to you, since I communicated it last year to the Royal and Oeconomical Society of Florence, in my Memoir on the *Luciole*; and it will be equally known to the whole scientific world, as it will be published in the xiii<sup>th</sup> volume of the *Chemical Annals* of the celebrated Brugnatelli.

The

The experiment of placing the luminous flies in oxygen is not new. It was before made by Forster; for which see his Theory of Heat, and the Journal de Rozier for 1784. He observed that they give much more heat in this fluid, not only at intervals, but continually. M. Forster moreover assures us, that he discovered the organs of respiration in those insects, which M. Spallanzani could not find. He describes them precisely, by observing that in each of the luminous rings there are two air passages, furnished with valves at their apertures, and that these passages are lost in the interior structure of the animal.

I have seen two species of luccioloni, or shining worms; namely, a larger and a smaller, of which I have given a slight description in my Theory of Heat, where I speak of phosphori. The first species also differs from the second in colour; the former being nearly grey, and the latter nearly black. The first have much luminous matter in the three last rings; and the others have less in the last ring but one. These may perhaps have been the species observed by the Naturalist of Pavia. Both species conceal their phosphorus at pleasure with the utmost facility. They seem therefore to be aware of its effects, particularly the second species.

It is not true, as certain naturalists, as well as M. Spallanzani, pretend, that the luccioloni and lucciole are animals of the same species, different only in sex, that is to say, that the former are the males, and the latter the females. I can affirm that I have seen the lucciole pregnant, with the abdomen filled with eggs: and this part, which before was so luminous, had its phosphoric part very much diminished, being reduced to merely two points, and two small lateral portions. The lucciole begin to disappear precisely at the time of their pregnancy, which is the cause why they conceal themselves. If they be sought for among the grass and underwood, they are found in this state\*.

If it be certain then that phosphoric wood, the luccioloni and the lucciole continue to shine under oil, it must be admitted that this light does not arise from a slow combustion, as Spallanzani pretends, because oil contains no air to support it. This is the legitimate and immediate consequence which must follow from this experiment, and cannot be refused. The experiments of Spallanzani lead to peculiar consequences, though different from those he has deduced. It is possible that the unrespirable air may act in a particular manner on these phosphori, which may be capable of preventing the emanation of their lights; and that oxygen, by a particular action of a contrary kind, may augment it.

Why should it be disputed that the different airs may produce peculiar effects on these substances, with which we are not yet acquainted? To Spallanzani will be due the honour of first observing them. In the same manner as various fluids are pernicious to natural phosphori, and prevent their shining, why may not the air produce the same effect? I have found by experiment, that the phosphorus of the lucciole is suddenly extinguished if they be plunged in alcohol, or in vinegar; but continues to shine in oil in the same manner as in water and in air.

It may be objected to me, that Spallanzani has made the experiment, that the phosphorus of the shining worms produces a diminution of volume in oxygen gas, at the same time that their light is increased; whence it

be concluded that the process is analogous

\* See the note No. 1. p. 78. tom. II. of my Theory of Heat. C.

to combustion. But this consequence is not certain. How many substances are there which have the property by their emanations of altering vital air, in the same manner as combustion, which nevertheless do not burn nor emit light? The same may be the case with the phosphoric matter of these insects.

There is not a perfect analogy, as Spallanzani pretends, between the phosphori he has observed and the urinous phosphorus, because the latter does not shine but at a certain heat; whereas the natural phosphori shine at any temperature whatever, provided it be not so great as to alter their substance. This proves, in my opinion, that the light is not an effect of combustion; for every combustion requires a more or less elevated degree of heat.

With regard to the explanation given by Spallanzani of the change of wood into the luminous matter, which supposes that the hydrogen and carbon, being set at liberty, attract oxygen; I will venture to say, on the strength of my experiments, that it is not probable. It is certain (for I have observed it myself, and probably some others before me), that wood, when luminous, has almost totally lost its resinous part, and that consequently in that state it retains scarcely any either of the carbonic or hydrogenous principle\*, to which its combustibility was owing. And in fact, such wood as has become luminous is with difficulty burned in the fire, and produces no flame, as every one may try. Indeed it cannot be otherwise; for the progress of putrefaction, which reduces it to that state, must have deprived it of much of its component parts; particularly the most volatile, such as hydrogen. Besides which, as I observed some months ago to Brugnatelli, I think very differently from Spallanzani with regard to the constitution of these substances; namely, that they become phosphorescent in proportion as they have lost their inflammable principle, and that the property of absorbing and retaining the light depends on that circumstance.

My opinion may be extended, in preference to that of Spallanzani, to the cause of the phosphoric property of animals; for it is more reasonable: because we cannot imagine that their luminous matter is either resinous or oily, and consequently is not inflammable; neither can it contain much carbon or hydrogen. See my *Memoire on the Lucciole*, *Ann. Chim. et Historiques de Pavia*, tom. xiii.

If the lucciole shine beneath the water, as Spallanzani maintains, because the oxygen gas contained in water serves to maintain their combustion, why does not the phosphorus of Kunckel also shine beneath that fluid? It was likewise necessary to have exhibited some experiments in support of that opinion; as for example, to have shewn that the phosphorus of those animals alters or absorbs the vital air contained (uncombined) in water, and that water which does not contain it is not capable of causing them to shine.

*Pavia,*

*April 15, 1797.*

\* Of what principles, then, is this vegetable residue composed? N.

## VII.

*Sketch of the History of Sugar, in the early Times, and through the Middle Ages (a).*

*By W. FALCONER, M.D. F.R.S. &c. &c.*

THE use of sugar is probably of high, though not remote antiquity, as no mention of it is made, as far as I can find, in the sacred writings of the Old Testament (b). The conquests of Alexander seem to have opened the discovery of it to the western parts of the world.

Nearchus\*, his admiral, found the sugar cane in the East Indies, as appears from his account of it, quoted by Strabo (c). It is not, however, clear, from what he says, that any art was used in bringing the juice of the cane to the consistence of sugar.

Theophrastus †, who lived not long after, seems to have had some knowledge of sugar, at least of the cane from which it is prepared. In enumerating the different kinds of honey, he mentions one that is found in reeds (d), which must have been meant of some of those kinds which produce sugar.

Eratosthenes ‡ also is quoted by Strabo (e), as speaking of the roots of large reeds found in India, which were sweet to the taste both when raw and when boiled.

The next author, in point of time, that makes mention of sugar, is Varro §, who, in a fragment quoted by Isidorus (f), evidently alludes to this substance. He describes it as a fluid, pressed out from reeds of a large size, which was sweeter than honey.

Dioscorides (g) ||, speaking of the different kinds of honey, says, that “there is a kind of it, in a concrete state, called *saccharon*, which is found in reeds in India and Arabia Felix. This, he adds, has the appearance of salt; and, like that, is brittle when chewed. “It is beneficial to the bowels and stomach, if taken dissolved in water; and is also useful “in diseases of the bladder and kidneys. Being sprinkled on the eye, it removes those

(a) Manchester Memoirs, iv. 291.

(b) Since writing the above, I have observed that the *sweet cane* is mentioned in two places of scripture, and in both as an article of merchandize. It does not seem to have been the produce of Judea, as it is spoken of as coming from a far country. Isaiah, chap. xliii. v. 24. Jeremiah, chap. vi. v. 20.—It is worthy of remark, that the word *sacchar* signifies, in the Hebrew language, inebriation, which makes it probable that the juice of the cane had been early used for making some fermented liquor.

(c) Εἰσηκε δὲ περὶ τῶν καλάμων ὅτι ποιοῦσι μέλι, μέλισσών μιν ὕδων. Strabon. l. xv.

(d) Ἀλλὰ δὲ ἐν τοῖς καλάμοις. Fragment of Theophrastus preserved in Photius. See p. 864. edit. Augsb. 1601.

(e) Καὶ τὰς ζίζας τῶν φυτῶν, καὶ μέλις τῶν μεγάλων καλάμων, γλυκεῖας καὶ φύσει κατεψυγμέναι. Strabon. l. xv.

(f) Indica nam magna nimis arbore crescit harundo;

Illius e lentis premitur radicibus humor,

Dulcia cui nequeant succo contendere mella. Isidor. lib. xvi. cap. 7.

(g) Est et aliud concreti mellis genus, quod saccharon nominatur. In India vero et Felicia Arabia, in harundinibus invenitur. Salis modo coactum est; dentibus, ut sal, fragile; alio idoneum et stomacho utile, si aqua dilutorum bibatur; vexatæ vesicæ, renibusque auxiliatur. Illitum ea discutit, quæ tenebras oculorum pupillis offundunt. Matthioli Diosc. cap. lxxv.

\* Ante Christ. ann. 325. † A. C. 303. ‡ A. C. 223. § A. C. 64. || A. C. 35.



"substances that obscure the sight." The above is the first account I have seen of the medicinal virtues of sugar.

Galen \* appears to have been well acquainted with sugar, which he describes, nearly as Dioscorides had done, as a kind of honey, called *sacchar*, that came from India and Arabia Felix, and concreted in reeds. He describes it as less sweet than honey, but of similar qualities, as detergent, desiccative, and digerent. He remarks a difference, however, in that sugar is not like honey injurious to the stomach, or productive of thirst (a).

If the third book of Galen, "*Upon Medicines that may be easily procured*," be genuine, we have reason to think sugar could not be a scarce article, as it is there repeatedly prescribed.

Lucan † alludes to sugar, in his third book, where he speaks of the sweet juices expressed from reeds, which were drank by the people of India (b).

Seneca ‡, the philosopher, likewise speaks of an oily sweet juice in reeds, which probably was sugar (c).

Pliny § was better acquainted with this substance, which he calls by the name of *saccaron*; and says, that it was brought from Arabia and India, but the best from the latter country. He describes it as a kind of honey, obtained from reeds, of a white colour, resembling gum, and brittle when pressed by the teeth, and found in pieces of the size of a hazel nut. It was used in medicine only (d).

Salmasius, in his *Plinianæ Exercitationes*, says, that Pliny relates, upon the authority of Juba the historian, that some reeds grew in the Fortunate Islands, which increased to the size of trees, and yielded a liquor that was sweet and agreeable to the palate. This plant he concludes to be the sugar cane; but I think the passage in Pliny (e) scarcely implies so much.—Hitherto we have had no account of any artificial preparation of sugar, by boiling or otherwise; but there is a passage in Statius ||, that seems, if the reading be genuine, to allude to the boiling of sugar, and is thought to refer immediately thereto by Stephens in his *Thesaurus* (f).

Arrian ¶, in his *Periplus* (g) of the Red Sea, speaks of the honey from reeds, called

(a) De simplic. Medicamentis. Lib. vii.

(b) Quique bibunt tenera dulces ab arundine succos. Lucani *Pharſaliæ* lib. iii. lin. 237.

(c) Aiunt inveniri apud Indos mel, in arundinum foliis, quot aut ros illius cœli aut ipsius arundinis humor dulcis et pinguior gignat. Senec. *Epistol.* l. i. *Epist.* lxxxiv.

(d) Saccaron Arabia fert, sed laudatius India. Est autem mel in arundinibus collectum, gummiū modo candidum, dentibus fragile, amplissimum nucis avellanæ magnitudine, ad medicinæ tantum usum. Plin. *Histor. Natural.* l. xii. cap. viii.

(e) Plin. *Hist. Nat.* lib. vi. cap. xxxii.

(f) Et quas præcoquit Ebosita cannas

Largis gratuitum cadit rapinis. Stat. *Sylv.* l. vi. 15.

Haud dubie (inquit Stephanus) cannas intelligit ex quibus saccharum exprimitur vel coquitur. Et fortasse cannas pro saccharo ipso posuit. Sed qui Ebosita illi, hæcenus apud neminem invenimus. Populi fortasse sunt Indicæ, ubi saccharum potissimum nascitur. Steph. *Theſ. Vox Canna.* Læſio autem dubia est. Vide Not. Marklandi in hunc locum.

(g) Μελι το καλαμινον το λεγομενον Σαχαρι. Page 150. Ed. Amſtelod. 1683, 8vo.

\* Anno post Christ. nat. 143. † Lucani mors, A. D. 65. ‡ Senecæ mors, A. D. 65.

§ Plinii mors, A. D. 77. || A. D. circ. 80. ¶ A. D. 145.

*sacchar* (Σαχχαρ) as one of the articles of trade between Ariace and Barygaza, two places of the Hither India, and some of the ports on the Red Sea.

Ælian \*, in his Natural History, speaks of a kind of honey, which was pressed from reeds that grew among the *Prasii*, a people that lived near the Ganges.

Tertullian † also speaks of sugar, in his book *De Judio Dei*, as a kind of honey procured from canes (a).

Alexander Aphrodisæus (b) ‡ appears to have been acquainted with sugar, which was in his time regarded as an Indian production. He says, “ that what the Indians called “ sugar, was a concretion of honey, in reeds, resembling grains of salt, of a white colour, “ and brittle, and possessing a detergent and purgative power like to honey ; and which “ being boiled, in the same manner as honey, is rendered less purgative, without impairing “ its nutritive quality.”

Paulus Ægineta (c) § speaks of sugar as growing, in his time, in Europe, and also as brought from Arabia Felix ; the latter of which he seems to think less sweet than the sugar produced in Europe, and neither injurious to the stomach nor causing thirst, as the European sugar was apt to do.

Achmet (d), || a writer who, according to some, lived about the year 830, speaks familiarly of sugar as common in his time.

Avicenna (e), ¶ the Arab physician, speaks of sugar as being a produce of reeds ; but it appears he meant the sugar called Tabaxir or Tabarzet, as he calls it by that name.

It does not appear, that any of the above-mentioned writers knew of the method of preparing sugar, by boiling down the juice of the reeds to a consistence. It is also thought, the sugar they had was not procured from the sugar-cane in use at present, but from another of a larger size, called Tabarzet (f) by Avicenna, which is the *Arundo Arbor* of Caspar Bauhin, the *Saccar Mambu* of later writers, and the *Arundo Bambos* of Linnæus. This yields a sweet, milky juice, and oftentimes a hard crystallized matter, exactly resembling sugar, both in taste and appearance.

The historians of the Crusades make the next mention of sugar of any that have fallen under my observation.

The author of the *Historia* (g) \*\* Hierosolymitana says, that the Crusaders found in Syria certain reeds called *Cannameles*, of which it was reported a kind of wild honey was made ; but does not say that he saw any so manufactured.

(a) Mella viridanti confragrant pinguis canna. Tertullian. de Judio Dei.

(b) Alex. Aphrodisæi lib. ii. Probl. 79.

(c) Paul. Æginetæ Vox Mel. MELA. p. 632. Medic. Art. Princ. Ed. Henrici Stephani, 1567.

(d) Vide Meurfii Gloss. Græc. Barb. & Du Cange Gloss. ad Script. med. & inf. Græcitat. 11.

(e) De Zuccaro. Lib. II. Tract. II. De Melle. Lib. II. Tract. II.

(f) Some of the writers say, that it was so called from the name of a place Σαχχαρ Ταβαρζης, τοπος ὡς λευκός εἰς Συρίαν. Constantinus a Secretis, MS. quoted from Du Cange Gloss. Græc. The word Tabarzet signifies white, and is translated, by Du Cange, Saccar Album. Herbelot says, that the Persians called by that name the hardest and most refined sugar. Bibliothèque Orientale, p. 810.

(g) Pars secunda, p. 595.

\* A. D. circ. 145. † A. D. 195. ‡ A. D. 212.

§ A. D. circ. 400. vel secundum Friend multo posterior. Hist. Medic.

|| A. D. 830.

¶ A. D. 980. natus.

1100.

Albertus Agnenfis (a) \* relates, that about the same period, "the Crusaders found sweet "honeyed reeds in great quantity, in the meadows about Tripoli, in Syria, which reeds "were called *Zucra*. These the people (the Crusaders army) sucked, and were much "pleased with the sweet taste of them, with which they could scarcely be satisfied. This "plant (the author tells us) is cultivated with great labour of the husbandmen every "year. At the time of harvest, they bruise it when ripe in mortars; and set by the "strained juice in vessels, till it is concreted in form of snow, or of white salt. This, "when scraped, they mix with bread, or rub it with water, and take it as pottage; and it "is to them more wholesome and pleasing than the honey of bees. The people who were "engaged in the sieges of Albaria Marra and Archas, and suffered dreadful hunger, were "much refreshed hereby."

The same author †, in the account of the reign of Baldwin, mentions eleven camels, laden with sugar, being taken by the Crusaders, (b) so that it must have been made in considerable quantity.

Jacobus de Vitriaco mentions ‡, (c) that "in Syria reeds grow that are full of honey, by "which he understands a sweet juice, which by the pressure of a screw-engine, and con- "creted by fire, becomes sugar." This is the first account I have met with of the employ- ment of heat or fire in the making of sugar.

About the same period § (d) Willermus Tyrensis speaks of sugar as made in the neighbour- hood of Tyre, and sent from thence to the farthest parts of the world.

Marinus Sanutus mentions (e) ||, that in the countries subject to the Sultan, sugar was pro- duced in large quantity, and that it likewise was made in Cyprus, Rhodes, Amorea, Marta, Sicily, and other places belonging to the Christians.

Hugo Falcandus (f) ¶, an author who wrote about the time of the Emperor Frederic Bar- barossa, speaks of sugar being in his time produced in great quantity in Sicily. It appears to have been used in two states; one wherein the juice was boiled down to the consistence of honey, and another where it was boiled farther, so as to form a solid body of sugar.

(a) Calamellos ibidem mellitos, per camporum planiciem abundanter repertos, quos vocant *Zucra*, fuxit po- pulus illorum salubri succo lætatus; et vix ad saturitatem prædulcedine explere hoc gustato valebant. Hoc enim genus herbæ, summo labore agrorum, per singulos excolitur annos. Deinde, tempore messis, matu- rum mortariolis indigenæ contundunt, succum colatum in vasis suis reponentes, quousque coagulatus induref- cat, sub specie nivis vel salis albi. Quem rasum cum pane miscentes, aut cum aqua terentes, pro pulmento sumunt; et supra favum mellis gustantibus dulce ac salubre esse videtur. His ergo calamellis melliti saporis, populus in obsidione Albariæ Marra et Archas multum horrenda fame vexatus, est refocillatus.

Gest. Dei per Francos, p. 270.

(b) Gest. Dei, p. 353.

(c) Sunt autem calamelli, calami pleni melle succo dulcissimo, ex quo quasi in torculari compresso, et ad ig- nem condensato, prius quasi mel posthæc quasi *Zuccara* efficitur. Gest. Dei, p. 1675.

(d) Per infitiores ad ultimas orbis partes deportatur. Gest. Dei, p. 835.

(e) Marin. Sanut. L. I. Part I. Cap. 2.—in parte secunda Gest. Dei.

(f) In Præfatione ad Libr. de Calamitatibus Siciliæ.

\* 1108. † 1110. ‡ 1114. § 1124. || 1306. ¶ 1170.

The foregoing are all the passages that have occurred to my reading on this subject. They are but few and inconsiderable, but may save trouble to others, who are willing to make a deeper enquiry into the history of this substance.

Jan. 24, 1790.

The following passage, taken from the *Viridarium Francisci Mendozæ, Sacræ & Profanæ Eruditionis. Coloniz Agrippinæ, 1633*, seems to point out, though rather obscurely, the construction and principles of Balloons.

“Vas æreum, plenum aëre, aliter demergendum, in summa aqua sustentatur, cum ea sit naturaliter multo gravius; ergo navis lignea, aut cujuscunque alterius materiæ in summa aëris superficie constituta, et elementari igne repleta, supra aërem sustinebitur, nec prius in ipso aëre submergetur, quam navigii gravitas superet levitatem ignis, quo plenum est.”

Problema XLVII. Utrum aër parte aliquâ sit navigabilis.

W. F.

### VIII.

*Experiments and Observations on the Effect of Annealing a Plate of Metal, consisting of fine or alloyed Gold, with one twenty-fourth Part of Tin\*. By MATHEW TILLET.*

THOUGH the advantage be great, that men of enlightened minds and well known artists should make inquiries into the kind of works to which their attention has been particularly applied, and should render their observations public; it is nevertheless true, that their reputation, in many respects well founded, does in some instances cause the results drawn from their experiments to be too speedily adopted; and that when they are carefully repeated, it does not always happen that they prove exact. These artists, no doubt, well informed and faithful in their operations and reports, have not pretended to offer them as decisive; but by neglecting to consider the facts before them in every point of view, they have too speedily concluded that these facts, universally considered, were such as to them they appeared at first sight.

Looking over the *Journal de Physique* for the month of September 1788, I saw a paper entitled “Experiments and Observations on the Fusion of Gold with Tin,” which I read with great attention, as being the production of a man of merit well versed in his art, namely, Mr. Alchorne, assay-master to the English mint. As the object of his experiments is interesting to every artist who uses gold and silver; as its aim is to remove their fears respecting the mixture of a certain quantity of tin with fine or alloyed gold, and as these artists do nevertheless retain their apprehensions with regard to the smallest mixture of tin with the gold of various fineness, which they are incessantly melting; I have thought it proper to repeat the experiments of Mr. Alchorne: and while I admit part of the facts he has related, I must refute those which my own trials shew to be erroneous, and which, if they had not escaped his attention, would doubtless have led him to make certain exceptions, in the too positive consequences he has drawn from his operations.

Before I enter upon a detail of the experiments of Mr. Alchorne, as well as my own, I think it proper to take notice of the manner in which the editor of the *Journal* announ-

\* *Memoirs of the Academy of Sciences at Paris for the year 1790, being the concluding volume, printed in 1797.*

ees the memoir in question. He appears to be convinced that the world had adopted an erroneous opinion respecting the object of the paper previous to its appearance ; whence it follows, that this Journal, which is deservedly esteemed, and of wide circulation, cannot fail to produce a strong impression in this respect, which ought to be contradicted. It is in fact founded on operations which have not been pursued with sufficient accuracy ; and on this account they tend to produce a false security in the mind of artists who work the most precious of metals.

It has long been a received fact among metallurgists, observes the editor, that tin mixed with gold in the smallest quantity, either in substance or in vapour, is totally destructive of the malleability of that metal. But Mr. Woulfe, fellow of the Royal Society of London, communicated to that Society, in 1764, a memoir of Mr. Alchorne, assayer at the Mint in the Tower of London, and since printed in the Philosophical Transactions, in which he proves, that tin may be mixed with gold in a moderate quantity without producing these bad effects. These experiments, he adds, have not been contradicted ; notwithstanding which, several of the most celebrated authors have continued to follow the ancient opinion, though in no respect founded upon fact. It may therefore be supposed, that the memoir of Mr. Alchorne has not been sufficiently known and attended to ; for which reason I have thought proper to give an abstract, to make it more generally known.

M. Alchorne relates, that he had long doubted this extraordinary property attributed to tin ; and that an opportunity having offered, he made various experiments on that subject. He mixed twelve ounces of fine gold with different proportions of tin from sixty grains to half an ounce. These compounds were beaten under the hammer, passed through the laminating rollers, and struck in the fly-press without shewing any brittleness. He likewise attempted to expose gold to the vapour of tin ; for which purpose he put twelve ounces of gold of 22 carats into a small crucible, which he placed in a larger crucible, and surrounded it with tin, and subjected the whole to a considerable heat for half an hour ; but the gold lost nothing of its ductility. He carried his researches still farther : he alloyed the foregoing mixtures with copper, and afterwards added tin to the gold thus alloyed with different proportions of copper and silver ; but in all the various cases, twelve ounces of gold alloyed with tin in the quantity of half an ounce, and of copper two ounces and a half, suffered hammering and laminating to the thickness of strong paper, and could be wrought into small toys and drawn into fine wire with the same facility as the gold of commerce.

M. Alchorne observes, that the old opinion adopted by so many authors owes its origin probably to the arsenic which tin commonly contains, as he found that twelve grains of that semi-metal in regulus rendered the same number of ounces of gold brittle. Whence he concludes that tin, like the other baser metals, does not injure gold, but in proportion to the quantity of arsenic it contains, and that there is nothing in tin which can deprive gold of its qualities, as was before observed.

From this short account of the experiments and observations of M. Alchorne, we see that he affirms that gold, whether pure or alloyed, being fused with tin in the proportion of one part of the former to twenty-four of the latter, forms an alloy which preserves ductility sufficient to endure hammering and laminating to the thickness of strong paper, to be used in toys and drawn into fine wire with the same facility as the common standard gold.

Though

Though I was persuaded that tin deprived gold of its great ductility, or at least rendered it so brittle that it could not be reduced to thin leaves, nor more especially be made to pass the wire plate but by virtue of repeated annealing, and peculiar treatment which gold of the usual ductility does not require, I nevertheless determined to repeat the experiments of Mr. Alchorne, as well from esteem for that skilful artist, as to supply such facts as might appear to have escaped his notice.

My first experiment consisted in mixing 24 grains of fine gold with one of tin, taken from an ingot of this last metal which contains no arsenic. I wrapped this grain of tin in the 24 grains of gold reduced to a very thin leaf, rendered very flexible by annealing. I placed these 25 grains upon a piece of charcoal hollowed out, upon which they could be supported during their fusion. I even sprinkled a small quantity of calcined borax upon the metal, in order that the fusion might be more sudden, that the metal might flow together, and the tin unite with the gold without allowing time for it to become calcined. This alloy was speedily fused by the enameller's lamp, and reduced into a small button without any loss of weight. It was then flattened carefully beneath the hammer; but, notwithstanding my precaution in this respect, it cracked, and at last broke into three pieces, its thickness then being a quarter of a line or thereabouts.

I repeated this first experiment with a double quantity as well of pure gold as of tin. The result was the same. This second button was brittle, and likewise broke under the hammer, though I had carefully managed the process of hammering in order that this button might have continued whole notwithstanding the cracks.

It is easily seen that these experiments, which were in some measure preparatory, tended to intimate the consequences I had reason to expect when I should repeat them more at large, and in a manner more nearly resembling the experiments with which I meant to compare them.

[*To be concluded in our next.*]

#### ACCOUNTS OF BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1798. Part I. Quarto. 199 pages, with 26 pages of Meteorological Journal, and 7 plates. Sold by Elmsly, London.

**T**HIS part contains the following papers: 1. The Bakerian Lecture. Experiments upon the Resistance of Bodies moving in Fluids. By the Rev. Samuel Vince, A.M. F.R.S. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.—2. Experiments and Observations, tending to shew the composition and properties of Urinary Concretions. By George Pearson, M.D. F.R.S.—3. On the Discovery of four additional Satellites of the Georgium Sidus; the retrograde motion of its old Satellites announced, and the cause of their disappearance at certain distances from the planet explained. By William Herschel, L.L.D. F.R.S.—4. An Enquiry concerning the Source of the Heat which is excited by Friction. By Benjamin Count of Rumford, F.R.S. M.R.I.A. (See *Philos. Journal* II. 106.)—5. Observations on the Foramina Thebesii of the Heart. By Mr. John Abernethy, F.R.S.—6. An Analysis of the earthy

earthy Substance from New South Wales, called *Sydnia*, or *Terra Australis*. By Charles Hatchett, Esq. F.R.S. (See *Philos. Journal*, II. 72).—7. Abstract of a Register of the Barometer, Thermometer and Rain, at Lyndon in Rutland, for the year 1796. By Thomas Barker, Esq.—8. An Account of some Endeavours to ascertain a Standard of Weight and Measure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S.—9. A New Method of computing the Value of a slowly converging Series, of which all the Terms are affirmative. By the Rev. John Hellins, F.R.S. and Vicar of Potter's-Pury in Northamptonshire. And the Appendix, containing a Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical. Essay VII.—Of the Propagation of Heat in Fluids. Part II.—An Account of several New Experiments, with occasional Remarks and Observations, and Conjectures respecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. Octavo. 75 pages, with 2 plates. Cadell and Davies. Price 1s. 6d.

This second part accompanies a new edition of the first. The philosophical world will not need any general remark on the interesting nature of the subject, nor the manner in which the great author has treated it. For the present, I copy the abridgment of its Contents, and shall speedily give a fuller account.

Chap. I. Account of a circumstance of a private nature, by which the author has been induced to add this and the following chapters to the second edition of this Essay.—Experimental Investigation of the subject continued.—Oil found by experiment to be a Non-conductor of heat.—Mercury is likewise a Non-conductor.—Probability that all Fluids are Non-conductors, and that this property is essential to fluidity.—The knowledge of that fact may be of great use in enabling us to form more just ideas with regard to the nature of those mechanical operations which take place in chemical solutions and combinations; in the process of vegetation; and in the various changes effected by the powers of life in the animal economy.—Rapidly of Solution no proof of the existence of an attraction of affinity.—Strata of fresh water and of salt water may be made to repose on each other in actual contact, without mixing.—Probability that the water at the bottom of fresh lakes, that are very deep, may be actually salt - - - - - page 311

Chap. II. Water made to congeal at its under surface.—Observation respecting the formation of ice at the bottoms of rivers.—Reasons for concluding that heat can never be equally distributed in any fluid.—Perpetual motions occasioned in fluids by the unequal distribution of heat.—An inconceivably rapid succession of collisions among the integrant particles of fluids is occasioned by the internal motions into which fluids are thrown in the propagation of heat.—An attempt to estimate the number of those collisions which take place in a given time.—These investigations will greatly change our ideas respecting the real state of fluids apparently at rest. Fluidity may be called the life of inanimate bodies.—Conjectures respecting the vital principle in living animals; and the nature of physical stimulation - - - - - page 332

Chap. 3. Probability that intense heat frequently exists in the solitary particles of fluids, which neither the feeling nor the thermometer can detect.—The evaporation of ice during the

the severest frost explained on that supposition.—Probability that the metals would evaporate, when exposed to the action of the sun's rays, were they not good conductors of heat.—Mercury is actually found to evaporate under the mean temperature of the atmosphere. This fact is a striking proof that fluid mercury is a non-conductor of heat.—Probability that the heat generated by the rays of light is always the same in intensity; and that those effects which have been attributed to light ought perhaps in all cases to be ascribed to the action of the heat generated by them: a striking proof that the most intense heat does sometimes exist where we should not expect to find it.—Gold actually melted by the heat which exists in the air of the atmosphere, where there is no appearance of fire, or of any thing red-hot.—We ought to be cautious in attributing to the action of unknown powers, effects similar to those produced by the agency of heat.—The most intense heat may exist without leaving any visible traces of its existence behind it.—This important fact illustrated by the necessary result of an imaginary experiment - page 345

Chap. IV. An account of a variety of miscellaneous experiments.—Thermometers with cylindrical bulbs may be used to show that liquids are non-conductors of heat.—Ice-cold water may be heated and made to boil standing on ice.—Remarkable appearances attending the thawing of ice, and the melting of tallow, and of bees-wax, by means of the radiant heat projected downwards by a red-hot bullet.—Beautiful crystals of sea-salt formed in brine standing on mercury.—Olive-oil soon rendered colourless by exposure to the air standing on brine.—An attempt to cause radiant heat from a red-hot iron bullet to descend in oil.—Account of an artificial atmosphere, in which horizontal currents were produced by heat.—Conjectures respecting the proximate causes of the winds - page 367

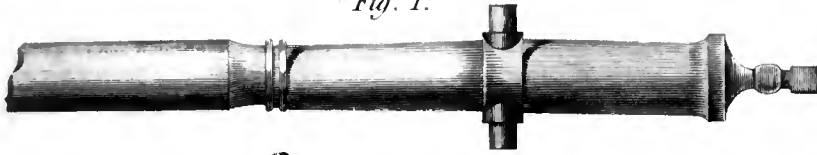
**A Practical Essay on the Club-Foot, and other Distortions in the Legs and Feet of Children,** intended to shew under what Circumstances they are curable or otherwise; with thirty-one Cases, and the Specification of a Patent granted the Author for his Method of Practice. By T. Sheldrake, Truss-maker to the Westminster Hospital and Mary-le-bone Infirmary. 8vo. 214 pages, with 14 plates. London: Printed for Murray and Highley, 1798.

This author shews by reasoning and induction, that the deformity which forms the chief subject of his treatise is produced by an unfavourable compression during the growth of the fœtus in utero. He adduces facts to prove, that mechanical means, if judiciously applied, will in numerous instances restore or place the organ in its proper state; and that it is of great importance that these remedies should be used as early as possible before the process of ossification is considerably advanced or completed. The contrivance and application of these means necessarily demand a knowledge of the formation of the parts, as well as of the nature of mechanical instruments. In both respects the author appears to have displayed considerable judgment, and has been rewarded by success. It is impossible to enter into any detail or description of his method within our limits. I shall therefore only say, that the nature of the cases and the respectability of the testimonies appear to deserve the attention of all who from professional pursuits or individual misfortune are urged to the consideration of this subject.

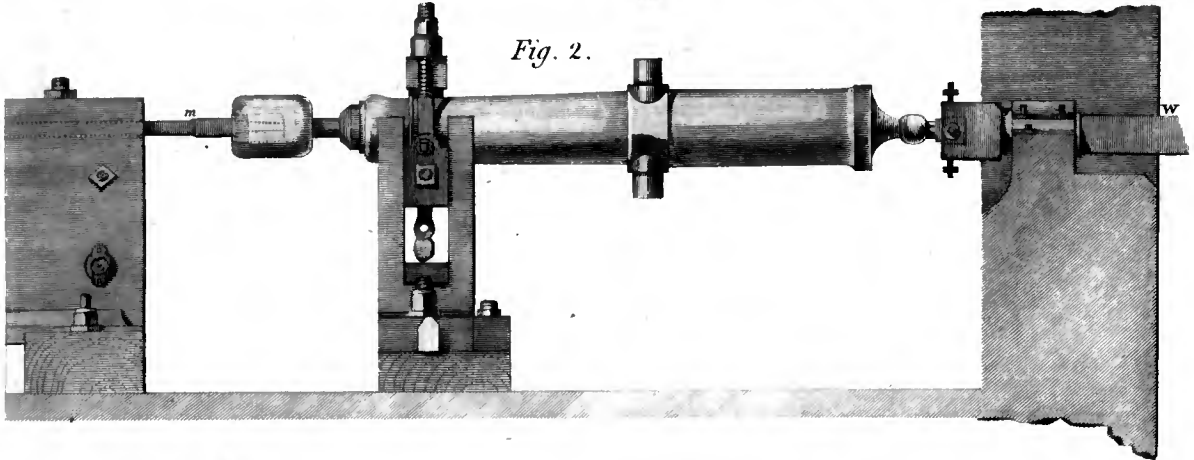
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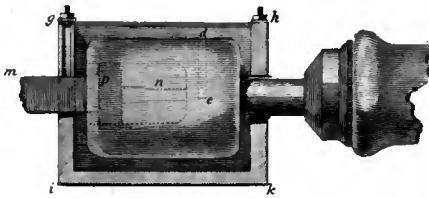
*Fig. 1.*



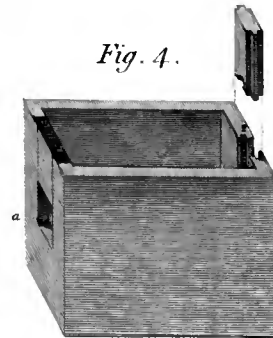
*Fig. 2.*



*Fig. 3.*



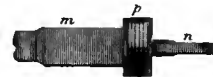
*Fig. 4.*



*Fig. 5.*



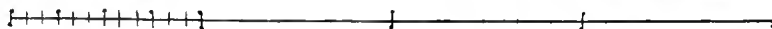
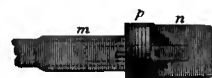
*Fig. 7.*



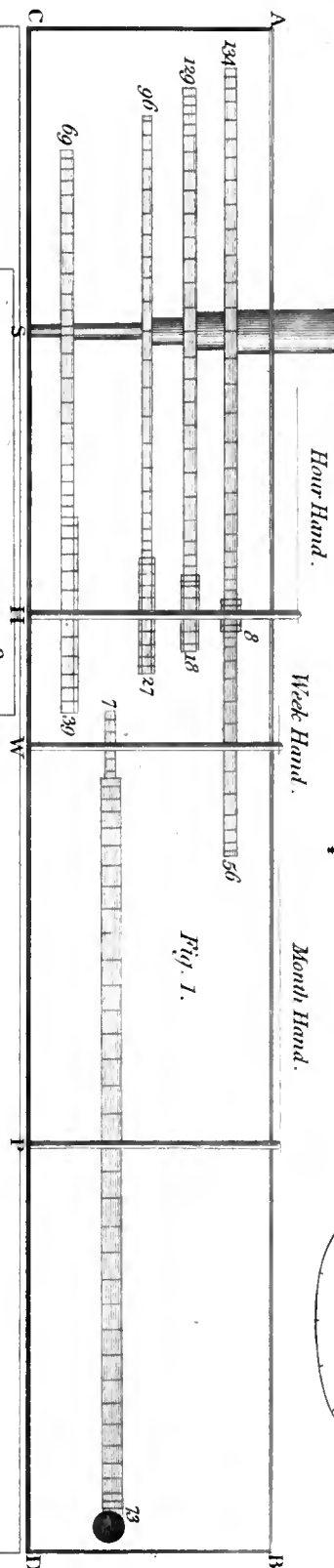
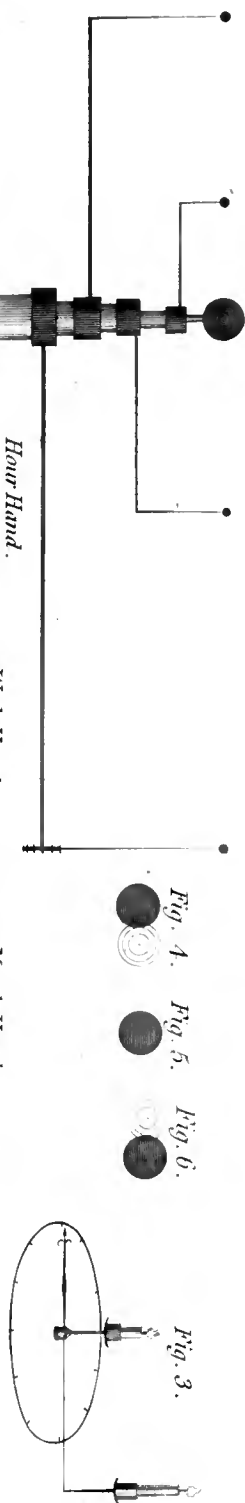
*Fig. 6.*



*Fig. 8.*









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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JULY 1798.

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ARTICLE I.

*Memoir on a New Metallic Acid which exists in the Red Lead of Siberia. By VAUQUELIN.  
Extracted from the Bulletin of the Soc. Philom.\**

**B**Y a new examination of the red lead of Siberia, Vauquelin is convinced that this mineral contains a metallic acid very different from all those which have hitherto been known. The following are the principal results of his experiments :

The red lead ore was reduced to fine powder, and boiled in a saturated solution of carbonate of potash. An effervescence of considerable duration was produced ; the powder was dissolved, but soon afterwards a precipitate fell down of a yellowish white colour. The solvent had assumed a beautiful golden colour.

The precipitate proved to be carbonate of lead.

Nitric acid was poured into the alkaline fluid till the excess of carbonate of potash was saturated. The fluid exhibited an orange-red colour. It was then mixed with a solution of tin, recently prepared, with which it assumed a brown colour, that afterwards became greenish. When poured into a nitric solution of lead, it immediately generated the red lead. By spontaneous evaporation, it afforded crystals of a beautiful orange red, besides those of the nitrate of potash.

The nitric acid being poured into the solution of the red crystals occasioned no precipitate ; but when, after evaporation to dryness, the crystals of nitrate of potash, which re-

\* This abstract is translated from the Journal de Physique, bearing date for Nov. 1794, but lately printed.

remained at the bottom of the capsule, were washed with alcohol, a blue liquor was obtained, which after evaporation left a greenish blue powder, soluble in water, of an acid taste, and reddening the tincture of turnsol.

The red lead ore may likewise be decomposed by muriatic acid. If the latter be diluted with water, the mineralizing acid is precipitated in the form of a red powder: if it be concentrated, it re-acts on the metallic acid, deprives it of part of its oxygen, and causes it to pass to a deep green colour, while vapours of oxygenated muriatic acid are disengaged.

These experiments are sufficient to prove that the mineralizing acid of the red lead of Siberia is a new substance; but as it has some resemblance with the molybdic acid, Vauquelin has made a number of comparative experiments on their alkaline compounds, which presented very evident differences. The following are the most remarkable:

1. The acid of Siberian lead ore is red, when combined with potash; the molybdate of potash is white.
2. The molybdate of potash affords a white precipitate with the nitrate of lead, whereas the red lead ore is regenerated of a beautiful orange colour, resembling that of the native ore when it is reduced to powder.
3. The molybdate of potash affords a white precipitate in flocks, when added to the nitric solution of mercury. The salt formed by the same alkali and the acid of Siberian red lead affords a precipitate of a deep cinnabar colour.
4. The former affords a white precipitate with the solution of silver; the latter a precipitate of the most beautiful carmine red, which changes to a purple red by exposure to light.

The foregoing experiments sufficiently prove that this new acid is metallic, and differs much from the molybdic acid. It does not differ less from the other newly discovered metals.

Uranium does not become acid, and cannot combine with the caustic alkalis.

Titanium is soluble in acids, with which it forms crystallizable salts, and does not combine with the caustic alkalis.

Tungsten becomes yellow in acids without dissolving, and affords white crystallizable salts with the alkalis.

The author has not pursued this comparative examination, because the properties of the other metallic substances are sufficiently known. He promises to continue his researches as soon as he shall have procured more of this mineral.

P. S. Since this memoir was read to the Institute, Vauquelin has reduced the mineralizing acid of the red lead ore. This metal is grey, very hard, brittle, and easily crystallizes in small needles. The nitric acid acidifies it with considerable difficulty.

H. V. C. D.

*Farther*

## II.

*Farther Experiments and Observations on the Affections and Properties of Light.* By HENRY BROUGHAM, jun. Esq.\*

HAVING laid before the Royal Society an account of a course of experiments † on light, in which I had been engaged, and also of the conclusions which these experiments had taught me to draw; I proceed in the following paper to relate the continuation of my observations; which, I hope, may not prove wholly uninteresting to such as honoured the former part with their attention. I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these four operations, which we have hitherto remarked. I shall then consider several phenomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

## I.

*Observation 1.*—THE sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shut, a prism, with its refracting angle (of  $65^{\circ}$ ) upwards, so that the spectrum was cast on a chart placed at right angles to the incident rays, and four feet from the prism.

In the rays parallel to the chart, and two feet from it, I placed a pin, whose diameter was  $\frac{1}{16}$  of an inch, and fixed it so that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed (similar to those described above ‡), all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated one or two, at about three inches from the shadow; and though (from the pin's irregularities) the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 1. Plate VII. divided in the direction R A, according to the colours of the spectrum in which they were formed; R O B A was red, and the broadest; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narrowest of all.

*Observation 2.*—Between the pin and the prism,  $\frac{1}{16}$  of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart half a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green,

\* Philosophical Transactions, 1797.

† See Philos. Journal i. 551. 535.

‡ Philosophical Transactions for 1796, page 240, or Philosophical Journal i. 357.

caused the images to be of all the colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions: they were largest when red, and least when violet.

*Observation 3.*—In case it may be thought that the sides of the hole, through which the rays passed in *Observation 2*, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction, and dilatation of the images by reflection, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides that the red image, when measured, was longer than the rest: and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

*Observation 4.*—I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow in the former rays than in the latter); and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to reflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end: they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism, and they disappeared; but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins: in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

*Observation 5.*—I held, in the direct rays of the sun at half an inch from the small hole in the window-shut, a glass tube, free from scratches and opaque veins, but, like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this  
tube



tube was slowly introduced into the light, and so held that none of the rays might be refracted, a streak, chiefly white, was seen, similar in shape and position to those described before \*. When narrowly inspected, it was found to contain many images by reflexion in it. But these were much diluted by the abundance of white light, reflected without decomposition in the manner above-mentioned †. This streak lay wholly on one side of the tube; but I moved the tube onward a little, and another streak darted through the shadow, and extended all round on both sides: and now, when the tube was in the middle of the rays, there were two streaks on both sides, one a little separated from the other and continued through the shadow, the other on each side of the shadow: the former was evidently produced by refraction: it contained many images very like those by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermost, and the rest nearest the point of incidence. Images similar to these are also producible on the retina, as mentioned before ‡.

*Observation 6.*—I now placed a prism at the hole, and made the same images by refraction, out of homogeneous light. These inclined to the red, not (like images by reflexion) to the violet; but they were broadest in the red, and grew narrower towards the violet parts. In short, when viewed beside the images by reflexion, except in point of brightness and inclination, they differed from them in no respect.

The three first experiments shew, that when homogeneous light is reflected, some rays are constantly disposed into larger images than others are, that is, into images more distended in length, though of the same breadth. The fourth experiment shews, that the same takes place when light is inflected and deflected; and the two last shew that the same happens when the rays are refracted in a way similar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to shew, that this difference of size is not owing to the different reflexibilities and flexibilities of the rays. In order to this we shall both demonstrate, and then prove by experience, “that inflexion and deflexion do not decompound heterogeneous rays, whose direction is such, that they fall on the bending body.” In fig. 2. let AB be the body, GH, EF, CD, the limits of its spheres of deflexion, inflexion, and reflexion, respectively; and let IP be a white ray of direct light entering at P the sphere of deflexion: through P draw LK at right angles to GH; IP will be separated into PR red, and PV violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the sphere of inflexion; and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is RPV, is likewise the difference of the two former. Suppose this difference equal to nothing; or that PV and PR are parallel; then rRS the angle of the red’s inflexion will be less than vVO the angle of the violet’s inflexion, by the angle RPV: (when not evanescent) add RPV to rRS; then rRS will be equal to vVO: that is, the divergence will be destroyed, and the rays enter the sphere of reflexion, parallel and undecomposed. It is evident, therefore, that the effect arising from the different deflexibilities of the rays is destroyed by the equal

\* Phil. Trans. 1796, page 236. or Philof. Journal i. 555.

† Ibid. p. 237. or Phil. Journal i. 556.

‡ Ibid. p. 243. or Phil. Journal i. 559.

and opposite effect produced by their different inflexibilities; and the same thing may in like manner be shewn to happen in the return of the rays from the body after reflexion. But let the rays be so reflected that they shall pass by the body without entering any more than one sphere of flexion; then they will be separated by their flexibilities, as we before described. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by separating the compound light. And indeed, this may be easily proved by fact. At 144 feet from the bending body, the greatest fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting surface: the one fort is therefore more than 144 times more distended than the other, whereas the flexion could, at the very farthest, only double them. Also the distinctness, and brightness, and regularity of the colouring, are quite different in the two cases: the supposed cause would neither account for the order of the colours, nor for their absence in common specular reflexion, and refraction through two prisms joined together with their angles the contrary ways. Lastly, if we suppose the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a prism should be decompounded, formed into several coloured images, and then refracted, the violet being least, and the red most bent; all which is perfectly the reverse of what actually happens. I have multiplied the proof of this proposition, perhaps beyond what is necessary; but its great importance to the whole theory will, I hope, plead my excuse.

Let us now suppose that a homogeneous beam passes through the spheres of flexion, it will follow that no divergence can take place from the bending power of the body; so that we have only to estimate the effect produced by the reflexion, and to enquire whether the different reflexibilities of the rays can cause the images to vary their sizes according as they are formed by different rays. In fig. 3. let AB be the body, CD the limit of its sphere of reflexion, and IP a beam of homogeneous rays, as red, incident at P and reflected to R, forming there the image Rr. It is evident that the greater reflexibility of the rays IP can only alter the position of the centre of Rr, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of Rr shews that a greater quantity of rays is reflected, or that the same quantity is spread over a greater space, and that in the following way. Let IF fi be a beam of violet-making rays entering ABCD, and reflected so as to form the image Rv. The force exerted by AB decreasing according to some law (of which we are as yet ignorant) as the distance increases, is not sufficient to turn the rays back till they have come a certain length within ABCD. But for the same reason it turns back all that it does reflect before they come nearer than a certain distance: between these two limits, therefore, the rays are turned back. But the limits are not the same to all the rays; some begin to be turned at a greater distance from the body than others, and consequently are reflected to a greater distance from the middle ray of the incident beam. Thus if IF fi be changed to a red-making beam, it begins to be turned back at f<sub>1</sub>, and the rays farthest from AB are reflected to r instead of to v, where they fell when IF fi was violet-making; not but that the same quantity of rays is reflected: the only difference is, that the most reflexible are reflected farthest from the body by their greater reflexibility, and farthest from each other by this other property. Exactly the same happens in the case of refraction, mutatis mutandis; but there seems to be a slight variation in the manner in which the different rays are disposed into images of different sizes by flexion. In this case also the bending

ing body's action reaches farther when exerted on some rays than when exerted on others : but then, the direction of the rays not passing through the body, those which are farthest off and at too great a distance to be bent, never coming nearer, are not bent at all ; and consequently as the least flexible rays are in this predicament at the smallest distance, and the most flexible not till the distance is greater, the images formed out of the former must be less than those formed out of the latter. This difference in the way in which the phenomenon appears, does not argue the smallest difference in the cause : it only follows from the different position of the rays, with respect to the acting body, in the two cases. I infer then from the whole, that different sorts of rays come within the spheres of flexion, reflexion, and refraction, at different distances, and that the actions of bodies extend farthest when exerted on the most flexible. It may perhaps be consistent with accuracy and convenience to give a name to this property of light ; we may therefore say that the rays of light differ in degree of refrangity, reflexivity, and flexibility, comprehending inflexity and deflexity. From these terms (uncouth as, like all new words, they at first appear) no confusion can arise, if we always remember that they allude to the degree of distance to which the rays are subject to the action of bodies. I shall only add an illustration of this property, which may tend to convey a clearer idea of its nature. Suppose a magnet to be placed so that it may attract from their course a stream of iron particles, and let this stream pass at such a distance that part of it may not be affected at all : those particles which are attracted may be conceived to strike on a white body placed beyond the magnet, and to make a mark there of a size proportional to their number. Let now another equal stream considerably adulterated by carbonaceous matter, oxygen, &c. pass by at the same distance, and in the same direction. Part of this will also be attracted, but not so far from its course, nor will an equal number be affected at all ; so that the mark made on the white body will be nearer the direction of the stream, and of less size than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the subjects in the situation described : the thing may easily be conceived, and affords a good enough illustration of what happens in the case of light.

Pursuant to the plan I before followed, I now tried to measure the different degrees of reflexivity, &c. of the different rays ; but though the measurements which I took agreed in this, that the red images were much larger than the rest, and the green appeared by them of a middle size, yet they did not agree well enough (from the roughness of the images, and several other causes of error) to authorize us to conclude with any certainty " that the action of bodies on the rays is in proportion to the relative sizes of these rays." This, however, will most probably be afterwards found to be the case : in the mean time there is little doubt that the sizes are the cause of the fact.

## II.

SEVERAL phenomena are easily explicable on the principles just now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the sun refracted through a prism, extending through all the seven colours, a very singular deception takes place : the body appears of different sizes, being largest in the red, and decreasing gradually towards the violet. This appearance seemed so extraordinary, that some friends who happened to see it as well as myself,

myself,

myself, suspected the body must be irregular in its shape. On inverting it, however, the same thing took place; and on turning the prism on its axis, so that the different rays successively fell on the same parts, the visible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexivity of the rays, and follows immediately from Observation 2d and 3d.

2. Sir Isaac Newton found that the rings of colours made by thin plates, and by thick plates of glass (as he calls them), when formed of homogeneous light, varied in size with the rays that made them, being largest in the most flexible rays. I have had the pleasure of observing several other sorts of rings, so extremely similar, and formed by flexion, that I can no longer doubt of this being also the cause of the phenomena observed by Newton. I shall first describe a species, to prove "that the colours by thick and thin plates are one and the same phenomena, only differing in the thickness of the plates." Happening to look by candle light upon a round concave plate of brass, pretty well polished, so as to reflect light enough for shewing an image of the candle, I was surprised to see that image surrounded by several waves of colours, red, green, and blue, disposed in pretty regular order. This was so uncommon in a metallic speculum, that I examined the thing very minutely by a variety of experiments: these I shall not particularly now describe, but give a general idea of their results.

It must be observed, for the sake of clearness, that in the following enquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line passing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the distance between its extreme colours, red and violet.

In the first place, they were formed by the sun's light in the figure of rings, surrounding the centre of the sphere to which the plate was ground, at greater distances increasing their breadths, the colours pretty bright, though inferior in brilliancy to those of concave specula.

Secondly, the order of the colours was in all red outermost, and violet or blue innermost, with a greyish-blue spot in the common centre of the whole; and on moving the plate from the perpendicular position, the rings moved and broke exactly like those of specula.

In the third place, homogeneous light made them of simple colours; they were broadest when red, narrowest when blue and violet.

Fourthly, they decreased in breadth from the centre; and I found by a simple contrivance, that they were to one another in the very same ratio that the rays by specula follow.

In the fifth place, I compared the general appearance of the two sorts by viewing them at the same time, and was struck with their general appearance, unless that these of specula were most vivid and distinct.

These things made me suspect that they were actually caused by the thin coat of gums with which the surface of the plate was varnished, called lacker. Accordingly I took it off with spirit of wine, and found the rings disappear; on lackering it again they returned; and in like manner I caused a well finished concave metal speculum to form the rings of which we are speaking, by giving it a thin coat of lacker. This is a clear proof that these  
rings

fringes were exactly the same with those of thick plates (to use Newton's expression); for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the two kinds are made by different sized plates. We cannot, therefore, distinguish them, any more than we do the spectrum made by a prism whose angle is  $90^\circ$  from that made by one whose angle is  $20^\circ$ . This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

### III.

IN reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following:

1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface.
2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion.
3. That they should also in the same case produce colours by reflexion.
4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light.
5. That there should be many more than three, or even four fringes by flexion, invisible to the naked eye. And,
6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these proportions is demonstrated a priori, is evident from the 4th figure, where CD is the reflecting surface, vo a concavity bearing a small ratio to CD, Ao and AB rays proceeding to CD. The one, AB, will be separated into Br red, and Bv violet, by deflexion from o, and will be reflected to r' v', forming there the fringes. The other, Ao, being reflected, will be separated into Bx and By, by deflexion from v, forming other fringes, xy, on the side of vo's shadow opposite to r'v'. Also when vo is convex instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when vo is a polished streak, images by reflexion will be produced, as described Phil. Trans. for 1796, p. 269. (Philos. J. i. 593.) The same passage will also shew the reason why, on great inclinations, colours by reflexion should be produced. And the second proposition, with respect to flexion, follows from what was demonstrated in this paper (p. 149 and 150); it being that case where the rays either leave or fall on the speculum at such an inclination as to come only within the sphere of inflexion, without being deflected. The

fourth proposition is merely a simple case of flexion. And the two last require no illustration. I shall now relate how I inquired into the truth of these things a posteriori.

*Observation 1.*—Looking at a plane glass mirror exposed to the sun's light, I observed that up and down its surface there were minute scratches (called hairs by workmen), and that each of these reflected a bright colour, some red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different position with respect to the mirror, I saw the species of the colours change; the red, for instance, became green, and the green blue. I applied my eye close to the mirror, and received on it the light reflected from one hair. I observed several distinct images of the sun much distended and regularly coloured, just like those described above; the same appearances were observable in all specula, metal and glass, which had these hairs, and I never saw any metal one without some: their size is exceedingly small, not above  $\frac{1}{10000}$  of an inch. Rubbing a minute particle of grease on the surface of the speculum, images were seen on the fibrous surface; and they always lay at right angles to that direction in which the grease was disposed by drawing the hand along it.

*Observation 2.*—Besides these polished hairs, many specula have fewer or more small specks and threads, rough and black. Perhaps every polished surface is studded with a number of small ones, invisible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telescope, a small concave speculum not very well finished; its surface shewed several specks to the naked eye, and many with a microscope. Its diameter was  $\frac{3}{8}$  of an inch, its focal distance two inches, and the sphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the sun, coming through a small hole  $\frac{1}{4}$  of an inch diameter, into a very well darkened room; I then moved it vertically, so that the rays might be reflected to a chart 12 inches from the speculum, and consequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vast number of dark spots; these were of two sorts chiefly, circular and oblong. Of the former a considerable number were distinct and large, the rest smaller and more confused, but so numerous that they seemed to fill the whole image. None were quite black, but rather of a bluish grey, and the oblong ones had a line of faint light in the middle, just as is the case in shadows of small bodies. But the chief thing which I remarked was the colours. Each oblong and round spot was bordered by a gleam of white, and several coloured fringes separated by small dark spaces. The fringes were exactly like those surrounding the shadows of bodies, of the same shape with the dark space, having the colours in the order, red on the outside, blue or violet in the inside—the innermost fringe was broadest, the others decreasing in order from the first. I could sometimes see four of them, and, when made at the edge of the large image, I could indistinctly discern the lineaments of a fifth: when two of the spots were very near one another, their rings or fringes ran into one another, crossing.

*Observation 3.*—When the chart was removed to a greater distance, as six feet, the fringes were very distinct and large in proportion; also the smaller spots became more plain, and their rings were seen, though confusedly, from mixing with one another. When the speculum was turned round horizontally, so that its inclination to the incident rays might be greater, the distance of the chart remaining the same (by being drawn round in a circle),  
the

the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig. 5.

*Observation 4.*—I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneous rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexibility is greatest.

[To be continued.]

### III.

*Enquiries respecting the Colouring Matter of Vegetables, and the Action of Metallic Substances and their Oxides upon them; together with a New Process for obtaining Lakes of the most intense and solid Colours. Read to the National Institute (of France) 15 Vendemiaire, in the Year VI. By the C. GURTON\*.*

**L**INNÆUS, the great naturalist of the north, had affirmed that the red colours of vegetables announce the presence of an acid. It was long ago observed, that the juice of the violet acquires a beautiful blue shade in vessels of tin, the use of which metal was recommended in dispensatories for the preparation of violet syrup; and the original colour of such syrups as had been changed by keeping, was restored by long digestion in tin. Little attention however was paid to the cause of these phenomena; and our associate, Berthollet, in his Elements of the Art of Dyeing, had pointed it out no otherwise than by conjecture, when he supposed an acid to have combined with the oxide formed at the surface of the tin.

Such was the state of our knowledge on this subject, when, from the striking difference of colour of two preparations of the same fruit, I undertook to examine the circumstances in which these changes take place.

I suppress the detail of experiments to which I subjected almost all the acid coloured fruits in succession, such as the strawberry, the gooseberry, the plum, as well as the petals of flowers, turnsol, fernambouc, turmeric, &c. by treating them comparatively in vessels of glass, of porcelain, of metal, and metallic alloys, or by keeping them in digestion on plates of metal perfectly cleaned, or upon metallic oxides. I shall confine myself at present to such results as may improve the theory of vegetable colours, or afford some useful applications to the processes of the arts.

These experiments prove that the red colour of fruits is manifestly owing to the real action of their peculiar acid upon their colouring matter.

That tin, when it brightens or restores the colour of violets, does nothing more than resume, by superior affinity, the acid which had caused it to turn red.

That tin or its oxide is not, as has hitherto been thought, the only metal which exercises

\* Translated from *La Decade philos. litt. et politique*, No. II. An VI. (Jan. 1798.)

this affinity ; but that lead, bismuth, antimony, and zinc, produce the same effect ; that it takes place more speedily and completely with iron ; and that the contact of all these metals produces a very perceptible violet tinge in such infusions, as without this circumstance are of a lively and decided red colour.

That the green and acid part of fruits does not contain the colouring principle which is disposed to become red with acids ; and that the coloured part retains in combination that portion of acid which is necessary to maintain the state of re-action that determines the shade.

That although this colouring principle is modified in certain vegetables so far as to resist acid or alkaline re-agents to a certain degree, as fernambouc with regard to acids, and turn-sol with regard to alkalis, yet it may be brought to this condition ; which seems to shew that it is, if not essentially yet at least originally, of the same nature.

That the metallic oxides are not all equally proper to seize and fix vegetable colours ; that some among them appear to attack them with more facility, while others retain them with very little power.

Lastly, that the new metal called tungsten, carried to the last degree of oxygenation, which has not hitherto been tried in this respect, has a decided advantage over all the other metallic oxides ; that it is capable of forming lakes of great value to painters, which perfectly resist the proofs of lime-water, of acetic acid or radical vinegar, of hydro-sulphureous gas, and even to a certain point the oxygenated muriatic acid gas, that enemy of colours, which burns them suddenly, and, according to the expression of Citizen Berthollet, represents in a few instants the combined action of air and light. We shall be less surpris'd at this assertion, when it is recollected that this is the only metallic oxide which eludes the solvent power of the three mineral acids.

The oxide of tungsten easily becomes charged with the colours of all vegetable matters. I have hitherto found no more than one exception in the petals of nic-ago, of which I have not been able to extract the fine purple red, without being yet able to suspect the cause of this difference.

In general, the lakes formed with this oxide become deeper instead of fainter when they are diluted. It is necessary to soften the shade. I have remarked that they acquire still more intensity when the oxide has been previously rendered blue by boiling it in vinegar.

One of our associates, Citizen Vauquelin, being informed of the object of my researches, directed my attention to aloes. Citizen des Fontaines had the goodness to procure me several kinds. I shall speak only of that which bears the name of foccitrine. I made experiments on this plant, which is one of the most rich in colour, though it does not exhibit the slightest appearance while the equilibrium of its principles is maintained by the energy of vegetable life. The woody fibre, which is the external part, then serves as the covering of a very viscid matter, of a greenish white colour, weakly acid ; but scarcely has this matter been exposed to the air before it assumes a very lively red purple colour, which becomes very abundant by the progress of fermentation. I have formed lakes of this matter with alumine, oxide of tin, and the white oxide of zinc. None of them were comparable to that prepared with tungsten.

I do not doubt but that the oxide of this new metal may likewise be useful in the composition of colours for dyeing, at least for the dyeing of silks, which are not intended to with-stand



stand alkaline proofs. The oxide of tin is useful in these processes, because it is not easily attacked by acids; but our oxide is absolutely insoluble.

I shall conclude by a reflection which may render these researches more interesting. Wolfram, from which this oxide is obtained, is found in the territory of the (French) Republic. Several mines are already known, and the French chemists have been for some years past employed in simplifying the processes by which it is separated from foreign substances\*.

We may therefore hope that the properties I have here described will supply artists with additional means of giving durability to the productions of genius, and may open a new branch of national industry.

## IV.

*Abstract of a Memoir on Camphor and the Camphoric Acid, read to the First Class of the National Institute of France. By BOUILLON LA GRANGE.*

[Concluded from Page 101, Vol. II.]

*Habitudes of Nitric Acid with Camphor.*

THE nitric acid has likewise a different action upon camphor from that which we have already mentioned.

*The Camphoric Acid.*

Kosegarten has informed us, that by distilling nitric acid eight times in succession from camphor, an acid is obtained which differs in its properties from the oxalic acid.

As these results have not been confirmed by experiment, and it has not been demonstrated that this acid is peculiar in its nature and its affinity, I have repeated the experiments of Kosegarten, of which we possess only a simple notice in a letter translated from Crell's Journal, and printed in the 27th volume of the Journal de Physique, page 298. The method of preparing this acid is as follows:

*First Process for preparing the Camphoric Acid.*

Take four ounces, or 122,284 grammes of camphor, which introduce into a glass retort, and pour one pound, or 489,136 grammes of nitric acid, at 36 degrees, or specific gravity 1,33, and adapt a receiver well luted. Place the retort on a sand bath, and apply a gradual heat. Much nitrous and carbonic acid gas are disengaged; part of the camphor rises, while another part seizes the oxygen of the nitric acid. When the vapours cease to rise, unlute the vessels; return the sublimed camphor into the retort, pour thereon another pound of the acid, and distill a second time. This operation must be repeated until the camphor is totally acidified. Four pounds and fourteen ounces of this nitric acid are sufficient to acidify four ounces of camphor.

When all the camphor is acidified it crystallizes in the remaining fluid. The whole must then be poured on a filter after previous decantation of the acid, and distilled water

\* Journal des Mines, No. XIX. G. Our Cornish mines also afford the ores of tungsten. N.

must be thrown on the crystals in order to clear them of the portion of nitric acid which may adhere to their surface. The most certain indication that the camphor is acidified, consists in its crystallization by the cooling of the fluid which remains in the retort.

*Purification of the Camphoric Acid.*

This acid is purified by solution in hot distilled water, filtration and evaporation of nearly half the fluid, or till the period at which a slight pellicle is formed. The crystals of camphoric acid separate by cooling.

*A Second Process.*

Another method of procuring the camphoric acid consists in the use of nitric acid at 50 degrees, or specific gravity 1,532. This process is much speedier than the former, but its inconveniences are such as do not entitle it to be preferred to the other. In fact, the camphor is more speedily attacked by the concentrated acid; but instead of subliming, it passes over into the receiver, and a portion is likewise carried off in the gas which escapes. These circumstances occasion a real loss in the product, as may be seen by the following tables of the results:

By the first method.					grammes.
Nitric acid, at 36 degrees, 4 lb. 14 oz. or	-	-	-	-	2114,538
Camphor, 4 oz. or	-	-	-	-	122,284
Acid obtained, 14 gros, or	-	-	-	-	53,498
By the second method.					
Nitric acid, at 50 degrees, 2 lb. 12 oz. 2 gros, or	-	-	-	-	1352,168
Camphor, 4 oz. or	-	-	-	-	122,284
Acid obtained, 9 gros, or	-	-	-	-	34,392

S E C T. VII.

*Camphor and Oxygenous Gas.*

BEING desirous of knowing how camphor would be affected with oxygenous gas, I made the following experiment:

Oxygen gas was obtained from the superoxygenated muriate of pot-ash. After an inverted glass vessel was thus filled, it was transferred to the trough containing mercury, and a small portion of water was passed to the surface of the metallic fluid.

On the other hand, a small piece of camphor with a particle of phosphorus was placed in a small cupel. A tube was then bended in such a manner and applied, that one of its extremities was placed beneath the glass jar, and the other in a pneumato-chemical trough beneath a jar filled with water.

In this disposition of the apparatus the phosphorus was set on fire by means of a red-hot iron. The inflammation was communicated to the camphor. Much caloric was disengaged with a very brilliant flame; the inner surface of the vessel became covered with a black matter, which was detached by degrees, and floated upon the water over the mercury. A gas was collected at the same time, which exhibited all the characters of carbonic acid gas.

This experiment therefore confirms the result in Section II. for it cannot be doubted but that the matter in the retort was a true carbon.

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The water which had been placed under the jar became very odorant, and its smell was absolutely the same as that of the oil obtained by the processes already described. It was acid, and reddened the tincture of turnsol. Lime water ascertained the presence of carbonic acid in this water; but upon adding an excess of the carbonic acid the precipitate was not taken up; which fact led to a suspicion that another acid was present, and in fact a calcareous camphorate was obtained.

I observed that it was necessary to add a small portion of phosphorus to the camphor, which otherwise would not have taken fire. For an ignited body only dissipates it in vapour, and it cannot be inflamed but by the contact of a body at a much more luminous or higher degree of heat.

## S E C T. VIII.

### *Characters of the Camphoric Acid.*

THE camphoric acid has a slightly acid bitter taste, and reddens the tincture of turnsol. It is crystallizable, and a mass of its crystals resembles the muriate of ammoniac. By exposure to the air it effloresces. Cold water dissolves it with difficulty. One ounce of water at the temperature of 10 or 12 degrees of Reaumur, takes up no more than six grains; whereas at the boiling heat it dissolves 48 grains. When this acid is placed on ignited coals it emits a dense aromatic smoke, and is entirely dissipated. By a gentler heat it melts and is sublimed.

If the camphoric acid be put into a porcelain tube (heated), and oxygen gas be passed through, the acid does not undergo any change, but is sublimed.

By mere distillation our acid first flows and then sublimes; by which process its properties are in some respect changed. It no longer reddens the tincture of turnsol, but acquires a brisk aromatic smell; its taste becomes less penetrating, and it is no longer soluble either in water or the sulphuric and muriatic acids. Heated nitric acid turns it yellow and dissolves it. Alcohol likewise dissolves it; and if this solution be left in contact with the air of the atmosphere it crystallizes.

Camphoric acid does not produce any change in sulphur; alcohol and the mineral acids totally dissolve it; and so likewise do the volatile and the fat oils. It forms combinations with earths, alkalis, and metallic substances. We shall give a more particular account of these saline combinations in another Memoir.

Among metallic solutions, it decomposes only the sulphate and the muriate of iron.

It produces no change in the solution of indigo by sulphuric acid, nor in the tincture of nutgalls. It has no action on lime-water.

*Conclusion.*—From all these facts it follows, that camphor is a volatile oil rendered concrete by carbon; that these two distinct products may be obtained by the medium of alumine; and lastly, that by treating camphor with the nitric acid, another peculiar acid is obtained which differs from all the known vegetable acids,

1. By its crystallization.
2. Its sparing solubility in cold water.
3. Its burning without leaving any residue.
4. Its not precipitating lime-water.
5. Its producing no change in the sulphuric solution of indigo—a difference which remarkably distinguishes it from the suberic acid which turns the same solution green; and
6. By the formation of peculiar salts, every one of which exhibit a blue flame with the blow-pipe.

## V.

*An Account of several new Experiments on Heat, with occasional Remarks and Observations; and Conjectures respecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. By BENJAMIN, Count of RUMFORD\*.*

AT the end of a French Translation of the first edition of Count Rumford's Seventh Essay, by Professor Piçtet, that Translator added the following extract of one of the Count's private letters to him, dated June 9, 1797: "I should have been much surprised if my "Seventh Essay had not interested you; for in my life I never felt pleasure equal to that I "enjoyed in making the experiments of which I have given an account in that performance. You will perhaps be surprised when I tell you, that I have suppressed a whole chapter of interesting speculation, merely with a view of leaving to others a tempting field of "curious investigation *untouched*, and to give more effect to my concluding reflection, "which I consider as being by far the most important of any I have ever published." As these assertions were not originally intended for the public eye, the worthy author found his situation altered with regard to the philosophical world by this communication; and with a very honourable degree of delicacy conceived that it became his duty to let the public know with precision how far he had carried his enquiries in the investigation of the subject of that Essay, instead of giving obscure hints of important facts kept in reserve, and apparently to be brought forward, when others might make discoveries of the same kind. The work before us contains the matter in question.

The experiments and observations contained in this Second Part are as follow:

CHAP. I. When a quantity of water was frozen in a glass jar by placing the vessel in a freezing mixture, it was always observed that, as the ice first began to be formed at the sides of the jar, and gradually increased in thickness, the portion of water in the axis of the jar which last retained its fluidity, being compressed by the expansion of the ice, was forced upwards towards the end of the process, and formed a pointed projection or nipple, which was sometimes above half an inch higher than the rest of the upper surface of the ice. This fact induced the Count to make experiments relative to the defect of conducting power in fluids downwards. For, if a fluid be poured upon this cake of ice so as to cover the whole mass, and a heated solid be then suspended at a small distance from the papillary protuberance, it is evident that this last, if melted, will derive its fluidity from the transition of heat, through the fluid of which the conducting power or its absence was meant to be ascertained.

In an experiment with fine olive oil, the cake of ice or congealed water was three inches thick, four inches and three quarters in diameter, and the pointed projection rose half an inch above the upper surface. The temperature of the apartment was 31 degrees of Fahrenheit, and the external part of the jar as high as the ice was surrounded with a mixture of pounded ice and water. In this situation of the apparatus, fine olive oil, previously cooled to 32 degrees, was poured into the jar till it stood at the height of three inches above the surface of the cake of ice.

\* Abridged from the Second Part of his Seventh Experimental Essay.

A solid cylinder of wrought iron  $1\frac{1}{4}$  inch in diameter, and 12 inches long, provided with a hollow cylindrical sheath of thick paper, was heated to the temperature of 210 degrees in boiling water, and, being suddenly introduced into its sheath, was suspended from the ceiling of the room, and very gradually let down into the oil, until the middle of the flat surface of the hot iron, which was directly above the point of the conical projection of ice, was distant from it only 2-10ths of an inch. The end of the sheath descended 1-10th of an inch lower than the end of the hot metallic cylinder. The ice was perfectly visible through the oil in every stage of this experiment. How long the metallic cylinder was kept immersed in the oil is not said, but the time was no doubt sufficient to satisfy the Count that heat is not propagated downwards through this fluid. The ice was not in the smallest degree diminished, or otherwise affected, by the vicinity of the hot iron.

A similar experiment was made with a fresh cake of ice in the same jar, but with ice-cold mercury covering the cake to the height of about an inch. The surface of the mercury in the jar was cleaned with blotting paper, after which the whole was suffered to remain quiet for about an hour, when the hot cylinder of iron was very carefully introduced, and suffered to remain several minutes at the distance of a quarter of an inch from the point of the conical projection of ice.

In this as well as in the other experiment, the cylindrical sheath was made to project 1-10th of an inch below the base of the iron, for the purpose of diminishing the internal motions of the fluid.

In this experiment also the ice remained unchanged. In order to shew whether the ice were really in the state of melting with the least possible addition of heat, the Count touched it with his finger beneath the mercury; and he found that this operation could not be so speedily performed, but that signs of water having been produced became apparent on the clean and bright surface of the mercury.

From the results of these experimental investigations, it appears to our author that water, oil, and mercury, are perfect non-conductors of heat; and that when either of these substances takes the form of a fluid, all interchange and communication of heat among its particles, or from one of them to the other directly, become from that moment *absolutely impossible* \*. In the Philosophical Transactions for 1792, the Count has shewn the extreme imperfection of the conducting power of air, and in his Sixth Essay he has shewn how much reason there is to conclude that the particles of steam and of flame are in the same predicament: From all which circumstances he is disposed to conclude, that it is common to all fluids, and even essential to fluidity, that they shall not immediately or directly conduct heat from particle to particle.

This important circumstance was applied by our author, in the Philosophical Transactions above referred to, to account for the warmth of natural and artificial clothing, and of snow, as well as to explain various phenomena of winds. And in his Sixth Essay he availed himself of the non-conducting power of steam and of flame, to explain the effects of the blow-pipe, and investigate the most advantageous forms for boilers. And, lastly,

\* On the absolute impossibility of the transition of heat through fluids at rest, see the note in our Journal, I. 291. It may however be observed, that most of the Count's conclusions will be practically true; that is to say, with regard to such terms of time as his experiments justify. N.

in the third chapter of the present Essay, he has extended the application of his discoveries to the means which appear to have been used by the Creator of the world, to render the temperatures of different climates more nearly equal by means of the salt water, which covers so large a part of the surface of the globe. But the most interesting application remains to be made of this doctrine to chemistry, vegetation, and the animal economy; to the learned in which branches of science he begs leave most earnestly to recommend them. From his meditations he thinks it not improbable that every change of state, in every kind of substance, may be owing to heat alone; that every concretion is a true congelation effected by cold, or the diminution of heat; that every change from a solid to a fluid form is a real fusion;—and that it may be found, that the apparent violence with which certain solids are attacked by their solvents, is not owing to any particular or elective attraction, but to the considerable degree of heat or cold, and the great difference of specific gravity which ensues in the solvent, from this cause as well as from the subsequent change produced by combination.

If fluids be non-conductors of heat, it will necessarily follow, that change of temperature will produce currents in every chemical solvent, in proportion to the change of specific gravity; and the rapidity of the process of solution will be proportioned to that of the currents, or to the change of temperature. And again, if the saturated solution be either heavier or lighter than the solvent itself, currents will be produced from this cause also, which will tend to render the solution more or less rapid, according to the magnitude of this difference, and its direction with regard to the other difference caused by the change of temperature. An instance of these two causes operating jointly is adduced in the solution of common salt in water. If this solid be supported in a perforated vessel under water, but near its surface, the solution will be most rapid: first, because the temperature is diminished, and consequently the water is condensed in the process; and, secondly, because the solution of salt in water is itself more dense than the water itself. On both accounts therefore the brine will rapidly descend, and fresh portions of the solvent will continually be brought in contact with the salt.

On this occasion the author proposes a curious question: *Whether, in a case where the expansion by heat were equal to the condensation by the process of chemical union, the effect of solution could take place?*—He answers by observing, that if chemical attraction, as has been generally supposed, really exists and operates in the way of predilection beyond the point of actual contact, it is probable that the solution would take place.—But if this attraction be nothing more than has been just pointed out, it would follow that though solution would not be absolutely impossible, yet it would be so slow as hardly to be perceptible.

In the consideration of the solution of salt in water, the position of the solid near the surface of the fluid was stated as a circumstance essential to its rapidity. The uniform diffusion of solids through the whole mass of lighter fluids which dissolve them, has usually been considered by chemists as a proof of attraction between the two bodies operating at a distance from the place of contact. It seems to be a striking result of the Count's reasoning, that if no other causes should interfere but those which flow from the mutual combination of the two substances, the salt would not, at least in any moderate portion of time, be carried to the upper part of the fluid resting upon it. He made this the subject of a particular experiment.

A cylin-

A cylindrical glass jar  $4\frac{1}{2}$  inches in diameter, and  $7\frac{1}{4}$  inches high, was placed in the middle of another cylindrical glass jar  $7\frac{1}{2}$  inches in diameter, and eight inches high, which stood in a shallow earthen dish, nearly filled with pounded ice and water. This apparatus was placed in an uninhabited room, where the temperature was constantly at about 36 degrees of Fahrenheit. In this situation, a quantity of pure ice-cold water, slightly tinged with turnsol, was poured into the inner jar, to the height of more than two inches; and then by means of a glass funnel, which ended in a long narrow tube, by introducing this tube into the fresh water, and resting it on the bottom of the jar, a quantity of the strongest clear ice-cold brine of common salt, equal to that of the fresh water, was poured very slowly in. The difference of colour rendered the two fluids very distinguishable, and shewed that they were not disposed to mix together.

The space between the two jars was then filled with large fragments of ice and ice-cold water, because pounded ice would have obstructed the view; and when this was done, the tinged water of the interior jar was carefully covered with ice-cold olive oil to the height of about an inch. This last fluid served to prevent the water from being agitated by the air, or cooled by evaporation or the communication of heat.

The fluids remained in perfect tranquillity, without the smallest disposition to mix together, during four days. At the end of that time the smallest jar was removed without agitation, and placed in the window of a room heated by a German stove. In less than an hour it was perceived that the brine and the tinged fresh water began to mix, and at the end of 24 hours they were intimately mixed throughout.

The author leaves philosophers to deduce their own conclusions from this experiment; but in the mean time points out a result, which is not only curious in itself but capable of affording important consequences. He thinks there are strong reasons to conclude, that were a lake but very deep, its waters near the surface would necessarily be fresh, even though its bottom should be one solid mass of rock-salt. He suggests the advantage which might accrue to an inland country where salt is scarce, if on experiment it should be found that the water at the bottom of some deep lakes is salt. And that the water at the bottom of all very deep lakes ought necessarily to be salt, even in situations where no mines of salt exist, appears to him probable, from the geological facts which indicate that most of our continents have been covered by the waters of the ocean. If ever that event happened, he thinks it highly probable, that the salt water left at the bottoms of all deep lakes by the sea, on its retiring, must be there now.

CHAP. II. In the former part of this Essay, the author has availed himself of all the circumstances which accompany the cooling and congelation of water, to account for the striking effects which they produce in the economy of heat on the surface of the globe. As water contracts by cooling to the 41st degree, and afterwards expands until it assumes the solid state; it should seem at first consideration, that, when the heat is abstracted from the bottom of a vessel containing this fluid, the particles of water would rise and elude the refrigerating power until the whole mass was cooled to that temperature, and afterwards continue to circulate in the same manner as if the temperature were actually rising, until at length the whole mass having arrived at the temperature of 32°, it would become solid almost instantaneously. But these inferences suppose the particles to circulate with less impediment from inertia and resistance than would be sufficient to detain them long enough at the lower

surface to effect their congelation. Whether this supposition may agree with the actual state of things or not, might perhaps be ascertained by reasoning from former facts; but in all such questions where the decision can be immediately and simply had from experiment, it is certainly best to apply to that source of information. Count Rumford made the trial, by pouring mercury into a glass tumbler to the depth of one inch, and upon this about the same bulk of water. Both fluids were at the temperature of 60 degrees. The tumbler was then placed in a freezing mixture of snow and common salt, which reached no higher than the upper surface of the mercury. The ice was formed at the bottom in contact with the mercury, not only under these circumstances, but also when the experiment was varied by previously cooling the mercury to about ten degrees, and then gently pouring boiling hot water on its surface. This last fluid was instantly frozen, and gradually formed a thick cake of ice, covering the mercury, though almost the whole of the mass of the unfrozen water which rested on this ice remained nearly boiling hot.

Among the inferences deduced from this experiment, Count Rumford makes one, to account for the formation of ice at the bottom of rivers, which he thinks can only take place in such streams as do not constantly fill their bed, but occasionally overflow portions of ground cooled by the atmosphere below the freezing point\*. Another important inference from the same facts is, that it is impossible any fluid should be of the same temperature while exposed to light, though its mass be ever so small, and that at the difference of heat must occasion perpetual motions among its parts. This consequence is very fully explained in detail by our author, who considers fluidity as the life of inanimate bodies, and congelation as the sleep of death; and is thence disposed to reject altogether the attribution of attractive powers or exertions of any kind to dead motionless matter. He extends his meditations to the vital principle in living animals, and demands whether their life also do not depend on the internal motions in their fluids occasioned by an unequal distribution of heat? and whether stimulation be not in all cases the mere mechanical effect of the communication of heat?—The ancient hypothesis, that the life of an animal resides in its blood; the evident tendency of respiration, digestion, and insensible perspiration to produce and perpetuate inequalities of tem-

\* I am not acquainted with the peculiar circumstances under which ground ice is formed; but it is certainly possible that such an event should happen in a stream which constantly fills its banks. Suppose a stream to flow with very little agitation in contact with an atmosphere eight degrees or more beneath the freezing point. It is known that water whose parts are relatively at rest, or nearly so, may be cooled about eight degrees below 32° without assuming the solid state. The middle of our stream might therefore continue fluid when so cooled, and the congelation would only take place at the sides, where the friction against the banks would cause the requisite internal agitation. The middle cold stream, on account of its expansion beneath 41 degrees, would occupy the surface, and consequently could not rub against the bottom except in shallow places, or unless some means were to offer of sinking it. Suppose one or more springs rising from a great depth in the earth, and thence possessing the mean temperature of the climate, to flow into the principal stream. This mass of warm water would occupy the superior part of the stream, and cause the cold mass to descend; and wherever this touched the bottom it would be agitated, and form a coat of ice. These effects would be governed by the temperatures, the masses, and the local requisites, for which upon the whole we are in want of observations. As to the assumed facts, they are undoubtedly possible. A remarkable instance of a warm stream in winter is seen in the small river Wandle, which bursts out of the earth near Carshalton in Surrey, and after flowing with considerable velocity over a line of ten miles, and giving activity to thirteen mills, falls into the river Thames at Wandsworth without having had time to acquire the freezing temperature, even in the severest weather. N.



perature; the effects of heat and cold introduced into the system in liquid foods, with various other physiological events, are adduced by our author in support of the probability of that doctrine which he recommends to the attention of philosophers.

CHAP. III. In the third chapter, the Count proceeds to point out a variety of striking consequences and observations which flow from the imperfect conducting power of fluids. One of the most immediate of these is the very great degree of heat which may exist at a small distance from other bodies intensely cold. Thus it is found that ice evaporates while exposed to an atmosphere in which the thermometer stands far below 32 degrees; an event which he rationally accounts for, by supposing that some of the particles of air which come into contact with the ice are so hot as not only to melt those particles of ice which they happen to touch, but also to reduce part of the generated water to steam before it has time to freeze again; or otherwise by supposing the same effect to be produced by the intense heat generated from the absorption of light by small projecting points of the ice. He even thinks the metals would evaporate if they were bad conductors of heat, instead of being very good conductors as in fact they are; and in proof of this he urges the fact, that mercury, which from its fluidity is what he would call a non-conductor, is known to evaporate by the mere heat of the atmosphere.

That the most intense heat is often excited in the midst of masses of cold liquids, is not to be doubted. The sun's rays generate heat of extreme intensity; but when circumstances are not favourable to its accumulation it is soon dispersed, and leaves no traces of its existence which can be measured either by instruments or the organs of sense. The Count does not think it improper to infer, that the heat excited by a ray of light in an indefinitely small particle of solid and opaque matter floating in a mass of cold water, may be equally intense with that which is generated in the focus of the most powerful burning mirror or lens. Hence he accounts for various effects of the sun's light, which gradually produce changes of the same nature as those which arise from very elevated temperatures. Thus wood is rendered brown or superficially charred, luna cornea is rendered black, or as may be supposed superficially reduced, metallic oxyds are deprived of oxygen, the green leaves of vegetables emit the same fluid, &c.

Among other familiar instances of intense heat, in circumstances where no visible signs appear, the Count adduces that of the ascending current of air from a candle. Iron is fully red hot at the temperature of about 1000° of Fahrenheit's scale; brass melts at 3807°, copper at 4587°, silver at 4717°, and gold at 5237°; and it must be obvious that this last temperature obtains where gold undergoes fusion. But fine gold, silver, or copper wire flattened, such as is used to cover thread to make lace, melts instantly on being held in the flame of a candle, or even if held for a few seconds over the flame at the distance of an inch, where there is no appearance of fire or ignition. The air, or many of its particles, must therefore be heated to this intensity, though their number may be insufficient to cause any very elevated degree of temperature in a large mass of metal or a thermometer.

From this hypothesis of intense heat in the small parts of fluids, or bodies suspended in them, it will follow that chemical solutions and precipitations are or may be effected by the sole agency of heat, and will not differ from fusion and congelation;—that the points of temperature at which bodies assume the solid, the fluid and the gaseous states will be of the utmost consequence with regard to these events, and that perhaps there may be no other essential difference.

difference between one body and another but what arises from this circumstance;—that these points being changeable by combination, give rise to a prodigious number of consequences different from those which obtain in the simpler elements. We must refer to the original Essay for the more ample elucidation of these points, where chemical philosophers will see the outline traced of an immense field of research, which, if it should not include the whole of the phenomena which have been designated under the name of elective attraction, will certainly comprehend a large mass of effects which never fail to present themselves in every natural change which comes under our notice.

CHAP. IV. The last and concluding chapter contains a variety of miscellaneous experiments. 1. If a thermometer with a long cylindrical bulb being at the temperature of the air in summer, or any temperature above the freezing point, be plunged to half the length of its bulb in ice and water, the mercury will fall in the tube only half as much as if the whole bulb had been immersed. This experiment succeeds equally well, when the superior half of the bulb is covered with a sheath lined with soft fur to prevent the communication of heat from the air during the experiment. It shews that the upper half of the mercury is not cooled, or, in other words, that heat does not pass downwards in fluids.—2. Ice-cold water standing on ice at the bottom of a thin tube of glass, may be boiled by holding the tube inclined over the flame of a candle applied near the upper end, and gradually removed lower down as the ebullition proceeds. In this way all the water in the tube may be brought into the most violent ebullition, to within a quarter of an inch of the ice, before this begins to be melted.—3. The radiant heat from a red hot iron bullet was not found to make its way downwards through liquid water, nor through melted tallow, nor melted wax. The experiment was tried on a thermometer about a quarter of an inch beneath the surface of each of these fluids. These facts are analogous to the observation of Scheele, who found that radiant heat does not pass through glass. When the red-hot ball was held over a cake of ice, the heat appeared to be transmitted by communication, though very slowly. The excavation in ice was deepest near the middle. The contrary was the case both in tallow and in wax. Count Rumford does not directly explain the cause of this remarkable difference. It is the only immediate fact I recollect having seen in his Essays, in proof of the assertion that water stands alone, with regard to the property of expanding in the lower degrees of refrigeration; and consequently of contracting as its temperature increases in that part of the scale. From this property it would follow, that a warm stream of water must descend immediately beneath the bullet, while an ascending current obtains near the circumference of the cavity. But if the expansion of tallow be uniform from the earliest point of fusion, the heated fluid will not descend, but will spread out sideways, and circulate in a contrary direction to the current in water; while the ascending stream of cold fluid will be near the centre, and will defend part of the tallow in that region from being fused.—4. Beautiful crystals of sea salt were formed in brine standing on mercury in an open vessel for half a year. The Count makes it a question, whether the mercury contributed in any manner to this effect? It seems probable that it did not, and that evaporation equally slow and undisturbed would have afforded the same result in other circumstances.—5. A similar remark may be made on his observation of olive oil rendered colourless by exposure to the air for six months upon brine, in a place where the sun's rays never entered. The same effect happens to olive oil which remains in unclosed vessels in work-shops.—6. Part of the last-men-

tioned

tioned colourless oil being imperfectly congealed, an attempt was made to cause radiant heat to descend through its mass. It was expected that the transparent or melted portion of the oil would in this case have been hemispherical, or at least of some convex figure; but this did not happen, for the surface beneath the fluid or melted part continued flat. Whence the Count infers, that the fusion was effected merely by the heat absorbed or generated by the sides of the tumbler.

7. The concluding experiment of this Essay affords a striking result of the circulation of fluids under different circumstances. When the instrument described in our Journal, I. 342, was placed by accident in the window of a room warmed by a German stove, the difference of temperature, on the side nearest the window from that opposite the internal part of the room, was such as to keep up a constant circulation of the fluid. This event induced the Count to substitute a box of plate glass in the place of one of the panes of his window. This apparatus, which was 13 inches high,  $10\frac{1}{2}$  inches wide, and one inch within from plate to plate, was half filled (and as I suppose afterwards completely filled) with the saline solution with pieces of amber floating in it, as described at the place last quoted. The Count expected to behold the currents as usual in this new vessel; but to his great surprise they proved not vertical, but horizontal, exhibiting actual winds in opposite directions, which springing up in the different regions of this artificial atmosphere, prevailed for a long time with the utmost regularity, while the small particles of the amber collecting themselves together formed clouds of the most fantastic forms, which being carried by the winds rendered the scene perfectly fascinating.

Several subordinate circumstances gave these motions a more intimate resemblance to the atmospherical phenomena. One remarkable appearance never failed to present itself regularly every day during the three weeks that the experiment was continued. The clouds, after having been driven about all day by the different currents, in the liquid (of which there were sometimes as many as six or seven running in opposite directions at the same time), never failed to collect themselves together in the evening into large masses, sometimes forming only one, and sometimes two or three strata at different heights, where they remained to all appearance perfectly motionless during the night.

The experiment was put an end to by the accidental breaking of the vessel.

## VI.

*An Account of certain Motions which small lighted Wicks acquire when swimming in a Basin of Oil; together with Observations upon the Phenomena tending to explain the Principles upon which such Motions depend. By PATRICK WILSON, F.R.S. Edin., and Professor of practical Astronomy in the University of Glasgow\*.*

DEAR SIR,

Glasgow College, April 28, 1795.

I NOW sit down to give you some account of the little hydrostatical lamp which I so briefly mentioned to you in a former letter. As I am far from being sure whether what I

\* In a Letter to Mr. John Playfair, F. R. S. Edin. &c. inserted in the Edinburgh Transactions, vol. iv.

have to offer upon this subject may be entitled to the notice of the Edinburgh Royal Society, so I will refer this point to your determination after you have had leisure to consider the contents.

The phenomena treated of in the sequel were quite new to me a few months ago, and, so far as I know, have not hitherto been attended to or described by any body else. What I have called the hydrostatical lamp, consists of a small circular patch of common writing paper, about three-eighths of an inch in diameter, having about a quarter of an inch of soft cotton thread standing up through a puncture in the middle to serve as a wick; and the phenomena in question are certain motions which such minikin lamps acquire when lighted and made to swim in very pure fallad oil.

A shallow glass basin, with sides rising nearly perpendicular, or a common glass salver, will conveniently contain the oil for these experiments. As soon as the lamp is lighted, it will immediately sail briskly forward in some direction till it meets the side of the vessel, and afterwards will take a circular course, always bearing up to the sides, and so will perform many revolutions.

Sometimes the circulation is from right to left, and sometimes in the contrary direction, according as that point of the paper base, which in the direct sailing kept always foremost, turns away from the side of the glass, a little to the right or to the left hand of that which comes to be the point of contact. This turning away of what may be called the leading point of the base is distinctly observable by a partial rotation of the lamp round the wick as an axis, as soon as it arrives at the side of the vessel. Sometimes, though rarely, the leading point itself attaches itself to the side, and forms the vinculum, in consequence of the well-known corpuscular attraction between the elevation of oil around the base and that belonging to the sides of the glass; and when the vinculum so corresponds to the leading point, the lamp will be found to stand still without any tendency to circulate.

When the little wick has any sensible eccentricity upon the circular paper base, the lamp will sail so as to make that part of the base which lies nearest to the wick the stern; and if the base of the lamp be clipped to an oval form, and the wick placed in the longer axis eccentric, that end of the base nearest the wick will also keep hindermost, when the lamp sails across the salver. In the same manner, if there be an equilateral triangle, having its wick in the perpendicular which bisects any of the sides, either the vertex or side will become the stern, and keep hindermost, according as the wick is placed nearest the one or the other. Lamps so constructed are found also to circulate upon their arrival at the side of the vessel, when the leading point turns away from the glass, as it commonly happens.

Whatever be the cause of the sailing of the lamp directly forward, the perpetual circulation after it arrives at the side seems to proceed from the force which formerly impelled it still acting in the same manner, but in a direction inclined to that of the corpuscular attraction which forms the vinculum; and it is evident that this inclination will be greater or less according as the leading point is more or less averted from the glass. When it so happens that the leading point and vinculum coincide, it should seem that both forces just now mentioned must urge the lamp in a direction perpendicular to the side of the glass; in which case it must stand still, agreeable to observation.

The next thing which I had occasion to take notice of when the lamp sailed in a direct course, was a seemingly very active repulsion between its stern and the oil at the surface con-

tiguous

iguous to it. This became manifest when very fine charcoal dust was lightly scattered around the lamp. As it then proceeded in its course, it marked out a spreading or diverging *wake* behind it entirely clear of all dust, in consequence of the particles being chased backwards and laterally with a motion much more than merely relative.

Desirous of learning how this disposition of the dust would take place when the lamp was stationary, I constructed one of a fine wafer, and with an excentric wick consisting of a soft cotton thread doubled; and to prevent the wafer or base from catching fire I coated its upper surface with gold leaf. When this was made to rest immoveably upon the oil, the dust retired in all directions so as to leave the space adjacent to the wafer quite free from every particle. But here it was observable, that this dispersion of the dust, by the seeming repulsion of the base of the lamp, was much more rapid at that side which lay nearest to the wick than at any other part, and least of all sensible at the side diametrically opposite.

The circumstances last mentioned seem sufficiently to account both for the progressive motion of the lamp, and for the general law of this motion formerly described. For, regarding this dispersion of the dust as yet only in a general way, and as the effect of some repulsion between the base and the oil contiguous to it, the facts above mentioned plainly indicate, that in all cases this repulsion is strongest at that part of the base nearest the wick or flame; and as action and reaction are equal and contrary, the lamp must therefore be impelled, in the direction of a line drawn through the wick, towards that part of the base most remote from it, and where the reaction is the least.

But in order to obtain a still more competent knowledge of the physical cause of these motions, it seemed now necessary to enquire more particularly into this apparent repulsion, between the base of the lamp and the surrounding oil, as indicated by the dispersion of the dust in the manner above described: and here the following considerations presented themselves:

The oil in the basin, when of an uniform temperature, has all its parts in a state of equilibrium and of rest. When the lamp is lighted, it is evident we have a very active cause introduced tending to destroy that equilibrium. This cause is the flame which broods over a small portion of the oil, and is separated from it only by the intervention of a piece of paper or a wafer. The oil in such circumstances, in consequence of being violently heated, must suddenly increase in volume, and must now, on account of the decrease of its specific gravity, be pressed upwards by a force sufficient to raise part of it above the general level. But this heated portion of oil, in its endeavour to rise up, will meet with a resistance equal to the weight of the incumbent lamp, which will determine it, in seeking a vent, to slide out from under the base in a thin superficial stream; and it seems to follow with equal certainty, that this constant stream will flow most rapidly and most copiously towards that side of the base of the lamp where the resistance is least, or where it has the shortest way to press forward; that is, from under the wick or flame to the edge of the base which is the nearest, according to what we have seen to be agreeable to the phenomena. But, from the laws of motion, it is certain that the re-action of this stream of rarefied oil, thus issuing most rapidly and most copiously from a particular side of the base, must impel the lamp in the contrary direction, and make it sail in the manner we have seen. It may further be remarked, that the heated oil so retreating from the flame, and endeavouring to rise somewhat above the general level, in consequence of its diminished specific gravity may more or less lift up that side

of the base nearest the wick, and aid the re-action of the recoiling stream, by making the lamp sail in the opposite direction, as it were down hill.

That the rarefied oil under the base has really a constant tendency to rise above the general level, seems undeniable from the following facts; namely, that after any of the lamps has burned a little while, and has got its base soaked with the oil, as soon as the flame is blown out the lamp sinks to the bottom; and even a lamp with its base made of a thin lamina of talc sails very well till the flame is extinguished, and then it immediately sinks.

Agreeable to the explanation which has now been attempted, I found that when a topical heat was applied to the surface of the oil, by bringing the point of a poker dully red hot nearly into contact, there was soon produced a superficial stream or efflux from the iron in all directions, which cleared the face of the oil from the charcoal dust in a wider and a wider circle, till at last the whole particles were crowded together at the confines of the basin.

When the oil in this experiment was shallow, having gold leaf beat into very minute parts mixed with it, an opposite stream was observed below setting in towards the poker in all directions, and then rising upwards. But this general tendency of all the parts of the fluid, of moving in quest of an equilibrium, is illustrated in a very entertaining manner as follows: Into a tea cup or punch glass nearly filled with pure water, pour a dessert spoonful of very clear salad oil with minute particles of gold leaf in it. If the water be cold, the oil when poured on at the centre, leisurely and continuedly, will rest upon the surface in the form of a lens, and remain insulated and equidistant from the sides of the vessel. A little lamp, when put upon this lens of oil and lighted, will sail and circulate as longer ones do in a basin. If it be now made to stand still, it is very amusing to observe the minute particles of the gold perpetually thrown out briskly at the stern in the superficial current, whilst the particles in the fund of the lens creep in all directions towards the lamp, and at last rise up under the base towards the flame, as the great centre of attraction, till they are caught by the retreating superficial stream, in which they rapidly trend off to some distance, when again they sink to renew the circulation.

When a patch of paper, or a wafer, or such light body, swims upon the oil in the basin, the point of a hot iron held near to it makes it quit its place, and move away by a seeming repulsion; but in reality by the heat generating a superficial stream flowing from the iron in all directions.

Again, if upon oil of turpentine, æther, alcohol, or any of the inflammable fluids possessing much tenuity, you throw a wafer much heated, it will immediately glide away and continue in motion till it cools; when the stream which issued from some part of it most copiously ceases. Double rum, melted tallow, bees-wax, and rosin, also afford the same continued efflux at the surface upon a topical application of heat, and the same phenomena as the oil does when little lamps are made to swim in them. It is somewhat remarkable, however, that though the inflammable fluids all agree in this, yet the topical application of heat at the surface of water does not produce similar effects.

For, if the point of a poker nearly red hot be held very close to the surface of water in a basin, the particles of the charcoal dust do not at all glide away, as they do in the case of oil, but seem to acquire only a slow irregular circular motion, which in time spreads wider, whilst

whilst the floating motes, or particles of dust, keep nearly their relative places; and the same thing happens, though the point of the iron touches the water so as to make it simmer.

I do not well know how to account for this, unless it may be a consequence of the known much less expansibility of water by heat, compared to that of the inflammable fluids, and which may be so inconsiderable as not to destroy the equilibrium, so far as to produce an efflux from the lighter and expanded fluid immediately under the heated body. Possibly too the parts of the water, as soon as heated, may transmit the surplus temperature to the contiguous colder water, much more rapidly than the inflammable fluids do in like circumstances, and thereby resist the high temperature necessary to that degree of expansion which would disturb the equilibrium and produce an efflux; not to mention that the maximum of this temperature can never, at any rate, exceed 212 degrees, the boiling point of water.

That the equilibrium, however, amongst the parts of water is disturbed by the local application of heat, though in a much smaller degree than what obtains among the inflammable fluids, appears from an experiment I was led to make with a small thin cup swimming on water, and so contrived as to carry and feed with oil a wick placed a little way down from the lip in the inside, so as to be on a level with the water. The consequence of this construction was, that the cup moved upon the water very slowly, but always with the flame evidently sternmost. The same cup, when taken from the water and put into a basin of strong rum, sailed a great deal faster, and according to the same usual law.

I am much afraid that, by this time, I have wearied you by such a detail of minute facts and circumstances, and by those frequent repetitions which every new subject more or less requires.—And I ever remain, dear Sir, your most obedient faithful servant,

PAT. WILSON.

*P. S.* Should you be inclined to repeat any of the experiments, the following directions and miscellaneous observations may be attended to: The thread I made use of for the wicks was of that soft kind commonly employed in the flowering of muslin. After making the puncture in the base, you put through a bit of the thread, which clip short off below, and with a pin force in the burr gently round the thread, to give the base a proper hold of it. Then clip away the superfluous thread above, leaving the wick about a quarter of an inch long; and so the lamp is completed. Set it then upon the oil by taking hold of the wick, that the paper base may not be bent or distorted by handling it; and, after the wick is touched with a drop of oil, it is ready for being lighted. For this purpose a bit of packthread, which has been steeped in oil, is a cleanly and convenient match, and sheds no impurities on the oil, as a candle or wax taper would do.

When you want the lamp to circulate, the oil must be very pure, and brought into full contact with the sides of the glass. The oil and the basin, or salver, should all be allowed to come to the same temperature, between 55 and 60 degrees of Fahrenheit. For, if any part of the brim be much hotter than the rest, the lamp, on arriving there, will leave the side, by the current issuing from the heated part forcing it away.

Sometimes the lamp, when sailing, veers a little into a different direction, by the base altering or warping by the scorching heat of the flame, which determines the stream to flow out most copiously at a different part of the base.

In the melted greafe which lies round the wick of a common candle, when lighted, there are sometimes observed atoms, which have been left by the snuffers, moving to and from the flame continually. These motions have been conceived by some as occasioned by attractions and repulsions, in consequence of an electrical quality imputed to the flame. It should seem, however, that they depend merely upon opposite currents, at the surface and immediately below the surface of the melted greafe, according to the principle above explained.

## VII.

*Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society of the Institute of Bologna, the Agrarian Society of Turin, &c.\**

THE apparatus made use of in most of the following experiments is the same as that of Poleni †. It is represented at Fig. 1. Pl. VIII. The reservoir X, of a conical form, has forty inches diameter at CE, and 30 at OP. FP is a broad plate of copper, the plane of which is perpendicular to the horizon; it is applied to the inside of the reservoir. The valve or flap FS, moveable by the handle K, is drawn up against the side of the vessel above F, in order that it may not impede the course of the particles of the fluid contained in the reservoir to the aperture P. I have applied different ajutages to this aperture, according to the exigence of the case. The tubes which I applied were made of tinned iron of the best quality; the longitudinal junction of the edges was made by immediate contact, and not by overlapping, and the whole of the workmanship was executed with great care. When the aperture was simply a hole through a thin plate, the thickness of its edge did not exceed one fourth of a line.

The upper vessel Z serves to maintain the water of the reservoir X at the constant height of the line CE, while it flows out through P. The plug AB is drawn more or less back, in order to regulate the introduction of the supply. The box or shelf DL prevents this water from exciting by its fall any agitation which might influence the emission at P. The opening at Q discharges the superfluous water which might rise above the line CE. The height of the surface CE above the centre of the orifice at P was 32,5 inches, in all cases where it is not otherwise expressed.

Most of the experiments here described were made in public at the Philosophical Theatre of Modena; various men of science were present at the rest; the different departments of experiment were performed by several persons at the same time. One of these operators repeated the seconds audibly from the clock; another drew back the valve SF; a third regulated by the means of the plug B the introduction of the supply of water, so that a very thin

\* Since the receipt of this memoir (which is equally valuable as the source of data for scientific processes, and of useful practical results, either unknown or disregarded in hydraulic works) it has remained in my hands with the intention of presenting my readers with an abstract of its contents. But, upon stricter consideration, I have thought it best to give it entire from the French original. N.

† De Castellis. This treatise is reprinted in the third volume of Hydraulic Treatises, published at Parma. V.



sheet of water constantly flowed at Q. At the instant agreed upon, the passages of the water were again closed. Every experiment was repeated successively for a number of times, until the agreement of the results had removed every suspicion of error. I am assured that even in the most complicated cases, the quantity of error could not exceed one fortieth part.

The measures indicated in the course of these experiments were taken from a toise adjusted by that of the Academy, which Citizen Lalande sent me in 1783. These measures, as well as all the others of the 18th century, will undergo the fate which is prepared for them by the establishment of the new metre. They may be reduced to this new standard, by observing that the foot is to the metre as 100 to 308.

The wisest philosophers have their doubts with regard to every abstract theory concerning the motion of fluids; and even the greatest geometers avow, that these methods, which have afforded them such surprising advances in the mechanics of solid bodies, do not afford any conclusions with regard to hydraulics but such as are too general and uncertain for the greater number of particular cases. Impressed with a conviction of this truth, I have attended to theory only when it combined with the facts, and was necessary to unite them under a single point of view. Even this small portion of theory may, if the reader pleases, be rejected; and he may consider the following propositions simply as the results of experiment.

When I quote the estimable work of Citizen Bossut, on hydrodynamics, I refer to the edition of 1786\*.

*Proposition 1.* The motion of a fluid is communicated to the lateral parts which are at rest.

Newton has affirmed, that when motion is propagated in a fluid, and has arrived beyond the aperture BC, fig. 2, the motion diverges from that opening, as from a centre, and is propagated in right lines towards the lateral parts NK, as well as towards S. The simple and immediate application of this theorem cannot be applied to a jet which issues from the aperture BC at the surface of still water. Circumstances enter into this case, which transform the result of the principle into particular motions. It is nevertheless true, that the jet BC communicates its motion to the lateral parts NK; but it does not repel them towards P and Q, but on the contrary transports them along with its own stream towards S.

*Experiment 1.* The horizontal cylindric pipe AC, fig. 3, is introduced into the vessel DEFB, which is filled with water as high as DB. Opposite and at a small interval from the aperture C commences a small rectangular channel of tinned iron, SMBR, which is open at top SR; the inclined bottom MB rests on the edge of the vessel B. It is 24 lines broad; the diameter of the tube AC is 14.5 lines; the extremity A is applied to the aperture P of fig. 1. The water of the reservoir being suffered to flow through the tube AC, the jet rises along the small channel MB, and flies out of the vessel in the stream BV. By this means a current is produced in the fluid of the vessel DEFB; this fluid enters into the channel SR, and issues by MBV along with the jet AC, so that in a few seconds the water DB falls to MH.

\* I consider this treatise as superior to all which before were extant. It is founded on a combination of the principles of experiment and of theory. I have profited by these principles, and several particular remarks which the same Citizen Bossut and Citizen Prony have been so good as to communicate after perusal of my memoir. V.

*Experiment*

*Experiment 2.* Bring some very light or moveable bodies near the jet of water P Y, fig. 1, which issues from the aperture P, and falls from a certain height E into the inferior vessel R T. It is seen that these bodies are carried along by the air which descends with the jet P Y. Part of this air is carried along and plunged into the water of the inferior vessel.

These experiments clearly prove, that the fluid which issues by B C, fig. 2, impresses its motion on the lateral parts N K; not by impelling them towards P Q, but by carrying them along with itself towards S. I call this the lateral communication of motion in fluids. Newton was acquainted with this communication, and has deduced from it the propagation of rotatory motion from the interior to the exterior strata of a whirlpool. Is this lateral communication of motion occasioned by the viscosity or mutual adhesion of the parts of the fluid, or their mutual engagement or intermixture, or the divergency of those parts which are in motion? We may perhaps be able to give some account of this when we shall have seen the effects; but in the mean time, whatever may be the cause, let us take the effect as experience points it out; let us consider it as a principle, and endeavour to apply it to some particular cases in order to ascertain the result.

The first circumstance to which I propose to apply this principle is the increase of expenditure of fluid issuing out of an orifice fitted with additional tubes.

*Proposition 2.* If that part of an additional cylindric tube which is nearest the side of the reservoir be contracted according to the form of the contracted vein of fluid which issues through a hole of the same diameter in a thin plate, the expenditure will be the same as if the tube were not contracted at all. It is well known, that when the water of a reservoir is suffered to flow through a circular orifice in a thin plate, the fluid vein which forms the jet becomes contracted at a short distance from the orifice; and the diameter of the contracted vein is nearly 0,8 of the diameter of the orifice. Poleni first observed, that by applying an additional cylindric pipe to the orifice, of the same diameter as the orifice itself, and from two to four times that length, the expenditure is increased from 100 to 133. To account for this augmentation, he supposes that the fluid vein is less contracted in pipes than after passing through the thin plate. The supposition was not unreasonable; but it could not apply to the case announced in this proposition. I shall proceed to give the particulars in the following experiment.

*Experiment 3.* To the aperture P, of fig. 1, I applied a circular orifice 18 lines in diameter, pierced through a thin plate. Four cubical feet of water flowed into the vessel Y in 41 seconds.

I then applied to the orifice a cylindric tube of the same diameter, and fifty-four lines long. The four cubic feet flowed out in 31 seconds.

Instead of this simple cylindric tube, I applied the compound tube of fig. 5; the parts of which have the following dimensions in lines:  $AC = GI = MN = 18$ ;  $DF = 14,5$ ;  $AB = 11$ ;  $BG = 10$ ;  $GM = 37$ ;  $AM = 58$ . With this compound tube the expenditure of four cubic feet of water was made in 31 seconds, as with the simple cylindric tube.

The form of the conical portion A C D F was nearly the same as that of the contraction of the vein which issues through a thin plate. The vein must therefore have passed through a contraction nearly equal to that of the contracted vein from a thin plate; the expenditure  
never-

nevertheless was more abundant, in the same proportion as through the simple cylindric tube. It follows, therefore, that the velocity of the section D F, and of the whole conoid A C D F, must have been greater than that of the contracted vein from a thin plate; and it remains to be shewn what was the cause of this augmentation of velocity which takes place within the tube, and does not manifest itself externally.

That the conical tube A C D F does not itself cause any augmentation of expenditure, is evinced by the following :

*Experiment 4.* The conical tube A C D F, from which the remaining part D G M N I F was separated, was applied to the orifice P. The four cubic feet were emitted in 42 seconds, which is the time of the expence through the orifice itself A C in the thin plate, with the difference of one second only. This slight variation arises from its being almost impossible to make the tube A D C F perfectly of the form of the natural contracted vein.

*Proposition 3.* The pressure of the atmosphere increases the expence of water through a simple cylindric tube, when compared with that which issues through a hole in a thin plate, whatever may be the direction of the tube.

It has long been known, that a heavy fluid which moves in a descending cylindric pipe tends to accelerate its motion. The inferior parts tend to separate themselves from the superior, and by that means cause the pressure of the atmosphere to increase the velocity of the superior parts. This successive acceleration of gravity cannot take place in an horizontal or ascending pipe. We shall nevertheless find that the pressure of the atmosphere acts even in these last situations to increase the velocity of fluid within the pipe. Certain questions of legal right, which arose in my country, respecting the quantity of water supplied by a pipe for watering lands (canal d'arrosement) directed my attention to this object.

In the year 1791 I made the following experiments publicly in the Theatre of Natural Philosophy at Modena :

*Experiment 5.* To the aperture P, fig. 1, I applied a cylindrical pipe 54 lines in length and 18 in diameter. At the distance of nine lines from the interior orifice P, twelve small holes were made in its circumference. When these small holes were open, the four cubic feet issued out in 41 seconds, in the same manner as through a thin plate. Not a single drop passed through any of the holes, and the stream did not fill the tube. The holes were then closed one after the other with wet skin. As long as there was one hole open the expence continued the same; but when at last all the twelve holes were well closed, the fluid stream issued out in a body which filled the pipe, and the four cubic feet were emitted in 31 seconds.

*Experiment 6.* To the cylindric tube K L B, fig. 6, 18 lines in diameter and 57 lines long, was joined the glass tube Q R S T, at the distance of eight lines from the interior orifice K. The glass tube was plunged in coloured water contained in the vessel T. When this apparatus was applied to the aperture P, fig. 1, the four cubic feet of water flowed out in 31 seconds. The coloured liquid T rose in the tube T R as high as S, at the height of 24 inches above the surface T.

The branch R T of the glass tube was shortened so that R T was only six inches longer than R Q. The efflux being then permitted to take place, the coloured liquor of the vessel T rose through the tube R T, and mixed with the water which flowed from the reservoir through K V, both of which flowed out at V, and in a short time the vessel T was emptied.

I repeated

I repeated this experiment with the compound tube fig. 5, and the results were the same.

*Experiment 7.* The cylindrical pipe K L V, fig. 6, was applied in an ascending and nearly vertical situation to the orifice R, fig. 8, of the vessel H I, of which the end H communicated by an opening of considerable extent with the water of the reservoir X, fig. 1. The charge on the upper extremity V of the tube was 27,5 inches. I inclined the tube a little from the vertical direction, in order that the jet might not fall back upon itself. The glass tube Q R T, fig. 6, in this new situation was so disposed that its lower extremity was immersed as before in the coloured liquid of the vessel T. When the efflux was permitted, the expenditure of four cubic feet was made in 34 seconds; and the coloured liquid rose in the tube R T to the height of near 20 inches. With the same charge of 27,5 inches the orifice of 18 lines in a thin plate would have afforded the four cubic feet in 45 seconds.

*Experiment 8.* A cylindrical vessel of 4,5 inches diameter had in its vertical sides near the base a circular opening of 4,5 lines in diameter, opened in a thin plate of tinned iron. The surface of the water contained in this vessel was 8,3 inches above the centre of the aperture. The water was then suffered to flow out of this aperture in the thin plate, and its surface was depressed seven inches in the vessel in 27,5 seconds of time.

To the same aperture was applied a cylindric tube of the same diameter, and in length 11 lines. The vessel was filled to the same height as before, and, the water being suffered to flow out, its surface was depressed seven inches in 21 seconds.

The same experiment was afterwards repeated in the receiver of the air-pump, under which the mercurial gauge stood at no more than 10 lines in height. The surface of the water in the vessel was depressed seven inches in 27,5 seconds, whether the aperture was made in a thin plate, or whether it was provided with an additional cylindric tube.

The height of the coloured water in the tube of glass measures the active quantity of the pressure of the atmosphere, which is exerted on the surface of the water to increase the expenditure. For example, in the sixth experiment  $32,5 + 24$  inches charge on the orifice P, and we have nearly  $\sqrt{32,5} : \sqrt{56,5} :: 31'' : 41''$ , as is required by the common theory of the motion of fluids which issue out of vessels by a small aperture. The same obtains in Experiment 7.

Daniel Bernoulli made the 7th experiment in descending tubes, and in diverging conical tubes, and explained the result merely by the theory of conservation of living forces. Euler and d'Alembert observed to him, that the pressure of the atmosphere was concerned in the effect\*. Though the case of the descending tube be different from that of the horizontal or ascending tube, the knowledge of the first of these two cases may nevertheless facilitate the knowledge of the second. Besides which, the causes which act in both cases are often combined together, and it is necessary to be well acquainted with both, in order to distinguish the results. On this account it is, that in the following proposition I have turned from my principal subject for a moment to consider the first case, after which I shall return to the second.

*Proposition 4.* In descending cylindrical tubes, the upper ends of which possess the form of the contracted vein, the expence is such as corresponds with the height of the fluid above the inferior extremity of the tube.

\* D'Alembert, *Traité des Fluides*, Art. 149.

The ancients remarked, that a descending tube applied to a reservoir increases the expenditure\*. Mariotte estimated that the water issues through C Q, fig. 7, with a velocity nearly the mean proportional between the velocities arising from the two heights A B, A C†. Guilielmini sought for the cause of this augmentation in the weight of the atmosphere, and determined the velocity at C to be the same as would arise from the whole height A C‡. In his reasoning he supposes that the pressure at C is the same for the state of motion as for that of rest; which is not true. In the experiments he made upon this object, he paid no regard either to the diminution of expenditure produced by the irregularity of the inner surface of the tubes, nor the augmentation occasioned by the form of the tubes themselves. By a singular accidental concurrence, one of these errors compensated for the other. I know of no other decisive experiment on this head since Guilielmini. I shall, therefore, proceed to establish the proposition upon the principle of virtual ascension combined with the pressure of the atmosphere, and that in a manner which shall be clear of every objection, of theory as well as of experiment.

Let B L K O represent a conical tube adapted to the form of the contracted vein §; the cylindrical tube L C Q K is of the same diameter as the contracted part. The fluid stratum, L K, continuing to descend through L C, tends to accelerate its motion, according to the laws of gravitation; and consequently when it passes from L K to M N, it tends to detach itself from the stratum which follows, or in other words it tends to produce a vacuum between L K and M N; and the same effect takes place through the whole length of the tube L C. The pressure of the atmosphere becomes active as far as is necessary to prevent the vacuum; and its action is alike both at the surface of the fluid at A, and at the inferior extremity of the tube at C. At A it increases the expenditure, and at C it destroys the sum of the accelerations which would be produced along L C, so that the fluid remains continuous in the tube.

Let T represent the time which the continuous column of fluid L C Q K employs to pass through the tube L C, whatever may be the velocity at L, and the successive acceleration from L to C. And if we suppose this same column to return upwards from D to E, it will pass through the space D E = L C in the same time T; during which it will lose all the acceleration it acquired from L to C. The pressure of the column E D, continued for the time T, is therefore the quantity required to destroy the successive acceleration from L to C, and to prevent the fluid from ceasing to be continuous in the tube L C: consequently that part of the pressure of the atmosphere which is exerted at C Q to destroy the sum of the accelerations through L C, is equal to the pressure of a column E D of a fluid, homogeneous to that of the reservoir A B. And since the same pressure must also be exerted on the surface A of the reservoir, if we take F A = L C, the fluid at L K will possess the velocity which is proper to the height F L = A C; without considering the retardation which the external inequalities of the tube L C Q K must produce.

\* Calix devexus amplius rapit. Frontin. de aquæduct. Art. 36. See also the Pneumatics of Hero, in the mathem. vet. ed. 1693, page 157.

† Mouvement des eaux, part. 3, disc. 2.

‡ Epist. hydrostatic. Oper. tom. 1. page 212.

§ When I speak of the form of the contracted vein, I always mean to express the conoid formed by the fluid issuing from an orifice through a thin plate.

*Experiment IX.* 1. The orifice P (fig. 1) through a thin plate is circular, and 18 lines in diameter. The charge of fluid above the centre of the orifice is 40 inches. Four cubic feet of water were emitted in 38 seconds.

2. To the orifice P, fig. 1, I applied the tube A C D, fig. 4, the upper end of which A C had the form of the contracted vein. The diameter at A was 18 lines in length, A D 31 inches, and the situation of the tube horizontal. The expenditure of four cubical feet was made in 48 seconds.

3. The same orifice and the same tube were applied to the horizontal bottom of the reservoir fig. 7, so that the tube was vertical, and A C = 40 inches, or the height of the charge in the two former experiments. The four cubic feet flowed out in 48 seconds, as in the second experiment.

*Experiment X.* The last described experiment was repeated with a circular aperture of 11,2 lines in diameter. The extremity A C, of the tube fig. 4, had the form of the contracted vein; the end A having the same diameter as that of the orifice. The other circumstances were as in the preceding cases. In the disposition, according to the first case four cubical feet of water flowed out in 98 seconds; in the second case the time was 130 seconds; and in the third case 129 seconds.

In each of these two experiments the tubes and the expence of water were the same for the second and the third cases; whence it follows, that the force by which the expenditure was governed was the same in both cases. Now the force which acts in the second case is the same as in the first; and consequently the same force likewise acts in the first and third cases. All the difference of the result between the first case and the two following arises from the retardation produced by the inequalities of the internal surface of the tubes.

*Experiment XI.* The height A B, fig. 7, being constantly 32,5 inches, and the orifice B O 18 lines in diameter, the tube B O C Q was applied to the orifice itself, the superior extremity of this tube having the form of the contracted vein. When the length of the tube was varied, the times of the efflux of four cubic feet of water were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet by experiment.	Time according to the theory with- out considering the retardation.	Difference between the theory and ex- periment.	Retardations com- puted from the following experi- ment.
3	41"	40"	1"	1",3
12	38"	35",2	2",8	3",4
24	35"	31",2	3",8	5"

The fifth column of this table is calculated from the proportion of retardation produced by the irregularities of the internal surface of the tubes. Citizen Boffut has observed, that these retardations\* increase rather in a less ratio than the velocity of the stream. This is perhaps the reason of the difference observed between the fourth and fifth columns.

*Experiment XII.* I applied to the orifice P, fig. 1, the same tubes as in the foregoing experiment one after another in an horizontal situation, the height of the charge being constant-

\* Hydrodyn. Art. 622.

ly 32,5 inches above the centre of the orifice. The times of emission were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet.	Differences.
0	41"	0
3	42",5	1",5
12	45",5	4",5
24	48"	7"

I must here observe, that the viscosity or mutual adhesion of the particles of the water \* is of very little consequence to the increase of expenditure through the orifice B O, fig. 7, by the additional tube B C. For as soon as a small hole is opened at K the increase of expenditure diminishes or entirely ceases, and the fluid is no longer continuous in the tube.

We will now return to tubes in the horizontal and ascending situations.

[ *To be continued.* ]

## VIII.

*Experiments and Observations on the Effect of Annealing a Plate of Metal, consisting of fine or alloyed Gold, with one twenty-fourth Part of Tin.* By MATTHEW TILLET.

[ Concluded from page 142. ]

I HESITATED to use my gold of 24 carats, from the just fear I had that it would lose the advantage of being the ductile of metals, and that I could not restore this property but by the method of parting. For this reason I determined to begin my experiments on a more decisive scale than the former, upon gold of 22 carats, or alloyed with  $\frac{1}{24}$ th part of copper. In this manner I should repeat some of the experiments related by Mr. Alchorne, from which he has constantly inferred that tin, mixed in a certain proportion with gold of this same fineness of 22 carats, does not deprive it of its ductility.

I therefore alloyed 1 gros 24 grains of tin taken from the ingot deprived of arsenic, which I have mentioned, with 4 ounces of gold, the fineness of which, namely 22 carats, was perfectly well ascertained. These two metals, reduced into small pieces, were mixed together, put into a crucible, and urged by the strong heat of a forge with two pair of bellows. When their fusion appeared to be complete, I poured the metal into a small ingot-mould proportioned to the quantity.

The ingot thus obtained had lost scarcely any thing of the weight of the two metals that composed it, which was a proof that the tin had united and incorporated with the four ounces of gold. But on attempting to bend the ingot, which was about six inches long and not more than two or three lines thick, I remarked, contrary to the nature of gold of 22 carats, that it was rigid, and would have required a considerable effort to give it any degree

\* Gravesande and others have attributed the increase of expenditure through descending tubes, to the natural cohesion of the particles of water. V.

of curvature, or bring it to the flexibility it would have possessed if no tin had entered into its composition. Hence I clearly saw, that this rigidity announced a diminution of its ductility; that the interposition of a substance, which was foreign to the gold, and incapable by its nature of maintaining the cohesion, was the cause of this want of flexibility; and that it could be attributed only to the tin, because copper alone, mixed with fine gold, though it gives a greater degree of hardness and rigidity than it before possessed, deprives it of very little of its ductility.

After this first observation on the state of the ingot afforded by the experiment, I came to the more decisive proof by hammering, particularly with the edge of the hammer, in order that the bar might be lengthened, and by that means submitted to the most decisive proof. I did not observe during the continuation of this process, till the bar was reduced to about two-thirds of its first thickness—I did not observe, I say, that its edges were cracked, or exhibited much of the appearance of brittleness; but as I was apprehensive that this accident might happen by too long hammering, I divided the bar by cutting off the part which had been hammered out. This part was placed in the midst of lighted charcoal, in order that, by a moderate annealing, it might recover the state of malleability it possessed before it was hammered.

But when I went to take it out of the fire, where it had undergone no greater heat than a cherry-red, I found it divided into two parts. After having suffered these to cool, I forged them again. They were extended with considerable ease, though with some cracks at the edges; but they did not yet satisfy the whole of my enquiries. I therefore annealed one of the two last-mentioned pieces a second time, and reserved the other in its hard hammered state to be passed between the laminating rollers. The annealed part, which might have the thickness of about a shilling (piece de douze sols), broke in the fire, though the heat was very gentle, into four or five portions. The longest of these portions, which best resisted the action of the fire, bent and twisted itself, and shewed, by this state of strong contraction in different directions, that it had tended to break and become divided into small portions similar to those which had already separated from it.

This accident gave me reason to suspect that the ashes, upon which I had annealed the plate of gold which broke in pieces, might also contain certain portions. I was not deceived; for the ashes being carefully washed left three or four fragments which the ignited coals had prevented from being seen.

It has been remarked, that I reserved one of the two parts of the portion of the ingot which I had forged a second time, and that I had kept it in its hammer-hardened state. I could not doubt, after the last-recited experiment, that any attempt to anneal this portion, even by the most moderate heat, would be attended with the same consequence, namely, that it would break in pieces. I therefore determined to extend it still more between the rollers, setting them up very gradually in order that the fracture, if it should take place, might be principally owing to the brittleness of the material, and not to the force of compression to which it was subjected. By this management I succeeded in extending the metal to double its length notwithstanding its hardness. In this manner it was rendered as thin as strong paper; though it must be confessed that the edges were cracked through their whole length like the teeth of a saw. But this accident is not at all surprising, when it is considered that gold, though alloyed simply with copper, whatever may be the cause, does



does not possess its usual ductility, particularly when it is laminated very thin without repeated annealing as the metal becomes hard.

It might be presumed, in reflecting on the experiment I have related, that the fracture of the pieces of gold was owing to an incomplete fusion or unequal mixture of the two metals. I was aware, from this notion, that it was proper to melt the ingot over again with all the parts which had been separated from it, and to neglect no precaution, after well mixing it, that it should be poured out in perfect fusion. At the moment this state of fusion was obtained, I threw a small quantity of calcined borax upon the metal, in order that its surface might become clear, and every foreign substance might be carried to the circumference. No other consequence could arise from this use of borax than a greater softness in the compound, and consequently less risk of its breaking when it came to be subjected to the hammer.

All my precautions were useless. I forged one end, which was lengthened very well without any perceptible crack; but the extremity of this ingot, afterwards reduced to a small thickness and exposed to the annealing heat, became divided into several parts, the longest of which was contorted, and would no doubt have broken if the heat had been stronger, or continued for a greater length of time.

Though the experiment, of which I have related the result, on the mixture of one part of tin and 24 parts of gold of 22 carats, might, in strictness, have been sufficient to prove that this alloy deprives gold of great part of its ductility, and exposes it, while annealing, to an accident which artists would scarcely be able to avoid, from the necessity they are under of continually restoring the malleability of the gold they work, by annealing it after it has become hard under the hammer; I nevertheless thought it proper to lay aside the objection I had to deprive my fine gold of its ductility, which could not be restored but by a new parting essay, and to repeat the experiment by using gold of 24 carats in such a quantity that my operation might be compared with that of Mr. Alchorne, from which chiefly he has drawn his conclusion.

I therefore employed 6 ounces of fine gold and 2 gros of tin for this new experiment. The first of these metals was divided into a great number of pieces, and I had included the two gros of tin in two leaves of gold taken from the 6 ounces, rolled out very thin, and so flexible, after annealing, that they perfectly enveloped the tin. After having put one part of the gold at the bottom of a small crucible, I placed the tin wrapped up in gold upon it, and over this I put the remainder of the gold.

When the whole of the two metals was in perfect fusion, I poured them speedily into an ingot mould, which I had before used, and obtained an ingot rather longer and cleaner than the two former.

As soon as it was cold, I forged one of its extremities with the edge of the hammer. It was lengthened without any perceptible crack; and when it was reduced to the thickness of one line, or thereabouts, I cut it off for separate treatment. By moderate annealing it maintained its integrity; and, with the exception of a few cracks, it passed the laminating rollers without breaking. As I was fearful, nevertheless, that it might break in some part if I continued to laminate it, I gave it a slight annealing. It had scarcely acquired a cherry redness between the charcoal before it broke into five or six parts, some of which were simply bended or twisted, and others flat as they quitted the rollers. Among the annealed

pieces of this extremity of the ingot, there was one sufficiently long, though a little curled, which I laminated a second time, with the determination of rendering it very thin without the least annealing. It acquired at least double the length it had at first without breaking; and, if we except the two sides of this plate which were cracked, the body, or main piece, was entire. It was spongy, and might be considered as if formed out of an ingot of common gold containing no tin, but not possessing the whole of its natural ductility.

It follows, from these experiments, that gold, whether fine or alloyed, when perfectly fused with a small portion of the finest tin, acquires rigidity and hardness by the mixture; that it loses somewhat of its distinguishing colour; and that it may, indeed, by careful management, be extended to a certain degree by the hammer, or still better by the rollers; but that, as it cannot be annealed without danger of breaking, it is by this defect deprived of the essential advantage of recovering its original softness after it has been strongly hammer hardened. It is not but by careful management in the use of the hammer, and by frequent annealing, that artists employed on works of gold and silver succeed in obtaining them without cracks, and bringing them to a state of perfection, without being obliged to have recourse to solder to repair the defects which excessive hardness under the hammer would occasion. How much, therefore, ought gold-workers, who continually have this metal in their hands, to be attentive to prevent the introduction of tin in their workshops, and never to employ such compounds of gold as are subject to break, or even to warp, while annealing! The expence of refining, which they would pay for depurating such compounds, would be of less consequence to them than the loss of time required for the careful management of such gold contaminated by tin, even if they did succeed in using it, and were not often forced to abandon, after much labour, a work nearly finished.

I do not doubt but that Mr. Alchorne, if he had carried his experiments further, or had considered them with regard to the methods of gold-workers, who frequently expose to the fire such pieces as they propose to raise or fashion according to their designs;—I do not doubt, I say, but that Mr. Alchorne would have cautioned artists against the accidents to which gold alloyed with tin is subject while annealing. He has observed a degree of ductility in this matter, and has not presumed that it might be taken away by means of fire, which, on the contrary, restores to most metals their flexibility and facility of working.

The opinion which has hitherto been maintained respecting the danger of a very small alloy of tin with gold is therefore well founded. In fact, it must have been difficult for such an opinion to have prevailed without foundation, when a multitude of artists are in a situation to verify the fact, and must be immediately struck with the brittleness of such gold, and be most strongly interested to discover its cause.

This example of an opinion generally received and supported on constant facts, which the greater number of artists have rather adopted than examined; this example proves that we ought not, without the greatest caution, to attack such received notions, particularly when as in the present case it is in no respect hurtful, and only tends to render the workmen more cautious in the use of the most valuable of metals.

The experiments of Mr. Alchorne have long remained upon record in the Philosophical Transactions, and have by that means acquired a sanction which demands a greater degree of attention. I have had no other aim in repeating them, and exhibiting an essential dependant fact not mentioned by that skilful assayer, than to give useful information to artists, and

and contribute to the certainty of their operations. I think this purpose will be answered, by leaving them in possession of all the apprehension they have hitherto entertained respecting the mixture of tin with gold.

If it should be thought that my experiments may not be as conclusive as at first sight they appear, they will at least produce no other effect than that of giving artists some useless trouble. They will with reason attend to them, however superfluous they may be supposed, and will prefer the care I recommend to the anxiety of working upon materials rendered suspicious by their harshness, and supposed to be incapable of annealing on account of their containing tin.

If it be allowable to form some conjectures on the fracture of plates of gold containing tin, when subjected to the annealing heat, it may be presumed, that the tin, which very speedily melts, while the gold requires a very strong heat for its fusion—it may be presumed, I say, that the parts of the tin intermixed in a sort of proportional equality with those of the gold, tend to separate by a speedy fusion and at a very gentle degree of heat; that they remain without consistence between the parts of the gold, while the latter preserve the whole of their solidity, and do not lose it even by the annealing heat; whence it seems, that the parts of the precious metal, when ignited among the coals, having no longer the solid connection formed by the tin, but, on the contrary, having an infinite number of small cavities occupied by particles of that metal in fusion, must tend to disunion; whereas the same accident does not take place in the pieces which have resisted the annealing, and have been laminated after cooling, because the particles of tin have become solid by cooling, and have recovered their original state of union with the gold.

This fracture of the compound does not take place with an alloy of gold and copper, for an opposite reason to that which has here been explained; namely, because these two metals require nearly the same heat for their fusion. The effect of annealing being therefore equal upon both, the metals, notwithstanding this treatment, preserve their natural consistence, even though the heat be carried near the point of fusion.

In support of the opinion I have presented, respecting the fracture of these plates of gold, I must observe that, by examining their surfaces under the microscope, a great number of particles of tin may be distinguished, which appear to be disengaged from the pores of the gold; that these surfaces, being applied by an annealing heat to a plate of iron or silver, adhere strongly by virtue of these particles of tin; that they cannot be separated but with difficulty, and even tear up some slight portions of the metal on which they were annealed when they are so separated.

I shall conclude these details, into which the subject of my experiments has forced me to enter, by remarking that the plates of gold here spoken of may be kept entire, when annealed, on a flat plate of metal, on which they may remain supported till cold; whereas, if placed on ignited charcoal, they frequently bend or twist, and are subject to break, particularly if an attempt is made to take them out with the tongs in their ignited state.

## IX.

*On the Knowledge of the Ancients respecting Gravity. By a Correspondent.*

THE respectable publishers of the "Journal of Natural Philosophy, Chemistry and the Arts," are desired to submit to the inspection of its ingenious Author the following observations relative to a Note at page 85 of the 15th Number. (Vol. II.)

"In the treatise of 'Plutarch De Placitis Philosophorum,' occurs no passage referring to the 'vibration of a body through the centre of the earth to the antipodes, and to the retention of the moon in its orbit by the combination of the projectile and gravitating forces.'"

The passage alluded to is probab'y in the treatise "De Facie quæ in Orbe Lunæ apparet;" but there, so far from proving a gravitating force, the absurdity of the supposition is endeavoured to be shewn.

Ed. Oxon.—8vo. T. iv. § ζ. Φιλοσοφων δε γκ ἀκνεγον, αν τα παραδοξα παραδοξοις αμυνεσθαι βελωνται, και μαχομενοι προς τα θαυμασια των δογματων, αποπτωτερα και θαυμασιωτερα πλαττωσιν' ωσπερ ετοι την επι το μεσον φοραν εισαγουσιν. 'Η, τι παραδοξον γκ ενεγειν; Ουχι την γην σφαιραν ειναι, τηλικαυτα βαθη και υψη και ανωμαλιας εχουσιν; Ουκ αντιποδας οικειν, ωσπερ θριπτας, η γαλεωτας, τραπεντας ανω τα κατω τη γη προϊσχομενες; ημας δε αυτες μη προς ορθας βεσηκοις, αλλα πλαγιους επιμενειν απονευσοντας, ωσπερ οι μεθυοντες; Ου μυδρες χιλιδαλαιας δια βαθους της της φερομενες, οταν εξικωσιν προς το μεσον, ισασθαι μηδενος απαντωνος, μηδε υπερειδοντος; Ει δε ρυμη κατω φερομενοι το μεσον υπερβαλλοιεν, αυτες οπισω γρεφεσθαι και ανακαμπειν απ' αυτων. Thus translated by Philemon Holland—"But we ought not to give ear unto philosophers, if they would maintain strange "paradoxes by other positions as absurd, or, to confute admirable opinions, devise others "much more extravagant and wonderful, like as these here who broach and bring in a "notion, forsooth, tending unto a middle, wherein, what absurdity is there not? Hold "not they that the earth is as round as a ball, and yet we see how many deep profundities, "haughty sublimities and manifold inequalities it hath? Affirm not they that there be antipodes dwelling opposite one unto another, and those sticking as it were to the sides of the "earth with their heels upward and their head downwards topsy turvy, like unto these wood-worms or cats (r.newts) which hang by their sharp claws? Would not they have even us also "that are here, for to go upon the ground not plumb upright, but bending or inclining sidelong, reeling and staggering like drunken folk? Do they not tell us tales, and would make "us believe, that if bars and masses of iron weighing a thousand talents a piece were let fall "down into the bottom of the earth, when they came once to the middle centre thereof, "will stay and rest there, albeit nothing else came against them nor sustained them up? And "if peradventure by some forcible violence they should pass beyond the said midst, they "would soon rebound back thither again of their own accord?"

So true is the observation of Cleomedes, that those among the ancients who treated of natural philosophy were much confused, and on this head greatly erred, not being able to discern, that since the world was of a spherical figure, the centre must of necessity be inferior to every part of it—αι δε λοιπαι σχεσε πολλην παρεσχον ταραχην τοις παλαιωτεροις των φυσικων—ου δυναθεντων επισησαι, οτι εν τω κοσμω, σφαιρικω τω σχηματι οντι, κατω μεν απο παντος αυτε, το μεσαιταλον ειναι αναγκαιον. De Meteoris, ed. Balforei, 1605, p. 9.

*Remarks on the preceding Communication.*

UPON consulting the original, to which I referred by memory, in the note at page 85 of the present volume, I perceive that both the passages alluded to are in the treatise *De Facie in Orbe Lunæ*. I was misled by another obscure passage in the third book *De Placitis Philosophorum*, under the title *Περὶ κινήσεως γῆς*. 17. where it is said that Philolaus the Pythagorean held that the earth revolves in an orbit (*περὶ τὸ πῦρ*) around the fire, and that Heraclides of Pontus and Ecphantus the Pythagorean attributed motion to the earth, not progressive, but rotatory, from west to east. I did not, as my learned correspondent seems to intimate, pretend to say that either Plutarch or the speakers in his treatise exhibited the doctrines of the gravitating and projectile forces, as being worthy of adoption. My implied argument was, that since they are extant in a passage of some length, there were in fact philosophers among the ancients who had maintained and developed them; though, from a variety of well-known causes, this doctrine remained without distinction among other ill-digested or false systems. The combination of these forces in the moon, which occurs in the paragraph to which Theo replies, in my correspondent's quotation, is *καίτοι τῇ μὲν σελήνῃ βονθεία πρὸς τὸ μὴ πέσειν, ἢ κινήσει αὐτὴ καὶ τὸ ριζῶδες τῆς περιαγωγῆς*. Ὡς περ, &c. "But the moon is prevented from falling by its motion, and the violence of its revolution; as bodies placed in a sling and whirled round do not drop out. For every body will be carried according to its natural motion, if not diverted by some other cause. The moon is not, therefore, carried in the direction of (or by) its weight, because its circular motion opposes this tendency."

## X.

*A short Mineralogical Description of the Mountain of Gibraltar. By Major IMRIE\*.*

THE mountain of Gibraltar is situated in  $36^{\circ} 9'$  north latitude, and in  $5^{\circ} 17'$  east longitude from Greenwich. It is the promontory which, with that of Ceuta upon the opposite coast of Barbary, forms the entrance of the straits of Gibraltar from the Mediterranean; and Europa Point, which is the part of the mountain that advances most towards Africa, is generally regarded as the most southern promontory in Europe. The form of this mountain is oblong; its summit a sharp craggy ridge; its direction is nearly from north to south; and its greatest length in that direction falls very little short of three miles. Its breadth varies with the indentations of the shore, but it no where exceeds three quarters of a mile. The line of its ridge is undulated, and the two extremes are somewhat higher than its centre.

The summit of the Sugar-loaf, which is the point of its greatest elevation towards the south, is 1439 feet; the Rock Mortar, which is the highest point to the north, is 1350; and the Signal-house, which is nearly the central point between these two, is 1276 feet above the level of the sea. The western side of the mountain is a series of rugged slopes, interspersed with abrupt precipices. Its northern extremity is perfectly perpendicular, except towards the north-west, where what are called the Lines intervene, and a narrow passage of flat

\* Transactions of the Royal Society of Edinburgh, iv. 191.

ground that leads to the isthmus, and is entirely covered with fortification. The eastern side of the mountain mostly consists of a range of precipices; but a bank of sand rising from the Mediterranean in a rapid acclivity covers a third of its perpendicular height. Its southern extremity falls in a rapid slope from the summit of the Sugar-loaf into a rocky flat of considerable extent called Windmill-hill. This flat forms half an oval, and is bounded by a range of precipices, at the southern base of which a second rocky flat takes place similar in form and extent to Windmill-hill; and also like it surrounded by a precipice, the southern extremity of which is washed by the sea, and forms Europa Point. Upon the western side this peninsular mountain is bounded by the bay of Gibraltar, which is in length nearly eight miles and a half, and in breadth upwards of five miles. In this bay the tide frequently rises four feet. Upon the north the mountain is attached to Spain by a low sandy isthmus, the greatest elevation of which above the level of the sea does not exceed ten feet, and its breadth at the base of the rock is not more than three quarters of a mile. This isthmus separates the Mediterranean on the east from the bay of Gibraltar on the west.

This mountain is much more curious in its botanical than in its mineralogical productions. In respect to the first, it connects in some degree the Flora of Africa with that of Europe. In respect to the latter, it produces little variety; perhaps a few substances and phenomena that are rare, but none that are peculiar.

The principal mass of the mountain rock consists of a grey dense (what is generally called primary) marble; the different beds of which are to be examined in a face of 1350 feet of perpendicular height, which it presents to Spain in a conical form. These beds or strata are of various thickness, from 20 to upwards of 40 feet, dipping in a direction from east to west nearly at an angle of 35 degrees. In some parts of the solid mass of this rock I have found testaceous bodies entirely transmuted into the constituent matter of the rock, and their interior hollows filled up with calcareous spar; but these do not occur often in its composition, and its beds are not separated by any intermediate strata.

In all parts of the globe where this species of rock constitutes large districts, it is found to be cavernous. The caves of Gibraltar are many, and some of them of great extent. That which most deserves attention and examination is called St. Michael's Cave, which is situated upon the southern part of the mountain, almost equally distant from the Signal-tower and the Sugar-loaf. Its entrance is 1000 feet above the level of the sea: this entrance is formed by a rapid slope of earth which has fallen into it at various periods, and which leads to a spacious hall incrustated with spar, and apparently supported in the centre by a large massy stalactitical pillar. To this succeeds a long series of caves of difficult access. The passages from the one to the other of these are over precipices, which can only be passed by the assistance of ropes and scaling-ladders. I have myself passed over many of these to the depth of 300 feet from the upper cave; but at that depth the smoke of our torches became so disagreeable that we were obliged to give up our pursuit, and leave caves still under us unexamined. In these cavernous recesses, the formation and process of stalactites is to be traced from the flimsy quilt-like cone, suspended from the roof, to the robust trunk of a pillar, three feet in diameter, which rises from the floor, and seems intended by nature to support the roof from which it originated.

The variety of form which this matter takes in its different situations and directions renders this subterraneous scenery strikingly grotesque, and in some places beautifully picturesque.

The

The stalactites of these caves when near the surface of the mountain are of a brownish yellow colour; but as we descended towards the lower caves we found them begin to lose their darkness of colour, which by degrees shaded off to a whitish yellow.

The only inhabitants of these caves are bats, some of which are of a large size. The soil in general upon the mountain of Gibraltar is but thinly sown; and in many parts that thin covering has been washed off by the heavy autumnal rains, which have left the superficies of the rock for a considerable extent bare and open to inspection. In those situations an observing eye may trace the effects of the slow but constant decomposition of the rock, caused by its exposure to the air, and the corrosion of sea salts, which in the heavy gales of easterly winds are deposited with the spray on every part of the mountain. Those uncovered parts of the mountain rock also expose to the eye a phenomenon worthy of some attention, as it tends clearly to demonstrate, that, however high the surface of this rock may now be elevated above the level of the sea, it has once been the bed of agitated waters. This phenomenon is to be observed in many parts of the rock, and is constantly found in the beds of torrents. It consists of pot-like holes of various sizes hollowed out of the solid rock, and formed apparently by the attrition of gravel or pebbles set in motion by the rapidity of rivers or currents in the sea. One of those, which had been recently laid open, I examined with attention. I found it to be five feet deep and three feet in diameter; the edge of its mouth rounded off as if by art, and its sides and bottom retaining a considerable degree of polish. From its mouth for three feet and a half down it was filled with a red argillaceous earth, thinly mixed with minute parts of transparent quartz crystals: the remaining foot and a half to the bottom contained an aggregate of water-worn stones, which were from the size of a goose's egg to that of a small walnut, and consisted of red jaspers, yellowish white flints, white quartz, and blueish white agates, firmly combined by a yellowish brown stalactitical calcareous spar. In this breccia I could not discover any fragment of the mountain rock, or any other calcareous matter, except the cement with which it was combined. This pot is 940 feet above the level of the sea.

Upon the west side of the mountain towards its base some strata occur, which are heterogenous to the mountain rock: the first or highest forms the segment of a circle; its convex side is towards the mountain, and its slopes also in that direction. This stratum consists of a number of thin beds; the outward one, being the thinnest, is in a state of decomposition, and is mouldering down into a blackish brown or ferruginous coloured earth. The beds inferior to this progressively increase in breadth to 17 inches, where the stratification rests upon a rock of an argillaceous nature.

This last bed, which is 17 inches thick, consists of quartz of a blackish blue colour, in the septa or cracks of which are found fine quartz crystals, colourless and perfectly transparent. These crystals are composed of 18 planes, disposed in hexangular columns, terminated at both extremities by hexangular pyramids. The largest of those that I have seen does not exceed two-eighths of an inch in length: they in general adhere to the rock by the sides of the column, but are detached without difficulty. Their great degree of transparency has obtained them the name of Gibraltar diamonds.

[*To be continued.*]

## ACCOUNT OF NEW BOOKS.

Memoirs of the Literary and Philosophical Society of Manchester, Vol. V. Part I. 8vo. 318 pages, with four plates. Price 6 shillings in boards. Cadell and Davies, London, 1798.

**T**HIS volume contains, 1. *Curfory Remarks, moral and political, on Party Prejudice.* By Samuel Argent Bardsley, M. D.—2. *Extraordinary Facts relating to the Vision of Colours; with Observations.* By Mr. John Dalton. The intelligent author of this valuable paper is himself the subject of many of his observations respecting the peculiarity of his perceptions of colours. This Memoir consists of an account of his own vision; an account of others whose vision has been found similar to his own; and observations respecting the probable cause of the peculiarities he describes. The solar spectrum, formed by the prismatic dispersion of light, exhibits to him only two, or at most three, colours, which he should call yellow and blue, or yellow, blue, and purple. This yellow comprehends the red, orange, yellow, and green of others; and his blue and purple coincide with theirs. He enters minutely into the requisite comparative observations on the several colours respectively as they affect his organs of sight. Pink appears, by day-light, to be sky-blue a little faded; by candle-light it assumes an orange or yellowish appearance, which forms a strong contrast to blue. Crimson appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue. Red and scarlet have a more vivid and flaming appearance by candle-light than by day-light. There is not much difference in colour between a stick of red sealing-wax and grass by day. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour. The colour of a florid complexion is dusky blue. Coats, gowns, &c. appear to Mr. Dalton, and the other individuals he mentions, frequently to be badly matched with linings, when others say they are not. On the other hand, they should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens. In all points where they differ from other persons, the difference is much less by candle-light than by day-light. Mr. Dalton, by various observations, shews, almost beyond a doubt, that this affection of the eye is caused by some of the humours, probably the vitreous, being coloured by some modification of blue.—3. *An Enquiry into the Name of the Founder of Huln Abbey, Northumberland, the first in England of the Order of Carmelites; with Remarks on Dr. Ferriar's Account of the Monument in the Church of that Monastery.* By Robert Uvedale, B. A. of Trinity College, Cambridge.—4. *On the Variety of Voices.* By Mr. John Gough. It is a fact which continually presents itself to our observation, that sounds differ from each other in other respects as well as in musical tone and their intensity. The oboe, the violin, and the flute, however perfectly they may be made to agree in these respects, are ever found to differ in something which may be called the character of the tone itself. The author of this paper conceives that the variety of human voices is of this last kind; and, in his reasoning on the philosophy of sound, he considers every tone as compounded. So that the same cotemporaneous sounds which are heard in the bell, though practically considered as if it emitted the fundamental note only, being conceived to exist in all other tones, these will be found to differ according to the number, the nature or relation, and the intensity of their component parts. That circumstances



stances of this kind may modify the aggregate of sonorous undulation, is scarcely to be doubted; but Mr. Gough seems to have overlooked that modification, which may be proved to be the distinguishing circumstance between tone and tone when they are in unison. If a toothed wheel be made to revolve very swiftly with its teeth against a string, which shall strike each tooth in succession, a musical tone will be produced, which will be more acute the swifter the rotation. But this tone will differ accordingly as the material of the wheel is itself disposed to give a tone of greater acuteness or gravity by each single or individual stroke. If the same note be afforded by a brass and by a wooden wheel revolving on one axis, the character of each note will differ in the manner here stated. Or more simply, if a cord of thirty or forty feet in length, or shorter if more convenient, be stretched, and in this state struck with the edge of the hand, the vibrations will be slow enough to be counted, and will evidently appear to be of two kinds. When the string is struck near the end or bridge, a wave or undulation will be propagated to the other bridge, and immediately return; which process will continue as long as the elasticity of the string can maintain the motion. The waves of this undulation will be larger or smaller, the more remote or the nearer the place of percussion is to the bridge; but the times of transmission of the waves backwards and forwards along the string will be invariable. The tone of such a string, supposing its dimensions and tension to be such as would produce sound, may therefore be considered as if the tone produced by the system of waves were interrupted as often as the re-action of each bridge causes the retrograde motion of the system; that is to say, it is afforded by a process similar to that of the experiment with the wheels, where the peculiar sound of a blow upon each tooth was incessantly repeated and suspended at regular intervals. Now, the musical acuteness or gravity of the tone depends altogether on the time of these intervals, and not at all on the nature of the sound which is thus repeated; but the character of the tone is governed by this last circumstance. And accordingly it is found by those who make harpsichords, and other similar instruments, that the character of the tone of the same string is wonderfully changed, accordingly as the jacks or hammers are made to operate nearer to, or further from, the bridge.—5. On the Benefits and Duties resulting from the Institution of Societies for the Advancement of Literature and Philosophy. By the Reverend Thomas Gisborne, M. A.—6. On an Universal Character. By James Anderson, LL.D. F.R.S. &c. By this letter, which bears date February 20, 1795, it appears that the Doctor had then made considerable progress in the investigation of the means of writing language by characters regularly constructed on those principles of mental operation which constitute universal grammar, and do not require the intervention of sound. To those who have considered this subject, particularly with respect to the practice of the Chinese, whose written words are known to represent, for the most part, things instead of words, and consequently are intelligible to nations who do not speak the same language, the practicability of this scheme will be a matter of no doubt. But whether the difficulties be such as to operate more strongly against its introduction in Europe than the immediate motives of convenience in favour of its reception, is a question which can only be decided by minute and continued attention to the whole subject. That these difficulties are comparatively small, and would yield to the industry of active and intelligent men, in the first instance, is scarcely to be doubted, provided the exertions of the first promoters of this scheme were carried so far as to afford a grammar, a dictionary, and one or two well written introductory

ductory books on morals, political economy, or the more confined sciences of mechanics, chemistry, or astronomy. The facility and accuracy of enunciation, the speed and brevity of description, and the extreme rapidity of perusal, would soon realize those expectations upon which our author and a few men of reflection now meditate as the objects of enjoyment destined for remote future ages. The Doctor has not entered into the particulars of his own scheme; and the present occasion does not admit of a detail of the multiplied means and advantages which present themselves in the contemplation of this interesting subject. From the date of his letter, it is to be feared that other avocations may have suspended this pursuit, as well on his own part as that of his able friend whom he mentions as being actively engaged in it.—7. *The Inverse Method of Central Forces.* Communicated by Dr. Holme.—8. *Observations on Iron and Steel.* By Joseph Collier. After a concise account of the methods of smelting and refining iron, a description is given of the manufacture of steel by cementation, with a good drawing exhibiting the plan and section of a furnace for performing this operation. I do not recollect meeting with a drawing of this kind in any English author. Mr. Collier's paper exhibits a summary of the most essential facts related by Reaumur, Duhamel, Vandermonde, Monge, and Berthollet, and others\*.—9. *Remarks on Dr. Priestley's Experiments and Observations relating to the Analysis of Atmospheric Air, and his Considerations on the Doctrine of Phlogiston and the Decomposition of Water.* By Theophilus Lewis Rupp. The modern chemistry which rejects phlogiston is ably defended in this paper, of which, from the close method of quotation and remark according to which it is written, it is impossible to give any fair account in this place.—10. *An Account of three different Kinds of Timber Trees, which are likely to prove a great Acquisition to this Kingdom, both in point of Profit, and as Trees for Ornament and Shade.* By Charles White, Esq. F.R.S. The trees are the black American birch with broad leaves, the Athenian poplar, and the iron oak with prickly cups. For the minute particulars of description, as well as the perspicuous detail of advantages, recourse must be had to the paper itself.—11. *An Analysis of the Waters of two Mineral Springs at Lemington Priors, near Warwick, including Experiments tending to elucidate the Origin of the Muriatic Acid.* By William Lambe, M. A. late Fellow of St. John's College, Cambridge.—12. *Some Account of the Persian Cotton Tree.* By Matthew Guthrie, M.D. F.R.S. &c.—13. *Experiments and Observations on the Preparation and some remarkable Properties of the Oxygenated Muriate of Potash.* By Thomas Hoyle, jun.—14. *Experiments and Observations on Fermentation and the Distillation of Ardent Spirit.* By Joseph Collier. On the chemical papers last mentioned, as well as the subsequent paper on bleaching, I make no remarks, not only for the sake of brevity, but because I hope on a future occasion to give a more ample account of their contents.—15. *Hints on the Establishment of an Universal Written Character.* By William Brown, M.D. Notwithstanding the great value of this paper, and the ability of its author, who has occasionally turned his thoughts during three years to Dr. Anderson's project, I cannot help wishing that he may not be the friend to whom that author refers. His hints contain an outline of the process by which an universal written character is to be established; but he seems rather disposed to consider the pro-

\* The author mentions certain omissions in my Chemical Dictionary, article Iron, which he would have found under the word Steel, if he had turned to that article:

ject as being very laborious, and in its practical application difficult to be established. Under such impressions, much is not to be expected in the progress. It seems nevertheless that mankind have rejected universal characters when offered to them, not because they were averse from the means of facility and perspicuity, but because those means were not in reality displayed. The Arabic numerals, the notation of music, and the symbols of quantity and operation used by algebraists, are universal characters confined to no language, and have been universally received because they are simple and appropriate; but the characters of chemists, as well as the few attempts at the more ample designation of things in the same manner, have not been received, because their authors have stopped short in their use and adaptation. If Bishop Wilkins, and the great men of the last century who favoured his universal character, had studied and used it to such an extent as to have written treatises according to that notation; or if the great Bergman had attempted a reform in the characters of chemistry, and adopted such as could be readily formed, easily distinguished, and compounded, with all the advantages of position, of which he was so well aware; if he had completed a set of tables exhibiting the whole of chemical science, and expressing things, quantities, actions, temperatures, and other habitudes, in a few pages, which those who attentively meditate on his compound tables of elective attraction may without difficulty conceive to be possible; the lovers of that science would have learned to read his work, and to write others according to the same method; and that for reasons of the same kind as have induced arithmeticians, musicians, and algebraists, to become masters of the universal characters proper to their respective sciences.—16. On the Process of Bleaching with the Oxygenated Muriatic Acid, and a Description of a new Apparatus for Bleaching Cloths with that Acid dissolved in Water, without the Addition of Alkali. By Theophilus Lewis Rupp.—17. Account of a remarkable Change of Colour in a Negro. By Miers Fisher. This man's father was the son of a native African and an Indian of Philadelphia. His mother was the daughter of an African man and an Irish woman. By a certificate here exhibited, it appears, that until the month of February 1792, being then at least 30 years old, he was of as dark a complexion as any African; at which period his skin began to change white, like that of an European, commencing at his fingers' ends, and proceeding, chiefly in the summer or warm weather, over the rest of his body. The greater part of the surface was thus changed at the time of the account being written, which was the 22d of November 1796. The change was not uniform over the whole surface, but gradually progressive along the skin; the black and white parts being separated by an irregular line. No facts are mentioned in explanation of this remarkable change.

Transactions of the Royal Society of Edinburgh, Vol. IV. Quarto. 470 pages, with 12 plates. Printed for Cadell and Davies in London; and Dixon and Balfour, Edinburgh. 1798.

This work is divided into three parts.

Part I. contains the History of the Society, or Reports concerning Memoirs presented, and Communications made, with a List of the Officers and Members, Donations, and other similar Particulars. It also contains Biographical Accounts of Lord Abercromby; of William Tytler, Esq. of Woodhouselee; of Mr. William Hamilton, late Professor of Anatomy and Botany in the University of Glasgow; and of John Roebuck, M. D.

Part II.

Part II. contains, I. Papers of the Physical Class.—1. Account of a Mineral from Strontian, and of a peculiar Species of Earth which it contains. By Thomas Charles Hope, Professor\* of Medicine in the University of Glasgow.—2. Observations on the Natural History of Guiana. By William Lochead, Esq.—3. On the Principles of the Antecedental Calculus. By James Glenie, Esq.—4. Observations on the Trigonometrical Tables of the Brahmins. By John Playfair, Professor of Mathematics in the University of Edinburgh.—5. Some Geometrical Porisms, with Examples of their Application to the Solution of Problems. By Mr. William Wallace, Assistant Teacher of the Mathematics in the Academy of Perth.—5†. On the Latitude and Longitude of Aberdeen. By Andrew Mackay, LL.D. & F.R.S. Edin.—6. An Account of certain Motions which small lighted Wicks acquire when swimming on a Basin of Oil. By Patrick Wilson, F.R.S. Edin. and Professor of Practical Astronomy in the University of Glasgow. (See *Philos. Journal*, II. 167.)—7. Account of a singular Halo of the Moon. By William Hall, Esq. of Whitehall, F.R.S. Edin.—8. A New Series for the Rectification of the Ellipsis, with Observations on the Evolution of a certain Algebraic Formula. By James Ivory, A.M.—9. Mineralogical Description of the Mountain of Gibraltar. By Major Imrie.—10. Description of a Thermometer, which marks the greatest Degree of Heat and Cold from one Time of Observation to another. By Alexander Keith, Esq. F.R.S. & F.A.S. Edin.—11. Description of a Barometer which marks the Rise and Fall of the Mercury from two different Times of Observation. By Alexander Keith, Esq. F.R.S. & F.A.S. Edin.—12. Meteorological Abstract for the Years 1794, 1795, 1796.—II. Papers of the Literary Class.—1. On the Origin and Principles of Gothic Architecture. By Sir James Hall, Bart. F.R.S. & A.S.S. Edin.—2. M. Chevalier's *Tableau de la Plaine de Troye* illustrated and confirmed from the Observations of subsequent Travellers and others. By Andrew Dalzel, M.A. F.R.S. Edin. Professor of Greek in the University of Edinburgh.

\* Now Joint Professor of Chemistry in the University of Edinburgh.

† By mistake, No. 5. is repeated.

Fig. 1.

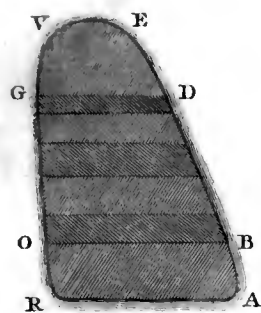


Fig. 2.

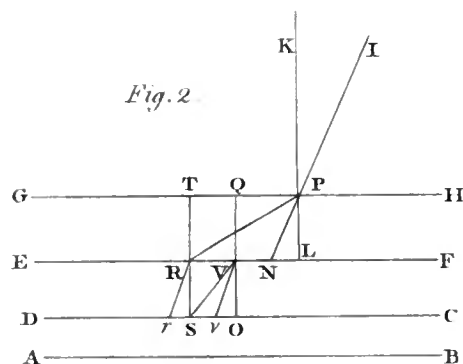


Fig. 3.

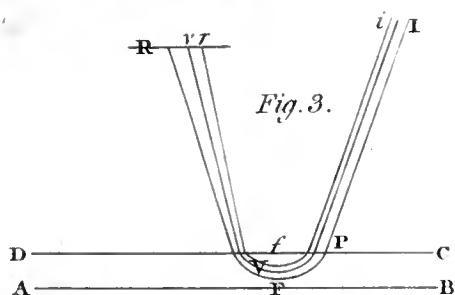


Fig. 4.

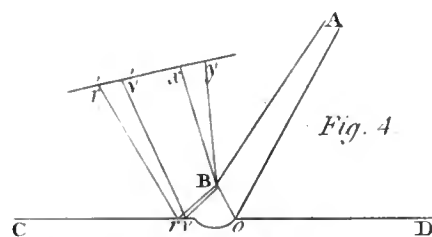


Fig. 5.

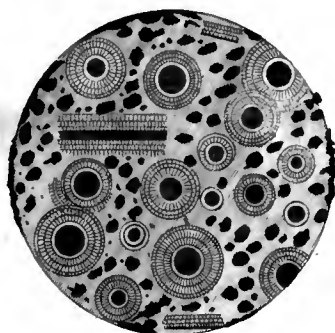


Fig. 6.

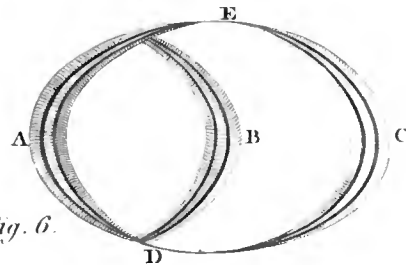


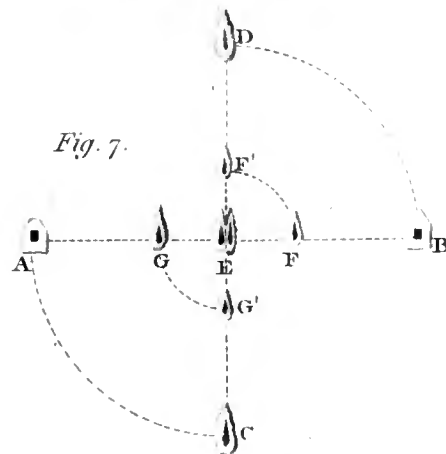
Fig. 8.



Fig. 9.



Fig. 7.



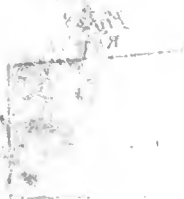
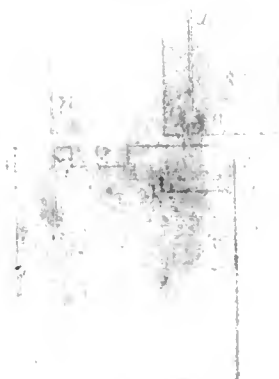


Fig. 1.

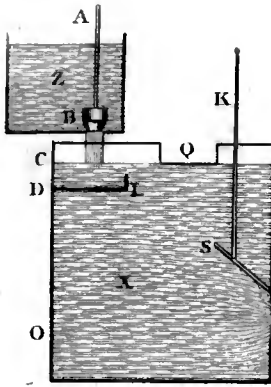


Fig. 2.

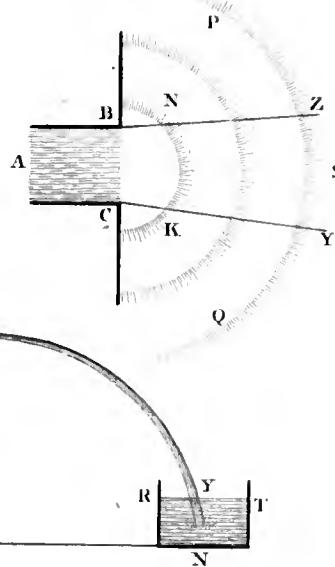


Fig. 3.

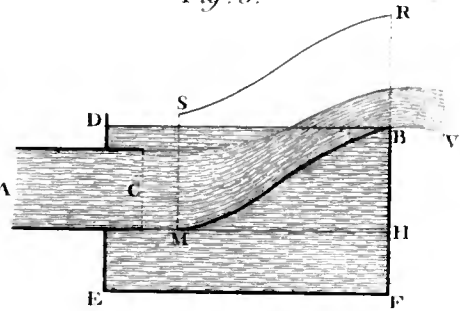


Fig. 4.

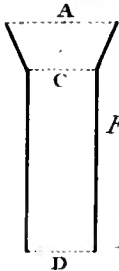


Fig. 5.

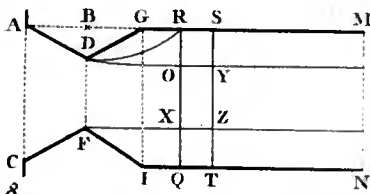


Fig. 6.

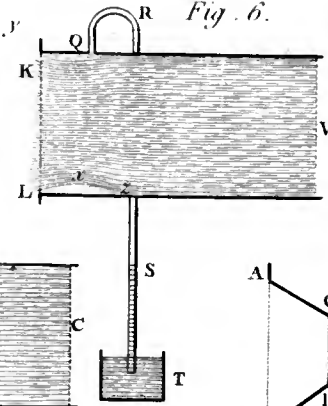


Fig. 7.

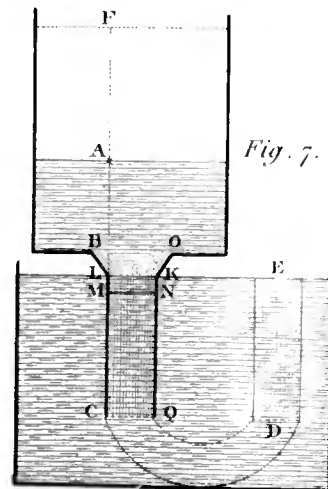


Fig. 8.

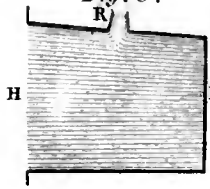


Fig. 9.

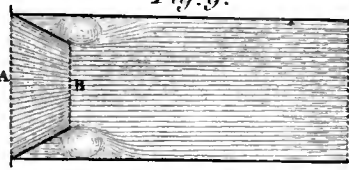


Fig. 10.

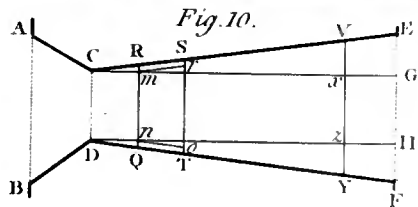


Fig. 12.

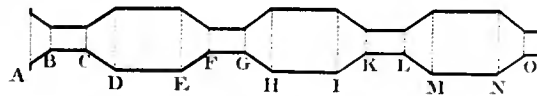


Fig. 13.

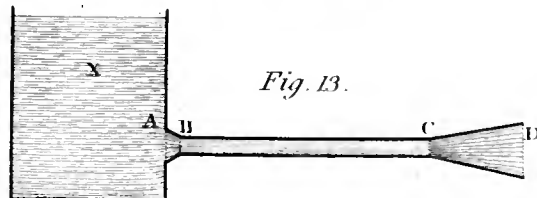
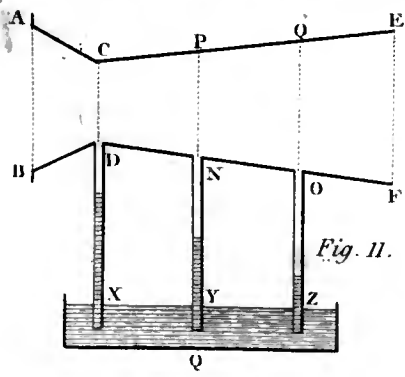


Fig. 11.







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NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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AUGUST 1798.

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ARTICLE I.

*Farther Experiments and Observations on the Affections and Properties of Light.*  
By HENRY BROUGHAM, jun. Esq.

[Concluded from Page 155 of the present Volume.]

*Observation 5.*

THE direct light falling on the speculum, and part of the reflected light on the horizontal white stage of a very accurate micrometer, I measured the breadth of the fringes, spots, &c. These, with the distance of the speculum from the window and micrometer, and the size of the sun's image, are set down in the following table, all reduced to inches and decimals.

			Inches.	Parts.
Distance of the speculum from the hole in the window shut	-	-	24.	
Distance of the speculum from the stage of the micrometer	-	-	18.	
Transverse axis of the sun's image	-	-	2.6	
Conjugate axis of the sun's image	-	-	1.4	
Length of the oblong dark spot	-	-	.4	
Breadth of the oblong dark spot	-	-	.0074	
Breadth of its first fringe	-	-	.0022	
Elliptic spot's transverse axis	-	-	.0016	
———— conjugate axis	-	-	.0068	
Breadth of its first fringe	-	-	.0034	
Transverse axis of a larger elliptic spot	-	-	.013	
Conjugate axis of the same spot	-	-	.0076	

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In the image where these measures were taken, there were seven other elliptic spots, a little less and nearly equal; all the others were much smaller and more confused.

*Observation 6.* On viewing the surface of the speculum attentively in that place whence the rays formed the oblong and first mentioned elliptic spots, I saw a dark but very thin long scratch, and a dark dent, similar in shape to the dark spaces on the image; the dark spot measured less than  $\frac{1}{230}$  of an inch; which makes its whole surface to the whole polished surface, as 1 to 34225, supposing the former circular or nearly so. All these measures will be found to agree very well, for their smallness and delicacy: thus, the ratio last mentioned is nearly the same which we obtain by comparing the image and the spot: the like may be said of the two spots mentioned in the table, i. e. their axes are proportional. I now could produce what spots I pleased, by gently scratching the speculum, or by making lines, dots, &c. with ink, and allowing it to dry; for these last formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced *a priori*.

*Observation 7.* The whole appearance which I have been describing bore such a close and complete resemblance to the fringes made round the shadows of bodies, that the identity of the cause in both cases could not be doubted. In order, however, to shew it still further, I measured the breadths of two contiguous fringes in several different sets: the measurements agreed very well, and gave the breadth of the first fringe .0056, and of the second .0034; or of the first .0066, and of the second .0034. The ratio of the breadths by the first is 28 to 17; by the second, 30 to 17; of which the medium is 29 to 17: and this is precisely the ratio of the two innermost fringes made by a hair, according to Sir Isaac Newton's measurement; the first being, according to him,  $\frac{1}{170}$  of an inch; the second,  $\frac{1}{290}$  of an inch\*. Farther, the two innermost rings made by plates have their diameters (not breadths) in the ratio of  $1\frac{1}{16}$  to  $2\frac{3}{8}$ †, and the distance between the middle of the innermost fringes (made by a hair), on either side the shadow, is to the same distance in the second fringes as  $\frac{1}{3}$  to  $\frac{2}{7}$ ; therefore, the diameters of the two first rings made by the specks in the speculum, are as  $\frac{209}{1363}$  to  $\frac{627}{1363}$ ; which ratio differs exceedingly little from that of  $1\frac{1}{16}$  to  $2\frac{3}{8}$ , the ratio of the diameters of rings made by plates, either those called by Newton thick, or those which he names thin: for suppose this difference nothing,  $2\frac{3}{8} \times \frac{209}{1363} = 1\frac{1}{16} \times \frac{627}{1363}$ ; and the difference between these two products (now stated equal) is not much above  $\frac{1}{9}$  in reality.

*Observation 8.* The last thing worth mentioning in these phenomena was this: I viewed the fringes through a prism, holding the refracting angle upwards, and the axis parallel to that of the dark space; then moving it till the objects ceased descending, I saw in that posture the fringes much more distinct and numerous; for I could now see five with ease, and several more less distinctly. This led me to try more minutely the truth of the 5th proposition, with respect to the number of the fringes surrounding the shadows of bodies in direct light. Having produced a bright set of these by a blackened pin  $\frac{1}{25}$  of an inch in diameter, I viewed them through a well made prism, whose refracting angle was only 30°, and held this angle upwards, when the fringes were on the side of the shadow opposite to me. I then moved the prism round on its axis; and when it was in the posture between the ascent and descent of the objects, I was much pleased to see five fringes plainly, and a great number beyond, decreasing in size and brightness till they became too small and confused for

\* Optics, book iii. obs. 3.

† Book ii. parts 1 and 4.

light. In like manner, those formed by a double flexion of two bodies, and those made out of homogeneous light, were seen to a much greater number when carefully viewed through the prism. And this experiment I also tried with all the species of fringes by flexion which I could think of.

*Observation 9.* The same appearances which were occasioned by the metal speculum, might be naturally expected to appear when a glass one was used. But I also found the like rings or fringes of colours and spots in the image beyond the focus of a lens; nor was a very excellent one belonging to a Dollond's telescope free from them. The rings with their dark intervals resembled those floating specks so often observed on the surface of the eye, and called "*muscæ volitantes*," only that the *muscæ* are transparent in the middle, because formed by drops of humor: they will, however, be found to be compassed by rings of faint colours, which will become exceedingly vivid if the eyes be shut and slowly opened in the sun's light, so that the humor may be collected; they also appear by reflexion, mixed with the colours described in *Phil. Trans.* for 1796, p. 268, (or *Philos. Journal* L. 593.)

*Observation 10.* The sun shining strongly on the concave metal speculum, placed at such a distance from the hole in the window that it was wholly covered with the light; upon inclining it a little, the image on the chart was bordered on the inside with three fringes similar to those already described: on increasing the inclination these were distended, becoming very bright and beautiful: when the inclination was great, and when it was still increased, another set of colours emerged from the side next the speculum, and was concave to that side. Here I stopped the motion, and the image on both sides the focus had three sets of fringes, and four fringes in each set; but when viewed through a prism (as before described), the numbers greatly increased, both the fringes and the dark intervals decreasing regularly. The appearance to the naked eye is represented in fig. 6, plate VII. where A D O being the image, A and C are the sets of fringes at the edges, and B the third set, there being none at E and D the sides, since the light which illuminates these quarters comes not from the edges of the speculum in so great inclinations. I now viewed the surface of the speculum, and saw it in the place answering to B in the image, covered with fringes exactly corresponding with those at B; and on changing the figure of that part of the speculum's edge between them and the sun, the fringes likewise had their figure altered in the very same way. On moving the speculum farther round, B came nearer to A in the image, according as the fringes on the speculum receded from that side which formed them; and before they vanished alike from the speculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours: among these I particularly remarked a brown chocolate colour, and various other shades and tinges of brown and purple. Just before the fringes at B appeared, the space between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them so minutely as I wished in this broad image. I therefore made the following experiment:

*Observation 11.* At the hole in the window-shut I held the speculum, and moved it to such an inclination that the colours by reflexion might be formed in the image: they were much brighter and far more distended than the fringes, and were in every respect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were also seen on the speculum, as the third set of fringes had before

been in Obs. 10; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though less distinctly, by a single reflexion. I now held a plain metal speculum, so that the rays might be reflected to form a white image on a chart. On inclining the speculum much, I saw the image turn red at the edge; it then became a little distended; and lastly, fringes emerged from it well coloured, and in regular order, with their dark intervals. This may easily be tried by candle-light with a piece of looking-glass; and those who without much trouble would satisfy themselves of the truth of the whole experiment contained in this and the last observation, may easily do it in this way with a concave speculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almost superfluous to add, that the fringes at B, fig. 6, are formed by deflexion from the edge of the speculum next the sun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decomposed at its first reflexion, and then undergoing a second, or, in other instances, without this second reflexion; and that the other fringes are produced exactly as described above, from the necessary consequences of the theory. I shall only add, that nothing could have been more pleasing to me than the success of this experiment; not only because in itself it was really beautiful from its variety, but also because it was the most peremptory confirmation of what followed from the theory *a priori*, and in that point where the singularity of its consequences most inclined me to doubt its truth.

Let us now attend to several conclusions to which the foregoing observations lead, independently of the propositions (*viz.* the five first) which they were made to examine.

I. We must be immediately struck with the extreme resemblance between the rings surrounding the black spots on the image made by an ill-polished speculum, and those produced by thin plates observed by Newton; but perhaps the resemblance is still more conspicuous in the colours surrounding the image made by any speculum whatever, and fully described in Obs. 10 and 11. The only difference in the circumstances is now to be reconciled. The rings surrounding the black spot on the top of a hubble of water, and those also surrounding the spot between two object-glasses\*, have dark intervals (exactly like those rings I have just now described, and the fringes surrounding the shadows of bodies); but these intervals transmit other fringes of the same nature, though with colours in the reverse order; from which Sir Isaac Newton justly inferred, that at one thickness of a plate the rays were transmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexivity nor refrangibility will account for either sort of rings; because the plate is far too thin for separating the rays by the latter, and because the colours are in the wrong order for the former; and also because the whole appearance is totally unlike any that refrangibility and reflexivity ever produce. To say that they are formed by the thickness of the plates, is not explaining the thing at all. It is demanded, in what way? And indeed we see the like dark intervals and the same fringes formed at a distance from bodies by flexion, where there is no plate through which the rays pass. The state of the case then seems to be this: "When  
" a phenomenon is produced in a particular combination of circumstances, and the same  
" phenomenon is also produced in another combination, where some of the circumstances  
" before present are wanting; we are entitled to conclude, that the latter is the most gene-

\* Optics, book ii. p. 11.

“ral case, and must try to resolve the other into it.” In the first place, the order of the colours in the Newtonian rings is just such as flexion would produce; that is, those which are transmitted have the red innermost, those which are reflected have the red outermost: the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermost and innermost must be understood relative position only, or position with respect to the thickness of the plate, not of the central spot. Secondly, the thinnest plate makes the broadest ring (the diameter of the rings being in the inverse subduplicate ratio of the plate’s thickness): just so is it with fringes by flexion; nearer the body of the fringes are broadest, and their diameters increase in the same ratio with the diameters of the rings by plates whose thickness is uniform; each distance from the bending body therefore corresponds with a ring or fringe of a particular breadth, and the alternate distances correspond with the dark intervals. The question then is, what becomes of the light which falls on or passes at these alternate distances? In the case of thin plates, this light is transmitted in other rings: we should therefore be led to think, that in the case of the light passing by bodies, it should be at one distance inflected, and at another deflected: and in fact the phenomena agree with this; for fringes are formed by inflexion within the shadows of bodies; they are separated by dark intervals; the fringes and the intervals without the shadow decrease in breadth according to the same law, so that the fringes and intervals within the shadow correspond with the intervals and fringes without respectively. Nor will this explanation at all affect the theory formerly laid down; it will only (if found consistent with farther induction) change the definite spheres of inflexion and deflexion into alternate spheres. At any rate, the facts here being the same with those described by Newton, but in different circumstances, teach us to reconcile the difference, which we have attempted to do, as far as is consistent with strictness; and what we have seen not only entitles us to conclude that the cause is the same, but also inclines us to look for farther light concerning that cause’s general operation: and I trust some experiments which I have planned, with an instrument contrived for the purpose of investigating the ratio of the bending power to the distances at which it acts, will finally settle this point.

II. Another conclusion follows from the experiments now related, viz. that we see the great importance of having specula for reflectors delicately polished, not only because the more dark imperfections there are on the surface the more light is lost, and the more colours are produced by flexion (these colours would be mostly mixed, and form white in the focus), but also because the smallest scratches or hairs being polished produce colours by reflexion, and these diverging irregularly from the point of incidence are never collected into a focus, but tend to confuse the image. Indeed it is wonderful that reflectors do not suffer more from this cause, considering the almost impossibility of avoiding the hairs we speak of: however, that they do actually suffer is proved by experience. I have tried several specula from reflecting telescopes, and found that though they performed very well from having a good figure, yet from the focus (when they were held in the sun’s light) several streaks diverged, and were never corrected; others had the hairs so small that it was very difficult to perceive the colours produced by them unless they fell on the eye. Glass concaves were free from these hairs, but they were much more hurt by dark spots, &c. In general the hairs are so small in well wrought metals that they do little hurt; but when enlarged by any length of exposure to the light and heat in solar observations, they produce irregularities

round:

round the image. Such at least I take to be the explanation of the phenomenon observed at Paris by M. de Barros during the transit of mercury in 1743, and recorded in *Phil. Transf.* for 1753. But there is another more serious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the history has been given, on viewing attentively the surface of the speculum, every part of it was seen covered with points of colours, formed by reflexion from the small specular particles of the body. I never saw a speculum free in the least from these; so that the image formed in the focus must be rendered much more dim and confused by them than it otherwise would be.

III. The last conclusion which may be drawn from these experiments is a very clear demonstration in confirmation of what was otherwise shewn, concerning the difference between coloured images produced by reflexion and those made by flexion. This complete diversity is most evident in the experiments with specula, the colours produced by which, in the form of fringes and rings, ought, as well as the others described as images by reflexion in *Obs.* 11, to be the same in appearance with those formed by pins; whereas no two things can be more dissimilar.

It remains to examine the 6th proposition. For this purpose I made the following observations:

*Observation 1.* Having procured a good specimen of Iceland crystal, I split it into several pieces, and chose one whose surface was best polished. I exposed this to a small cone of the sun's light, and received the reflected rays on a chart; nothing was observable in the image farther than what happens in reflexion from any other polished body. Some pieces indeed doubled and tripled the image, but only such as were rough on the surface, and consequently presented several surfaces to the rays. When smooth and well polished, a single image was all that they formed. The same happened if I viewed a candle, the letters of a book, &c. by reflexion from the Iceland crystal.

*Observation 2.* I ground a small piece of Iceland crystal round at the edge, and gave it a tolerable polish here and there by rubbing it on looking-glass, and sometimes by a burnisher (it would have been next to impossible to polish it completely). I then placed the polished part in the rays near the hole in the window-shut, and saw the chart illuminated with a great variety of colours by reflexion, irregularly scattered as described above\*. I therefore held the edge in the smoke of a candle and blackened it all over, then rubbed off a very little of the soot, and exposed it again in the rays. I now got a pretty good streak of images by reflexion, in no respect differing from those made in the common way. Nor could I ever produce a double set or a single set of double images by any specimen properly prepared, either on a chart by the rays of the sun, or on my eye by those of a candle.

*Observation 3.* I ground to an even and pretty sharp edge two pieces of Iceland crystal, and placed one in the sun's rays. At some feet distance I viewed the fringes with which its shadow was surrounded, and saw the usual number in the usual order. I then applied the other edge so near that their spheres of flexion might interfere in the manner before described†, and thus the fringes might be distended: still no uncommon appearance took place, nor when other bodies were used with one edge of crystal, nor when polished pieces

\* *Phil. Transf.* for 1756, p. 270, or *Phil. Journal* I. 594.    † *Ibid.* p. 256, or *Phil. Journal* I. 587.

of different shapes and sizes were employed. The same things happened by candle-light, and also by refracted homogeneous light. In short, I repeated most of my experiments on flexion with Iceland crystal, and found that they were not changed at all in their results.

*Observation 4.* Having great reason to doubt the accuracy of an experiment tried by Mr. Martin, and in which, by a prism of Iceland crystal, he thought six spectra were produced, I was not much surprised to find that a prism made by polishing the two contiguous sides of a parallelepiped of Iceland crystal produced only two equal and parallel images, in whatever position the prism was held. But though, from the imperfect account which Martin gives of this appearance, it was impossible to discover his error from his own words, yet chance led me to find out what most probably had misled him; for, looking at a candle through the opposite sides of a specimen of Iceland crystal, I saw four coloured images (besides two white ones) of the candle. These were parallel to one another, and in the same line as represented in fig. 7, plate VII. where E represents the two regular images, G and F two others coloured very irregularly, and changing colours as the crystal was moved horizontally, sometimes appeared each two fold, and its two parts of the same or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the sides. On turning the crystal round so that its position might be at right angles to its former position, the images moved round, and were in a line perpendicular to AB, as CD. All this happened in like manner in the sun's rays; and on viewing the specimen I found it was split and broken in the inside, so as to be lamellated in directions parallel or nearly so to the sides: on these plates there were colours in the day-time by the light of the clouds; and it is evident that it was these fractures which caused the irregular images G and F, for other specimens shewed no such appearance. I would therefore conclude, that Iceland crystal separates the rays of light into two equal and similar beams by refraction, and no more\*.

As to the cause of the separation, I would hope that some information may be obtained from the experiments I have related: for from them it appears, that this singular property extends no farther than to the action of the particles of Iceland crystal on the particles of light in their passage through the body; and from Obs. 4, it is farther evident, that it is not owing to the different properties which Sir Isaac Newton conjectures the different sides of rays to have; for, if this were the cause, when the rays pass between two pieces of crystal an uncommon flexion would take place. Lastly, another fact (mis-stated by Bartolin† and Romé de Lisse‡) shews that the unusual refraction takes place within the body, while the other, like all refractions, begins at some small distance before the rays enter.

The writers just now quoted assert, that if the crystal be turned round so as to assume different positions, there is one in which the line appears single. The fact is very different,

\* Mentioning this account of Martin's mistake to Professor Robison of this university, I was pleased to find a full confirmation of it. It was that excellent philosopher who shewed the appearance to Martin; but he not understanding it, took the liberty of publishing the observation as his own, after first mangling it in such a way as to give him indeed some pretext for the appropriation. The Professor merely mentioned his having communicated it to Mr. Martin: how the latter used it we have shewn in the text: the theory of the appearance is somewhat more complex than appears by my observations. I was therefore pleased to find that the Professor was in possession of the true account of it, which is however foreign to the present purpose. B.

† *Experimenta Crystalli* abridged in *Phil. Trans.* vol. v.

‡ *Cristallographie*, vol. i.

as follows : When the crystal is turned round, the unusual image turns round also, and appears above the other : the greatest distance between the two images is when they are parallel to the line bisecting one of the acute angles of the parallelogram through which the rays pass : when the images are parallel to a line bisecting one of the obtuse angles they seem to coincide ; but they will be found, if observed more nearly, to coincide only in part. Thus (in fig. 9, plate VII.) A B and C D are the two black lines at their greater distance, and their extremities A and C, B and D, are even with one another ; that is, the figure formed by joining A and C, B and D, is a rectangle. But in the other case (fig. 8), A B and C D being the lines, the space C B (equal in depth of colour to the real line on the paper) is the only place in which the lines (or images) coincide. The space A C of A B, and B D of C D are still of a light colour, and the two lines A B and C D do not coincide, by the difference A C or B D ; that is, by the difference O P, the greatest distance (fig. 9). In short, the unusual line's extremities describe circles (in the motion of the crystal) whose centres are the extremities of the usual line, and whose radii are the greatest distance. From this it appears evident, that the unusual image is formed within the crystal, and turns round with the side of the particle or rhomboidal mass of particles which forms it. Farther, it is evident that the power which produces the division of the incident light is very different from common refraction, from the motion, and the effect taking place when the rays are perpendicular. Suspecting, therefore, that it might be owing to flexion, I made the following experiment, which undeceived me :

*Observation 5.* I covered one side of a specimen of Iceland crystal three inches deep with black paper, all but a small space  $\frac{1}{8}$  of an inch in diameter, and placed a screen with a hole of the same size, six feet from the hole in the window-shut of my darkened chamber, so that the rays might pass through the screen and fall on a prism placed behind, to refract them into a small and well defined spectrum, which was received on a chart two feet from the prism. This spectrum I viewed through the crystal, and of course saw it doubled ; but the two images were by no means parallel : the unusual one inclined to the red, and its violet was considerably farther removed from the violet of the other, than the two reds were from one another ; which shews that the most refrangible or least flexible rays were farthest moved from their course by the unusual action, and proves this to be very different from flexion\*.

From all these observations this conclusion follows : that the remarkable phenomenon in question arises from an action very different from either refraction or flexion, and whose nature well deserves to be farther considered. It may possibly belong to the particles of Iceland crystal, and in a degree to those of rock crystal, from the form and angles of the rhomboidal masses whereof these bodies are composed. Nor is this conjecture at all disproved by the fact, that glass shaped like these bodies wants the property ; for we cannot mould the particles of glass, we can only shape large masses of these ; whereas we cannot doubt that in crystallization the smallest masses assume the same form with the largest. But then other hypotheses may perhaps also account for the fact, such as atmospheres, electric fluid, &c. &c. ; so that till farther observations are made we ought to rest contented with barely suggesting the query. In the mean time, reserving to a future opportunity some inquiries concerning the chemical

\* When a candle or line is viewed through a deep specimen, the unusual image is tinged with colours.



properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But, first, may I be permitted to express a hope, that what has been already attempted (and for which no praise can be claimed farther than what is due to attentive observation, according to the rules of the immortal Bacon) may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations, as exhibited in one of the most pleasing, most important, and most unerring walks of physical science.

*Proposition I.* The sun's light consists of parts which differ in degree of refrangity, reflexivity, inflexity, and deflexity; and the rays which are most flexible have also the greatest refrangity, reflexivity, and flexity—or are most refrangible, reflexile, and flexible.

*Proposition II.* Rays of compound light passing through the spheres of flexion, and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from, the body.

*Proposition III.* The colours of thin and those of thick plates are precisely of the same nature, differing only in the thickness of the plate which forms them.

*Proposition IV.* The colours of plates are caused by flexion, and may be produced without any transmission whatever.

*Proposition V.* All the consequences deducible from the theory *à priori* are found to follow in fact.

*Proposition VI.* The common fringes by flexion (called hitherto the “three fringes”) are found to be as numerous as the others.

*Proposition VII.* The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.

*Proposition VIII.* This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it, in acting as well on perpendicular as on oblique rays, and in its sphere or space of exertion moving with the particles which it attends.

## II.

*Observations on Bituminous Substances, with a Description of the Varieties of the Elastic Bitumen. By CHARLES HATCHETT, Esq. F.R.S. Lond. and Edin. F.L.S. &c.\**

### SECT. I.

**I**T is now generally believed that the bituminous substances are not of mineral origin, but that they have been formed from certain principles of substances belonging to the organized kingdoms of Nature, which, after the loss of animal and vegetable life, have suffered considerable changes by long contact and union with mineral bodies.

These changes have been however so considerable, that the bitumens can no longer be referred to their first origin, and they are therefore regarded by general consent as forming part of the present mineral system.

\* Read before the Linnæan Society in June and July 1797.

The bituminous substances are :

Naptha,	Jet,
Petroleum,	Pit Coal,
Mineral Tar,	Bituminous Wood,
Mineral Pitch,	Turf,
Asphaltum,	Peat, and

those combinations of the oxides of certain metals with bitumen called Bituminous Ores\*.

Those who are acquainted with the nature of these substances will immediately perceive, that they may be formed into two divisions : the first of which consists of simple species, or unadulterated bitumens : and the second is composed of bitumen mixed or combined with the earths, vegetable matter, and metallic oxides ; so that these appear to merit the name of compound species.

I shall now first consider how the simple species are connected with each other.

## SECT. II.

IT has been the opinion of some eminent Naturalists and Chemists, that naptha is an ethereal oil produced from the more compact and solid bitumens by a sort of natural distillation. This however appears to be an hypothesis founded upon analogy, and supported only by a few local facts, which may often be questioned. But many facts and observations concur to prove that the contrary most frequently happens, and that the compact bitumens are often, if not always, formed from naptha and petroleum by inspissation. I will not however, now insist upon the proofs of this, as the varieties of the elastic bitumen, which I shall soon describe, will be sufficient for the purpose †.

## NAPTHA.

NAPTHA is a substance well known to Mineralogists, as a light, thin, often colourless oil, highly odoriferous and inflammable, which is sometimes found on the surface of the waters of springs, and at other times issuing from certain strata.

When exposed to the air, it becomes at first yellow, afterwards brown, and in the like proportion it thickens, and passes into

## PETROL or PETROLEUM.

THIS has a greasy feel, is thicker than the preceding substance, is transparent or semi-transparent, and of a reddish or blackish brown colour. By air it becomes like tar, and then is called

\* As I intend only here to notice the modifications of naptha and petroleum, I have not mentioned amber and the honey-stone.

† Bergman was of opinion, that the liquid bitumens were often, if not always, formed from those which are solid, by the means of subterraneous heat; and expresses himself thus: "Cæterum ad fidem pronum est, napham, petroleum, bituminosque liquores, quibus abundat Asia, plures harum materierum exhibens non tantum scaturigines, sed rivulos quoque, quibus etiam, parcius licet distributis, Australis Europa non caret: probabile, inquam, est, has pinguedines liquidas variis antea terris inhæsisse exsiccatas, et mediante calore subterraneo, si non semper, sæpe tamen fluiditatem recuperasse. Novimus ignem in alto haud rarè agere, quamvis in superficie vix obscura ejusdem indicia investigare liceat: novimus præterea e sicco aluminari schisto petroleum extorqueri justo caloris gradu, cui arte exponitur.—Bergman de Productis Vulcaniis Opuscula, tom. iii. p. 238.

**MOUNTAIN OR MINERAL TAR, BITUMEN PETROLEUM TARDE FLUENS.**

THIS substance is viscid, and of a reddish or blackish brown or black. When burned, it emits a disagreeable bituminous smell, and by exposure to the air it passes into

**MOUNTAIN OR MINERAL PITCH—BITUMEN MALTHA.**

THE mineral pitch much resembles common pitch, and, when heated, emits a strong unpleasant odour, like the former substance. When the weather is cold, it may be broken, and then exhibits, internally, a glassy lustre; but when warm, it is softened, and possesses some tenacity. It is however susceptible of a superior degree of induration, and then becomes

**ASPHALTUM—BITUMEN ASPHALTUM—PETROLEUM INDURATUM.**

THIS is a light, brittle substance, of a brownish black, or black. When broken, it shews a conchoidal fracture with a glassy lustre. It has little of the bituminous odour, unless it is rubbed or heated. It easily melts, is very inflammable, and, when pure, burns without leaving any ashes.

In this manner, naptha, by inspissation, passes successively through different states until it becomes asphaltum, which appears to be the ultimate degree of induration which the pure bitumens derived from naptha can receive.

I have at this time specimens before me which prove these gradations; and I have seen a remarkable instance in a bitumen brought from the Island of Trinidad, which exhibits mineral tar passing into mineral pitch, and lastly into asphaltum\*.

**SECT. III.**

THE division which comprehends the simple bituminous substances derived from naptha, may therefore be considered as terminating in asphaltum; but nature appears to have glided on by an uninterrupted chain which connects the simple bitumens with those which we have called compound; and this effect is produced by the gradual increase of the carbonic principle, and the introduction of extraneous matter, the different quantity of which, together with the greater or less degree of mixture or of chemical union, occasion considerable changes in these substances, so that they are gradually removed from those characters which distinguish the pure bitumens.

To form an accurate table of these gradations, it would be necessary to have comparative analyses of the different bituminous substances, and also to contrast the analyses with the properties of these bodies. But at present these analyses, for the greater part, are wanting; and although at some future time I intend to attempt a series of such experiments, I must now content myself with the observations and facts which I have been able to collect†.

\* The progressive changes of naptha into petroleum, mineral tar, mineral pitch, and asphaltum, appear to be caused by the gradual dissipation of part of the hydrogen of the bitumen, and the consequent development or disengagement of carbon. Hence, I am inclined to believe, arise the changes of colour, the degrees of inspissation, and the increased proportion of carbon found in those substances by chemical analysis. I would be understood however to mean that the carbon is only relatively increased, in respect to the other ingredients, in a given quantity of these bitumens, and that it predominates in proportion to the dissipation of a certain portion of the hydrogen, which was originally necessary to the forming of the bitumen in conjunction with the carbon.

† This paper was written and read before I had seen the ingenious experiments which the celebrated Mr. Kirwan has published, in the last edition of his *Elements of Mineralogy*.—Vide vol. ii. p. 514, (*Philos. Journ. I. 437.*)

From these I am of opinion, that the most immediate gradation from asphaltum (which is the last of the simple bitumens) into those which are compound, takes place in the substance called

### JET.

JET is a substance well known to be of a full black, sometimes however inclining to brown. It is considerably harder and less brittle than asphaltum. It breaks with a conchoidal fracture, and the internal lustre is glassy. It has no odour except when heated, and it then resembles asphaltum. It melts in a strong heat, and, when burned, leaves an earthy residuum.

Wallerius considered jet as asphaltum which had become indurated by time, and Mr. Fourcroy is of the same opinion\*. Others again have arranged it with the varieties of coal†. I am inclined however to believe, that it is neither asphaltum nor coal, but an intermediate substance which may be regarded as the first gradation from the simple bitumens into those which are compound. The matter of asphaltum undoubtedly enters into it in a large proportion, and has consequently stamped several of its characters upon it; but the increase of carbon, and of the extraneous or earthy matter which is intimately mixed or rather combined with it, has had so much influence, that the characters of coal are also in some measure apparent, and are rendered the more striking by the similarity of certain local circumstances which attend these two substances. The characters of coal are however by no means fully established in jet; but from this we pass immediately to another, in which these characters cannot be questioned.

This is the substance called

### CANNEL COAL,

which is of a full black, of a smooth, solid, even texture; it breaks in any direction, and the transverse fracture is conchoidal. It burns well, and is so compact that it is often employed, like jet, to be formed into trinkets.

The great resemblance which cannel coal has to jet in many of its properties, induces me to regard it as the next gradation of the compound bituminous substances, and as the leading variety of coal from which the others follow according to the degree of their bituminous character.

The limits of this paper will not allow me to enter into a circumstantial account of all the other varieties of pit-coal; neither is it necessary, after the gradations of asphaltum to jet, and of jet to coal, have been noticed. I shall not therefore describe the varieties of coal known by divers names in different countries, and even in different provinces, such as those called in England caking coal, rock coal, splent coal, &c. &c.; but shall only observe, that the pit coals in general appear to be composed of bitumen intimately mixed, or rather combined, with various proportions of carbon and earthy matter; and according to the intimacy of the union, and the excess of one or other of the ingredients, so the compound possesses more or less the characters of perfect coal, or, by various shades, passes into certain earthy or stony substances, which, although impregnated with bitumen, do

\* *Elémens d'Hist. Nat. et de Chimie*, tom. iii. p. 456.

† *Widenmann's Handbuch der Mineralogie*, p. 628.

not merit the appellation of coal, and these, also at length gradually lose the bituminous character\*.

It is likewise worthy of notice, that the quantity of earthy matter does not appear to be the principal cause why pit-coals do not burn with the rapidity which is to be perceived in some other earthy substances impregnated with bitumen. For we may conclude, that the slow combustion of coal proceeds from the joint effects produced partly by the relative proportions of the bituminous, carbonaceous, and earthy ingredients, and partly by the more or less perfect degree of mixture which connects them together, and which degree of mixture, I believe, in many cases, nearly approaches to chemical union, if not actually so: when, therefore, the degree of mixture is so perfect as that every particle of bitumen is connected with much carbon or earthy matter, it is not surprising that the rapid combustible property of the former should be checked in a considerable degree; and, by a parity of reasoning, when the mixture is gross and imperfect, so that it consists of a stony or earthy substance, which has simply imbibed bitumen, it is natural to expect that the bitumen (although less abundant than in coal) should enter readily into combustion, which is vehement in proportion to the shortness of its duration; and this we find to be the case in many earthy substances, and loose sand-stones which are simply impregnated with bitumen.—To return, however, to the varieties of coal, I must observe, that, from the causes above-mentioned, the different characters and properties of coal appear to me to be produced. That in this manner, perfect pit-coal passes into schistose or slaty coal; and this again, by certain gradations, passes into the varieties of combustible or bituminous schistus; which also, by the gradual decrease of the bituminous ingredient, become at length confounded with the varieties of the common or argillaceous schistus.

We have a remarkable example of this in the gradations of bituminous schistus into argillaceous schistus, which are to be observed at Kimmeridge, on the coast of Dorsetshire, where a peculiar bituminous schistus is found, which is used as fuel by the inhabitants, and is improperly called Kimmeridge coal.

By the series of gradations which have been noticed in the foregoing pages, the simple bituminous substances appear to pass into those which are compound; and these also, by declining shades, at last pass into substances appertaining to the class of earths and stones.

In the compound bituminous substances the prevalent earthy ingredient is for the greater part generally, if not always, argillaceous; and although certain calcareous grits (such as the Portland stone†) as well as limestones and marbles are found impregnated with bitumen, yet I know not of any instance in which this happens to the degree requisite to form a combustible substance.

This cursory view of the simple bitumens, and of their combinations, would be sufficient as an introduction to the principal subject of this paper; but, to complete the series, I shall make some observations on the vegetable substances which contain bitumen, and shall afterwards mention the mixtures of bitumen with metallic oxides.

\* From Mr. Kirwan's experiments it appears that carbon is a constituent principle of coal, and that the presence of it is a principal cause of those modifications which produce the species. It even seems chiefly to form the Kilkenny coal.—Kirwan's Elements of Mineralogy, vol. ii. p. 521.

† The Portland stone, when recently broken in the quarries, emits a strong bituminous odour, like the bituminous limestone or stink-stone. It is also full of extraneous fossils, or at least the vestiges of them.

## SECT. IV.

WHEN we consider the facts which apparently prove that vegetables have contributed principally to the formation of bitumen, we have every reason to expect that mixtures of vegetable matter with bitumen should frequently occur. But by the mixture of bitumen with the parts of vegetables, we understand the remains and parts of vegetables mixed and connected with the bitumen which they themselves have produced.

This seems to be the nature of the substance called

## BITUMINOUS WOOD, as well as of TURF and PEAT.

BITUMINOUS or fossil wood is found in many places; but in respect to that which is found at Bovey, near Exeter, and which is therefore called Bovey coal, there are some peculiarities which deserve to be mentioned. The Bovey coal is a dark brown, light, brittle substance, which in texture and other external properties much resembles wood which has been half-charred. It is not found as scattered logs or trunks, but forms regular strata.

The pits are on a heath which is flat and sandy; the stratum of sand is however but thin, after which a pale brownish grey clay is found mixed with quartz pebbles. This prevails to about six feet, at which depth the first stratum of the coal commences. The quality of this is however much inferior to that of the subsequent strata, which in all amount to seventeen, producing a depth of nearly seventy-four feet from the surface. Between each stratum of coal is a stratum of clay. The direction of the strata is from east to west, and the inclination or dip is from north to south. The inferior strata are thought to afford the best coal, and the coal is more solid and of a better quality towards the south. The thickest stratum of coal is from six to eight feet\*.

The Bovey coal burns readily with a flame like half-charred wood: it does not crackle, and, if but moderately burned, forms charcoal; or if completely burned, it leaves a small quantity of white ashes exactly similar to those of wood. The smell of it when burning also resembles that of wood, with a faint disagreeable odour. It is certainly very remarkable that this substance should form regular strata, although it possesses the texture and most of the properties of wood; and that these strata do not exhibit any of those irregularities on their surfaces, which might be expected, on the supposition that they were formed by the roots, trunks, and branches of trees long buried in the earth. It is also difficult to imagine wood to have been transported and deposited in this place at seventeen different periods, and yet it must be allowed that these strata have been formed by successive operations. I must confess, that after having twice visited and examined the spot expressly for the purpose, I still find myself utterly unable to offer any opinion upon the subject.

The characters of bitumen are but little apparent in the Bovey coal, and the superior strata even appear to have lost a portion of their combustible principle, while the inferior strata possess it. The lower parts also of these strata are more compact and more combustible than those parts which are immediately upon them†.

Another

\* In the winter, twelve men can raise about 120 tons of this coal in a week, the whole of which is employed in a neighbouring pottery.

† At about 100 yards to the west of the pits, is a bog of considerable extent, where peat is cut, and decayed roots and trunks of trees are found, which do not, however, in the least approach to the nature of the Bovey coal.

Another remarkable sort of fossil wood, which much resembles the Bovey coal, and in like manner is arranged among the bituminous woods, is that found in Iceland, which is called by the inhabitants Surturbrand. This is rather harder than the Bovey coal, but in every other respect is the same. It also forms strata many feet in thickness; but it is very extraordinary that these strata appear to be formed of trunks of trees, which, in their transverse section, exhibit the concentric circles of their annual growth, with this difference, that the trunks have been so compressed as to be nearly flat, so that the circles appear like parallel lines connected at their extremities by a short curve.

I did not observe such an appearance at Bovey; but this would depend upon the position of the trunks of the trees, in respect to the section of the strata.

Chaptal \*, Troil †, Bergman ‡, and many others, have been of opinion that the Surturbrand is wood which has been charred by the heat of the lava. But I cannot discern why it should be supposed that it has been acted upon by fire, any more than that the Bovey coal has been subjected to the effects of the same agent. The qualities of the two substances are the same; and as (from Archbishop Troil's and Professor Bergman's account) the Surturbrand is stratified, I think we may venture to pronounce that the circumstances under which they are found, are also similar §. The whole, therefore, of the opinion in favour of fire, appears

coal. Whether this bog has been in any manner connected with the formation of the above-mentioned substance, I do not pretend to determine.

A yellowish brown compact substance, which in colour and fracture resembles ferruginous clay, is also found occasionally with the Bovey coal: it is brittle, and is highly inflammable; it melts like a bitumen, and emits a smoke which in smell resembles amber. This substance is but rarely found.

\* Elements of Chemistry, vol. iii. p. 199.

† Von Troil's Letters, p. 43.

‡ Quid de ligno fossili Islandiæ sentiendum sit, gnaro in loco natali contemplatori decidendum relinquimus. Interea, ut cum Vulcani operationibus nexum credamus, plures suadent rationes, quamvis huc usque modum ignoremus, quo situm texturamque adquisiverunt hæc strata. Scilicet truncis arborum perquam crassis constant, qualis in Islandiâ nullibi reperiuntur, et ne quidem hoc tempore crescere posse videntur. Hi situ horizontali in stratis multorum pedum crassitie congesti sunt et petroleo plus minus penetrati, non jam molli, sed optimè indurato, a quo tam nigrorem, quam flammæ sub deflagratione qualitatem mutantur. Sed quod in primis attentionem meretur, est truncorum in lamellas planas compressio.

Ponamus truncum arboris cujusdam transversim sectum, hinc, uti notum est, figura oritur in orbem rediens circiter circularis, quæ omnia monstrat annotina incrementa, extimo propemodum parallela. Pingamus jam talem sectionem in tenuem laminam compressam, et veram habebimus ligni fossilis, de quo hæc agitur, ideam; nam in magnis hujus materiæ frustis, transversim sectis, quemlibet annotinorum orbium visu persequi licet, ita plerumque coactum, ut duas lineas fere parallelas exhibeat, quarum extrema brevi flexura sunt adunata.—Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? Nonne antea particulatum nexus putredinis quodam gradu fuerit relaxatus? Certe, nisi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Caterùm idem observatur phænomenon in omni schisto argillaceo.

Orthoceratitæ, quæ in strato calcareo conicam figuram perfectè servant, in schisto planum fere triangulare compressione efficiunt. Idem valet de piscibus, conchis, insectisque petrifactis. Causa adhuc latet, sed in utroque casu sine dubio eadem est, et digna quæ exploretur. Observatu quoque dignum est, quod idem reperiatur effectus, quamvis stratum calcareum sub schisto collocatum sit et majori ideo pondere comprimente onustum.—Bergman de Productis Vulcaniis Opuscula, tom. iii. p. 239.

§ "It is found (the Surturbrand) in many parts of Iceland, generally in the mountains, in horizontal beds; sometimes more than one is to be met with, as in the mountain of Lack in Bardestrand, where four strata of Surturbrand are found alternately with different kinds of stone."—Troil's Letters, p. 42.

to rest on the volcanic nature of Iceland; but it surely would be going too far were we to ascribe to fire all the phenomena which are observed in volcanic countries.

Bovey coal, like the furturbrand, resembles half-charred wood; and I will allow, and indeed am disposed to believe, that it is in a state nearly similar; but from this it does not follow that fire has been the cause.

Carbon is known to be one of the grand principles of vegetables, and also as that which is the most fixed, excepting the small portion of the earths contained in them. As a fixed principle, carbon appears to form, in great measure, the vegetable fibre; and after a certain degree of combustion, (by which the other principles have been dissipated,) it remains, and the particles of it keep the same arrangement which they possessed when the vegetable was complete. If, however, the combustion has been carried on with the free access of air, the carbon enters into combination with oxygen and caloric, and forms carbonic acid.

We have many examples in which carbon is formed or rather liberated from those substances with which it was combined in vegetables; and these are now explained as effects similar to those of combustion, although fire has not been the cause. In both cases the carbon has been freed from the more volatile principles; and under circumstances not favourable to the union of carbon with oxygen, the former must necessarily remain more or less undiminished.

During the combustion of vegetable matter, the more volatile principles contained in the vegetable fibre (which with carbon also form the resinous and other similar substances) appear to be first separated; and in proportion to this separation, the other more fixed substance, which we call carbon, is developed.

Thus, by the progress of combustion, wood becomes brown, and afterwards black; so that the state of the wood shews the degree of combustion to which it has been subjected, or, in other words, how far the separation of the other principles from carbon has been effected.

Combustion is therefore a species of analysis by which the principles of vegetables are separated, according to their affinities, and according to their degree of volatility. By this operation hydrogen and azote (if it be present in the vegetable) are first disengaged and form new combinations, while the carbon is the last which is acted upon; so that unless a sufficient quantity of oxygen be present, it remains fixed and unchanged.

But the same separation of the vegetable principles happens whenever vegetables in the full possession of their juices are exposed to circumstances which favour the putrid fermentation.—As in combustion, so by the progress of putrefaction does the vegetable lose its colour, become brown, and afterwards black; at the same time a gas is discharged, which is composed of hydrogen, azote, and carbonic acid.

When combustion is long continued with the free access of air, the whole of the carbon is dissipated in the state of carbonic acid; but in the process of putrefaction a considerable portion of carbon commonly remains even long after the putrid fermentation has ceased. Although, therefore, it is as readily developed by putrefaction as by combustion, it is not, however, when liberated from the other principles, so speedily dissipated by the former as by the latter process.

According to the degree of combustion within certain limits the carbon is more or less apparent, and the like prevails according to the degree of putrefaction; so that whenever the



the causes which have promoted this species of fermentation have ceased, the vegetable substance will remain with more or less of its first principles, and with more or less visible carbonic matter, according to the degree of putrefaction which has prevailed, and the vegetable substance will consequently have the appearance and properties of wood which has been charred more or less.

To this cause, therefore, I am inclined to attribute the formation and appearance of the Bovey coal and furturbrand; and I believe that the portion of oily and bituminous matter, which I have obtained from them by distillation, is nothing more than the remainder of the vegetable oils and juices which have been partly modified by mineral agents\*.

[ *To be continued.* ]

### III.

*New Methods of affording, at an inconsiderable Expence, the Heat and the Water required for performing Experiments in Chemistry.* By Citizen GURTON†.

**T**HERE is but one sure road to arrive at truth in natural philosophy, namely, by consulting nature herself by experiments. Independent of the sagacity necessary to direct these to objects precisely determinate, and to combine the means of operating, there is likewise an art of performing them, or, to speak more properly, of giving facility without diminishing the certainty of their results. To awaken the industry of philosophers with regard to such resources as may be obtained for the multiplication of experiments at the least possible cost, must therefore be a labour of utility to the advancement of science. When Franklin was asked how he could afford the charges of his experiments on electricity, at a time when he was far from being in circumstances of independence, he replied, that a man who could not saw with a gimblet, and bore with a saw, was not fit for an experimental philosopher. The services which Bergman has rendered to chemistry, and particularly to mineralogy, by the introduction of the blow-pipe, are well known. What a number of valuable observations would still be wanting, if he had not put this instrument into the hands of those who were unable to procure, or have access to the furnaces of the laboratory!

It is in consequence of these reflections, and the invitation I have received for that purpose, that I have determined to describe the small manipulations by which I obtain a very considerable saving of fuel and of distilled water in chemical experiments, to which I may add the saving of time, that most inestimable of all the desiderata for experimental research.

\* "Coal not only forms the residuum of all vegetable substances that have undergone a slow and smothered combustion, that is, to which the free access of air has been prevented, but also of all putrid vegetable and animal bodies: hence it is found in vegetable and animal manures that have undergone putrefaction, and is the true basis of their ameliorating powers: if the water that passes through a putrefying dunghill be examined, it will be found of a brown colour; and if subjected to evaporation, the principal part of the residuum will be found to consist of coal. All soils steeped in water communicate the same colour to it in proportion to their fertility; and this water being evaporated, leaves also a coal, as Messrs. Hassenfraz and Fourcroy attest."—Kirwan on Manures, p. 154, vol. v. of the Transactions of the Royal Irish Academy.

† Read before the National Institute of France, the 26th Brumaire, in the year 6, and inserted in the 24th volume of the Annales de Chimie, page 311.

In the second volume of the memoirs of the ancient academy of Dijon, I gave a description of a box containing a kind of portable laboratory, composed of a lamp with three wicks, disposed in the figure of an equilateral triangle, to form an internal current of air, with supports for the different vessels of digestion, distillation, evaporation, &c. I made a solution of silver with common aqua fortis and the metal in an alloyed state, which answered very well as a re-agent, without having occasion for any other utensils but this box and apothecary's phials, which are every where to be found. A number of these boxes have been fitted up by Citizen Dumoutier, more particularly for travellers, and I have reason to think that they have proved useful. But this apparatus must necessarily be confined in its application, and is different from the object I purpose to describe at present.

Ten years ago, I constructed a lamp, on the principles of Citizen Argand, with three concentric circular wicks, each having an interior and exterior current of air. The effect surpassed my expectations with regard to the intensity of the heat; but it was difficult to prevent the destruction of the hard folder round the wicks; and the glass retorts were frequently melted at the bottom, and disfigured. It may easily be imagined, that the quantity of oil consumed was considerable; and as it could not be used at the same time for giving light, it had; to say the truth, no more than a remote application to the object of the present communication.

A short time afterwards it occurred to me, to substitute, instead of the glass chimney of Argand's lamp, a cylinder of copper with an indented part or ledge a few millimetres \* above the flame, to perform the office of the indented chimney of glass, and by that means to render it practicable to raise the wick to a certain height without smoking. This cylinder has three branches like a chaffing-dish. By this apparatus two or three decilitres of water (about half an English wine pint) may be brought to boil in a copper or glass vessel in about six or seven minutes. It has served, and I still use it, for a number of operations; but it was not till after I had observed the degree of heat obtained from the lamp in its ordinary state, and particularly since I have substituted instead of the metallic tube a chimney of glass cut off at the length of three centimetres (rather more than one English inch) above the contraction, that I perceived all the advantages it was capable of affording; and that by means of a moveable support for the reception of the different vessels, which may be fixed at pleasure by a thumb-screw, this lamp furnace, at the same time that it gives light, and consequently without any additional expence, may with facility be used for almost every one of the operations of chemistry; such as digestions, solutions, crystallizations, concentrations; the rectification of acids; distillations on the sand-bath, or by the naked fire; incinerations of the most refractory residues; analyses with the pneumatic apparatus, or of minerals by the saline fusion, &c. &c. I have not hitherto met with any exception but for complete vitrifications and cupellations; for even the distillations to dryness may be performed with some precautions, such as that of transferring the matter into a small retort blown by the enameller's lamp, and placing its bottom on a little sand-bath in a thin metallic dish.

The support, here mentioned, is simply a copper ring eight centimetres (3,15 inch) in diameter, which is raised or lowered by sliding on a stem of the same metal. It is described in the memoirs of the academy of Dijon as part of the portable laboratory; for which reason

\* For the value of the new measures and weights of France, see *Philos. Journal*, I. 332.

it is unnecessary to speak more largely of it in this place\*. Nothing more was required but to adapt it to the square iron stem which passes through the reservoir of the lamp. The connection is made by a piece of wood, in order that less of the heat might be dispersed. As the lamp itself is capable of being moved on its stem, it is easy to bring it nearer or remove it at pleasure from the vessels, which remain fixed; a circumstance which, independent of the elevation or depression of the wick, affords the means of heating the retorts by degrees, of moderating or suppressing the fire instantly, or of maintaining it for several hours at a constant or determinate intensity, from the almost insensible evaporation of crystallizable solutions to the ebullition of acids; properties never possessed by the athanor, of which chemists have boasted so much. The advantage of these will be properly valued by those operators, who know that the most experienced and the most attentive chemists meet with frequent accidents, by which both their vessels and the products of their operations are lost, for want of power in the management of the fire.

I must here enter into some detail, in order to establish upon positive facts the possibility of applying the heat of a lamp to the operations I have enumerated, as well as to communicate the results of my experience to those who in preference, or for want of more extensive means, may be inclined to use this apparatus. I do not hesitate to say in preference; for, in the best appointed laboratory, the lamp will also be used in such operations as may be made with equal facility on the same quantities, in much less time and more conveniently than by the fire of a furnace, by burning in the former instance one or two decimes (or penny-worths) of oil instead of five or six decimes of charcoal. The proof of this has been made in the laboratory of the Polytechnic School, at the conclusion of my last course.

For the analysis of stones, such as the crystals of tin, on which I operated before the class at the session of the first Messidor last, I use the shortened chimney of glass. I begin by placing the mixture in a capsule of platina or silver of seven centimetres ( $2\frac{3}{4}$  inches English) in diameter. I place this capsule on the support, and regulate the heat in such a manner that ebullition shall take place without throwing any portion of the matter out of the vessel. As soon as its contents are perfectly dry, I transfer them into a very thin crucible of platina, of which the weight is not quite eleven grammes ( $252\frac{1}{2}$  grains English), and its diameter forty-five millimetres ( $1\frac{3}{4}$  inch English). This crucible rests on a small support of iron-wire, which serves to contract the ring; and the wick being at its greatest elevation, with the ring lowered to the distance of twenty-five millimetres ( $9\frac{1}{2}$  inches English) from the upper rim of the glass chimney, I produce in less than twenty minutes the saline fusion to such a degree, that from the commencement of the operation the decomposition proceeds as far as to 0,70 of the mineral†.

The same apparatus, that is to say, with the shortened chimney, serves for oxidations, incinerations, torrefactions, and distillations to dryness.

In such operations as require a less heat, I leave the lamp with its large chimney absolutely in the same state as when it is used for illumination; and by raising and lowering either

\* Several philosophers who have seen this apparatus at work, having requested me to give a drawing, I have accordingly annexed a description of the figures, which represent the whole together at the end of this memoir. I think it may be called the Economical Laboratory.

† See the *Annales de Chimie*, xxiv. 132; or our *Journal*, I. 545.

the ring which supports the vessel, or the body of the lamp if the vessels be fixed in communication with others, I graduate the heat at pleasure. Vinegar distils without interruption at six centimetres ( $2\frac{1}{2}$  inches English) from the upper termination of the chimney, that is to say, at 19 centimetres ( $7\frac{1}{2}$  inches English) from the flame. Water is made to boil in eight minutes, at the same height, in a glass vessel containing five decilitres (one wine pint English), and is uniformly maintained at the distance of twenty-two centimetres ( $8\frac{3}{4}$  inches) from the flame. It will soon be shewn, that I have another method of supplying chemists with distilled water; so that I scarcely ever repeat this operation but when I have no other supply at hand, or am desirous of avoiding all trouble whatsoever. In this case, I obtain two or three decilitres (or quarter pints) of water in the course of a winter evening, without the least portion of my time being employed in attending to the operation.

I must not in this place omit to mention a slight observation which this process has afforded, because it may lead to useful applications, and tends to point out one great advantage of this method of operating; namely, that an infinity of circumstances may be perceived, which might not even be suspected when the whole process is carried on within a furnace. I have remarked, as did likewise several of my colleagues who were then present, that a column of bubbles constantly rose from a fixed point of the retort on one side of the bottom. We were of opinion, that some particle of matter was in that place incorporated with the glass, which had a different capacity (probably conducting power) for heat from that of the rest of the glass. In order to verify this conjecture, I endeavoured the following day to distil the same quantity of the same water in the same retort, after having introduced a button of cupelled silver, weighing nine decigrammes ( $20\frac{1}{2}$  grains). At the commencement of the operation there was a small stream of bubbles from the same point as before; but a short time afterwards, and during the whole remaining time of operating, the largest and most incessant stream of bubbles rose from the circumference of the button, which was often displaced by the motion; and in proportion to the time the product of the distillation was sensibly greater. Whence we may conclude, that metallic wires or rods, distributed through a mass of water required to be kept in a state of ebullition, and placed a little below its surface, would produce, without any greater expence of fuel, nearly the same effect as those cylinders filled with ignited matter which are made to pass through the boilers\*.

It now remains to shew the economical method of supplying the water necessary for experiments.

When water is mentioned in chemistry, pure water is always meant. In medical prescriptions it is usual to prescribe spring water, though in many places the water of springs or streams is more loaded with selenite or sulphate of lime than the well water of other places. The same remark may be applied to river water, which is no doubt more wholesome than

\* When we attempt to reason on this curious fact of the metallic button, there are various circumstances which require to be considered. It does not seem probable, that a larger quantity of a fluid can be rendered elastic by a given quantity of heat, unless we suppose part of the heat to have been wasted in the former process. Does the metal, by the excellence of its conducting power, convey a portion of the heat more readily into the mass of liquid than might have been done by the ordinary process of circulation? Or is the thin stratum beneath the button so far insulated as to become more suddenly elastic, and, by rising in that state to the surface, to increase the rapidity of circulation, and the number of points at which vapour can escape? Would a tuft of silver wire produce the same effect? &c. &c. N.

The water of wells in places where selenite or plaister abounds, but which are nevertheless far from being pure, and are necessarily subject to vary according to the quantity of rain water which dilutes that which has remained upon the soluble matter.

Recourse is therefore had to distillation, to purify the water employed in laboratories; but if on the one hand we consider the labour and expence it requires, and on the other hand the quantity which it is necessary to have in readiness for the smallest operations; it will not appear surprising, when I assert, that there are few days in which a chemist does not avoid making some experiment, or obtain uncertain results, for want of having this article in his power. It is only with distilled water that perfect reagents can be prepared; distilled water is consumed in infusions, macerations, solutions, and edulcorations; repeated lotions demand a large quantity of this fluid; it ought to be used even for rinsing vessels; and in order to avoid deceitful conclusions, it is even proper to use it in the hydropneumatic vessels.

I have long been in the habit of supplying most of these demands with rain water; not with that which is directly received, though in fact it is of considerable purity, particularly in countries where there is no reason to fear that plaister should rise with the dust; but the quantity of this would be too small. I have, therefore, used water collected with care from the roofs of houses after the rain has washed the surface. I filter it without delay; and in this manner from time to time I obtain a considerable provision, without much labour and at no expence. But it is evident, that in order to depend on the purity of this water, it is necessary that there should be no gypseous matter in the composition of the mortar of the roof, or the plaistering of the chimneys. This condition does not obtain at Paris; inso-much that I have sometimes found the first water of the gutters more selenitous than that of the Seine in the time of floods. I have thought of a method of supplying this resource, by a process which may be used in all countries; and the success which has attended my trials renders it a duty to communicate it, in order to place the instruments of analysis in the hands of a greater number of operators.

Rain water collected from the roofs of houses which have been previously washed, cannot, and in fact does not, contain any thing but the very small portion of sulphate of lime which it has taken up during its contact with the plaister of the chimneys and the pointing of the ridges. It is necessary, therefore, to deprive it of this, in order to have water in a very pure state. For this purpose I prepare a solution of barytes according to the process\* by which our colleague Vauquelin has rendered so easy what Bergman attempted with imperfect success. I pour this solution into the filtered rain water, until, after the precipitation has subsided, the last drop exhibits no alteration of transparency. I even add a small quantity in excess, which shews itself by the vinous colour it gives to paper coloured with fernambouc or brazil wood. This excess soon falls down in the state of carbonate of barytes, by simple exposure to the air. The precipitation may be very suddenly determined, by the addition of water impregnated with carbonic acid. Too much must not however be added, because it would take up a portion of the precipitate. But the spontaneous evaporation of the excess of gaseous acid in the open air would in this case soon restore its purity.

To judge with what facility and trifling expence the whole of the pure water for experi-

\* Philosophical Journal, I. 535. Dr. Hope in the Edinburgh Transactions, iv. 36. informs us, that, contrary to the assertions of many chemists, the native carbonate of barytes may be deprived of its acid by mere heat (of a smith's forge, in a black lead crucible). N.

ments may be procured, it is sufficient to observe, that by using an aqueous solution of barytes of which the specific gravity was no more than 1.0205, the quantity of 15 grammes in weight ( $344\frac{1}{2}$  grains English), or 0.1473 cubic metres (4.1 cubic inches English), proved sufficient completely to purify eleven decilitres ( $2\frac{1}{2}$  wine pints English) of water. Consequently one decilitre or part of the same aqueous solution of barytes will serve to afford 74.62 parts, or about 8 (French) pints of distilled water.

I must add an observation, which is very proper to shew that the collection of rain-water is worth the trouble and care of procuring it in the circumstances most favourable to its purity. By a comparative operation on rain-water, I found that river water acquired 60 grammes, or four times the quantity of the same solution of barytes.

I have no doubt but that the aqueous solution of barytes for the purification of water will, in the course of time, be introduced into manufactories for dyeing. It will serve, at a very moderate expence, to render the artist master of the shades he means to produce, without waiting for the season in which he considers his water as most pure. I gave this advice to a manufacturer who requested me to analyse the water of a small stream which supplied his works.

These observations may also probably be applied to another purpose. It is known that water saturated with sulphate of lime is much less putrescible than purer water: would it not be of advantage, in long voyages, to take a supply of water expressly loaded with this earthy salt, and, when it is wanted for use, to purify it three or four days before hand by a small portion of the solution of barytes?

This solution would occupy little room, and be attended with inconsiderable expence. If it were apprehended that a small portion of barytes might remain in solution, which would not in fact be without danger, as this earth is perceptibly noxious, a proof might be made; or, to speak more correctly, the depuration might be rendered absolute by the addition of a few drops of the solution of carbonate of soda. All these manipulations are among those which may be very easily practised by persons of no chemical experience.

*Explanation of the Figures of the Economical Laboratory, Plate IX.*

*Figure 1.* represents the whole apparatus ready mounted for distillation, with the tube of safety and a pneumatic receiver.

A is the body or reservoir of the usual lamp of Argand, with its shade and glass chimney. The lamp may be raised or lowered at pleasure by means of the thumb screw B \*, and the wick rises and falls by the motion of the small toothed wheel placed over the waste cup. This construction is most convenient, because it affords the facility of altering the position of the flame with regard to the vessels, which remain fixed; and the troublesome management of bended wires above the flame for the support of the vessels is avoided, at the same time that the flame itself can be brought nearer to the matter on which it is intended to act.

D, a support consisting of a round stem of brass, formed of two pieces which screw together at about two-thirds of its height. Upon this the circular ring E, the arm F, and the nut G slide, and are fixable each by its respective thumb-screw. The arm also carries a

\* In the lamps of this construction made in London there is a spring in the socket, sufficiently rigid to prevent the lamp A from falling by its weight when B is unscrewed. I suppose Citizen Guyton's lamp was provided with the same convenience, but that by oversight it may not have been mentioned. N.

moveable

moveable piece H, which serves to suspend the vessels in a convenient situation, or to secure their position. The whole support is attached to the square iron stem of the lamp by a piece of hard wood I, which may be fixed at any required situation by its screw.

K represents a stand for the receivers. Its moveable tablet L is fixed at any required elevation by the wooden screw M. The piece which forms the foot of this stand is fixed on the board N; but its relative position with regard to the lamp may be changed by sliding the foot of the latter between the pieces O O.

P, another stand for the pneumatic trough. It is raised or lowered, and fixed to its place, by a strong wooden screw, Q.

R is the tube of safety, or reversed syphon, invented by Citizen Welter, and described in the third cahier of the Journal of the Polytechnic School, p. 437\*.

Fig. 2. shews the lamp furnace disposed to produce the saline fusion; the chimney of glass shortened; the support D turned down; the capsule of platina or silver, S, placed on the ring very near the flame.

Fig. 3. The same part of the apparatus, in which, instead of the capsule, a very thin and small crucible of platina, T, is substituted, and rests upon a triangle of iron wire placed on the ring.

Fig. 4. exhibits the plan of this last disposition:

#### IV.

*An Account of some Experiments made by Mr. JOHN CUTHBERTSON, with a View to determine an unequivocal Method of ascertaining the Power of electrical Machines.*

**H**OWEVER great the influence and probable importance of electricity may appear in a large class, and perhaps in the whole of natural phenomena, we still find that a number of fundamental experiments remain to be made. Among these there is scarcely a more desirable object than to determine the degree of excitation or quantity of electricity afforded by machines, in proportion to the surface exposed to friction. When philosophers endeavour to communicate to each other the indications of power in their respective electrical apparatus, they either describe the length and appearance of the simple spark from a conductor, or the explosion from a certain measure of coated surface, or else the distance to which the attractive power of the prime conductor is rendered perceptible upon a thread or pendulous body. The first of these methods is subject to variation from the magnitude of the conductor itself, the figure of its termination, and particularly that undulation of

\* And also in the 2d vol. of the Annales de Chimie, p. 311. This apparatus serves, in a great measure, to prevent the bad effects of having the vessels either perfectly closed or perfectly open. Suppose the upper bell-shaped vessel to be nearly of the same magnitude as the bulb at the lower end of the tube, and that a quantity of water, or other suitable fluid, somewhat less than the contents of that vessel, be poured into the apparatus: In this situation, if the elasticity of the contents of the vessels be less than that of the external air, the fluid will descend into the bulb, and atmospheric air will follow and pass through the fluid into the vessels: but, on the contrary, if the elasticity of the contents be greater, the fluid will be either sustained in the tube, or driven into the bell-shaped vessel; and if the force be strong enough, the gaseous matter will pass through the fluid, and in part escape. N:

which

which the first account was given in our Journal, vol. i. page 83 : and the last method, being subject to modification, not only from the structure of the less essential parts of the machine, but also from the dimensions and figure of the apartment in which the experiments are made, has been accordingly very little used. Electricians have, therefore, with considerable reason, been disposed to avail themselves of the second method, according to the simple computation described in our work last referred to, page 87. But to this method also Mr. Cuthbertson, the constructor of the great Teylerian machine at Haerlem, offers serious objections; in consequence of which he took the trouble to repeat some experiments at my request. These experiments, together with his observations and such remarks as have occurred to myself, will form the subject of the present memoir.

The Honourable Henry Cavendish, Esq., from a series of experiments upon the charges of electrical jars \*, has deduced, that the quantities of electricity which coated glass of different shapes and sizes will receive with the same degree of electrization, are directly as the area of the coating, and inversely as the thickness of the glass; and that, when the intensities vary, the quantities of electricity in like circumstances are nearly as the length of the spark. Mr. Cuthbertson's great experience has led him to modify these general conclusions. I have found that in great intensities the length of the spark is much more than in proportion to the charge †; and from some facts hereafter to be related, there is reason to think that a real charge of low intensity cannot be measured either by the length of its very short spark, or even by the number of turns of the machine. Electricians, in general, use the best glass they can procure in their vicinity; whence their conclusions are, for the most part, applicable to one kind of glass only. But Mr. Cuthbertson has observed, that the different kinds of white glass, and still more the green, will require very different quantities of electricity to charge equal surfaces and thicknesses to the same height. He showed me a jar, of which the coating had been cut away until its capacity, as determined by the number of turns of the machine, became equal to that of another similar jar of the same thickness. The coated surface of the former of these two jars might be estimated at more than one-third part less than that of the former. Hence the necessity of some other test of electrical power, different from that which includes the dimensions of the jar as one of its elements, is evident. Mr. C. offers the explosion of steel wire for this purpose; the result of the facts observed by him being, that equal quantities of electricity, in the form of a charge, will cause equal lengths of the same steel wire to explode, whether the jar made use of be of greater or less capacity, within certain limits very easily comprehended in a loose verbal description. The primary object of his experiments was directed to the establishment of this proposition.

July 6th, 1798, I waited on Mr. Cuthbertson at seven in the evening, and found two electrical machines in a state of activity. The first consisted of a glass-plate 24 inches in diameter, rubbed by two pair of cushions, each 5 inches long, proceeding from the extreme of the circumference towards the centre upon opposite radii, whence the whole surface rubbed in each turn was 1193,8 square inches, or 8,29 square feet. The other machine was of the same dimensions, but had two plates and four pair of cushions. A jar fitted up with Lane's electrometer, the balls of which were invariably fixed, was used to ascertain the steadiness of the excitation. When this jar was applied to the prime conductor of the

\* Philosophical Transactions, lxvi. p. 196.

† Phil. Transf. lxxvi.



single plate-machine, it exploded five times in  $7\frac{1}{4}$  turns of the winch. Upon applying the same test to the double plate-machine it was found to afford very nearly, or rather more than twice the quantity of electricity. All the following experiments were made with the single plate.

A jar \*, No. 1, was applied to the prime conductor: after four turns some ramified flashes struck into the uncoated part, and at the sixth turn a spontaneous explosion took place over the clear glass. It must be remarked, that the mouths of none of the jars had any stopper or covering. Mr. C. then took a glass tube, which he inserted into the jar nearly to the bottom, and breathed twice through it, in order, as he said, to render it more capable of retaining its charge. This process, so contrary to the received opinion of electricians, who carefully avoid all dampness, is considered by him as of the same nature as the experiment of Brook, who found that a jar, the naked part of which was soiled by handling, would retain six times the charge without exploding, which it would have held if perfectly clean †. I had always considered this last experiment as a proof that glass, when greased by handling, does not attract vapour from the air as readily as when clean; and accordingly I have often used tallow as a good extemporaneous varnish for glass pillars. Mr. Cuthbertson's process, however, had the same effect; for the jar did not afterwards exhibit any flash or disposition to explode during the whole evening, though it was occasionally charged with ten and twelve turns. A brass ball supported on a stand by a stick of glass was placed so as to receive the explosion from the jar No. 1, when thus connected with the prime conductor. From this ball hung a small piece of pendulum wire ‡, confined at each end between small forceps constructed for that purpose. The clear portion of the wire which formed part of the circuit was exactly five inches in length. An explosion was passed through a moderate interval or length of spark; and this interval was gradually increased till, at last, the shock became so strong as completely to ignite the wire and divide it near the positive end, where a sparkling globule or two flew out: ten turns of the machine were required to produce this effect, and the length of the explosive spark was  $1\frac{1}{4}$  inch.

Another jar, No. 2 §, was applied to the prime conductor along with No. 1, and their outside coatings were connected together. The same quantity of wire as in the last experiment was placed in the circuit, and by the same method an explosion was obtained, which ignited and broke the wire with nearly the same appearances as before. In this case the number of turns were  $9\frac{3}{4}$ , and the length of spark  $\frac{3}{4}$  of an inch.

These experiments evidently confirm the result Mr. Cuthbertson obtained from his former trials; namely, that the quantity of electricity to disperse a given portion of wire will be

\* Height of the coating  $7\frac{1}{4}$  inches, diameter  $6\frac{1}{2}$  inches, both outside measure. Uncoated part  $4\frac{1}{4}$  inches perpendicular, though the mouth, in reality, was somewhat smaller than the body of the jar. Thickness 0,16 inch. Hence the surface was about 188 square inches, or  $1\frac{1}{4}$  square foot.

† Miscellaneous Experiments and Remarks on Electricity, &c. by A. Brook. Norwich, 1789.

‡ This wire is sold at the watch-tool makers. It is of steel flattened, and is used to make the small spring of the balance in watches. The wire here mentioned is 0,005 inch in breadth and one grain in weight; measures  $44\frac{1}{2}$  inches long.

§ Height of the coating  $8\frac{1}{2}$  inches, diameter 6,2 inches, both outside measure. Uncoated part  $3\frac{1}{2}$  inches perpendicular, though the mouth was, as in No. 1, somewhat smaller than the body of the jar. Thickness 0,17 inch. Hence the surface was about 190 square inches, or  $1\frac{1}{2}$  square foot, nearly the same as the other jar.

the same, even though the charged surface should be greatly varied. But it appeared desirable to ascertain the proportional quantities of electricity required to explode different lengths of the same wire. For this purpose 2,3 inches of wire were exploded by the jar No. 1, by five turns of the winch; which was probably too great a quantity, for the ignition was very strong, and deflagrating globules were thrown about. But by a more careful repetition of the experiment with  $2\frac{1}{2}$  inches of wire, the ignition and partial melting of the wire were very nearly the same as in the experiments which had been made on lengths of five inches. From these experiments there seems reason to suppose that the quantities of electricity may be as the lengths of the wires exploded.

A battery of fifteen jars, containing about 17 square feet of coated surface, was then used to explode five inches of the wire. Nineteen turns rendered the wire faintly red hot; but 20 turns caused it to explode in the same manner as in the experiments with the jars.

Half the length of wire, namely  $2\frac{1}{2}$  inches, was in the next place submitted to the explosion of the battery. Twelve turns ignited the wire, and 15 turns caused it to explode with somewhat more violence than in the experiments with the jars.

At this stage of the process, as the battery had required twice the quantity of electricity to produce the effect which had before taken place with a smaller quantity of coated surface, it became a question whether the length of the circuit, which was six feet on each side, might have influenced the results; and also whether the state of excitation had become less intense. For these reasons, the disposition of the apparatus was altered so that the circuit with the battery was the same as had before been used with the jars; and the trial jar, with Lane's electrometer, was again applied to ascertain the power of the machine. Five explosions were afforded by  $7\frac{3}{4}$  turns, as at first: this experiment was twice repeated, and shewed that the action of the machine continued to be the same as at first.

In this new disposition of the battery five inches of the wire were exposed to the explosion. No effect was produced by 12 turns; but by 21 turns the whole of the wire was dispersed in globules by a strong ignition. I suppose that 20 turns might have dispersed it in the same manner as in the first experiment.

Two inches and a half of the wire were then placed in the circuit. This portion was not affected by an explosion of 12 turns; but by 13 turns it was ignited and broken by the dissipation of a globule or two. Lastly, one inch of the wire was exposed to the shock; it was ignited, and partly dispersed in globules by 10 turns.

The whole of these experiments employed upwards of three hours, during the greatest part of which time the machine was in action. At the conclusion the trial jar was applied: it exploded five times in a little more than eight turns. The trial was repeated twice, and serves evidently to show the steadiness of the excitation, which had diminished only about  $\frac{1}{10}$ th part during this course of work.

Upon a review of the foregoing experiments, it is obvious that they would require to be repeated and extended, if the general course of Mr. Cuthbertson's former processes were not to be admitted in confirmation of the position, that equal quantities of electricity will ignite and disperse equal quantities of the same wire, without requiring any particular adjustment of the quantity of coated surface, provided the intensity be considerable. From the experiments with the battery it seems reasonable to conclude, that the quantities of electricity required to produce like effects upon wire will be greater, the lower the intensity, when the quantity

quantity of surface is greatly increased; in which case the velocity of the electric fluid may be supposed insufficient for the whole charge to exist in the conducting wire at one and the same time; or its impetus may be less; or, lastly, there may be a considerable waste from the conducting power of the air through the very thin stratum of air through which the explosion at last passes. This last supposition is far from being merely conjectural. For by some experiments on the charge of a plate of air in connection with the gold leaf electrometer, of which I may hereafter give an account, I find that even in very low intensities the electric matter will pass in a constant stream through intervals of about one inch; a conclusion which might likewise be deduced from the very small duration of the usual charge in the prime conductor.

With regard to the curious fact of the spontaneous explosion of a clean jar; which, when damped by breathing into it, was found much more capable of retaining its charge, there appears to be some difficulty in the theory. I have mentioned my supposition that Mr. Brook's experiment of soiling the jar with the hand, might have succeeded from the perspirable matter having operated as a varnish to the expulsion of moisture from the air. Mr. Cuthbertson's experiment overthrows this notion. I am now inclined to think that it depends upon undulation. The bare surface of clean glass may become charged in successive zones of the contrary electricities, as Priestley and others have shewn. When these zones have acquired the requisite intensity, they may explode into each other, and produce an undulation in the whole charge, which may greatly favour its flight through the interval from coating to coating. But when the surface is covered with distinct insulated particles of moisture, the escape from particle to particle must be by smaller leaps; the tranquillity of the charge will be scarcely at all disturbed; and the spontaneous explosion will not take place until the intensity has become so great, as to carry it through a space equal to the sum of the intervals between any one row of particles which form a line from coating to coating.

## V.

### *A Short Mineralogical Description of the Mountain of Gibraltar. By Major IMRIE.*

[Concluded from Page 187, Vol. II.]

AT no great distance from where these crystals are found, upon the same slope of the mountain, but rather nearer to the level of the sea, a stratum of argillaceous matter has been laid open, divided into many thin beds, the broadest of which does not exceed a foot in thickness. Its general colour is of a whitish grey, with a small mixture of yellow; and it is divided transversely by straight septa or cracks, both sides of which are covered with dendritical figures of a yellowish brown colour, beautifully representing the objects of landscape. At the western base of the mountain, on a level with the sea, by which it is washed, a very extensive stratum occurs of the same nature as the last described, bearing from north to south, parallel with and dipping towards the mountain nearly at an angle of 40 degrees.

In some parts of the western slope of the mountain, towards the south, are found nests of a dark red shivery clay, in which are imbedded flints of a dirty sap green colour: of those no regular stratum is to be perceived; many of them are unshapely masses; but they in gene-

ral tend to the rhomboidal form, and are from three to four inches long by two or three broad, and an inch and a half thick. They are not incruited as the flints found in chalk, nor have they the appearance of having been worn by attrition.

Upon different parts of the mountain, towards its base, are found large quantities of sand composed of different materials, and assuming various appearances as to colour. The largest bank of this arenaceous matter is upon the western side of the mountain, and consists of small particles of crystallized quartz, colourless, and perfectly transparent per se, but of an ochreous colour in the mass on account of a red argillaceous earth which adheres to them. The sand of this bank is perfectly loose and uncombined: one half of it has been levelled into an extensive parade, its surface having been combined by the lime and rubbish from the ruins of the town. The southern extremity of the bank is still to be seen in its natural state, and forms the burying-ground of the garrison.

Upon the east side of the mountain is found another of these banks, of considerable extent, and, as I mentioned before, rising from the Mediterranean in a rapid acclivity, and reaching to one-third of its entire elevation. This bank is composed of small particles of crystallized quartz, of testaceous bodies rounded by attrition, and of a few minute particles of the calcareous rock; the whole has a whitish grey colour. The rain-water which falls from the bare mountain rock above the sand brings along with it calcareous matter, which is deposited upon the bank, and combines its surface into a crust, which in some places is so much indurated as to bear the pressure of the foot.

In other parts of the mountain where this sand is surrounded by the calcareous rock, and covered in and protected from the action of the air and corrosion of sea-salts, it is found in a perfect indurated state, combined by stalaclitical spar, and forming a minute breccia. A quarry of this arenaceous stone has been opened upon the south-east quarter of the mountain, and is made use of with great propriety to line the embrasures of some of the new works belonging to the garrison. Its inaptitude to fly off in splinters when struck by a ball gives, in such situations, additional safety to the defenders of the place.

The western side of the mountain's base around Rosia Bay, and the New Mole, is a rock composed of an aggregate of small fragments of every fossil that has been here described, with the addition of two different species of marble, that are probably adventitious, as their native beds have not been found in the mountain. The one of those is black, and the other of an olive green colour. The whole of this mixture produces a most beautiful breccia, and is firmly combined by a calcareous cement of a yellow verging towards an orange colour. It is susceptible of a high polish, except where fragments of the argillaceous strata occur: these can be easily smoothed down, but cannot be brought to a perfect polish. The fragments in this breccia are angular, and none of them have the appearance of being water-worn.

It only now remains for me to mention, what are generally called the fossil bones found in the rock of Gibraltar. These have been much talked of, and by some looked upon as a phenomenon beyond the power of explanation. The general idea which exists concerning them is, that they are found in a petrified state and inclosed in the solid calcareous rock; but these are mistakes which could only arise from inaccurate observation and false description.

In the perpendicular fissures of the rock, and in some of the caverns of the mountain (all of which afford evident proofs of their former communication with the surface), a calcareous concretion

concretion is found of a reddish brown ferruginous colour, with an earthy fracture and considerable induration, inclosing the bones of various animals, some of which have the appearance of being human. These bones are of various sizes, and lie in all directions intermixed with shells of snails, fragments of the calcareous rock, and particles of spar; all of which materials are still to be seen in their natural uncombined states, partially scattered over the surface of the mountain. These having been swept by heavy rains at different periods from the surface into the situations above described, and having remained for a long series of years in those places of rest, exposed to the permeating action of water, have become enveloped in, and cemented by, the calcareous matter which it deposits.

The bones in this composition have not the smallest appearance of being petrified; and if they have undergone any change, it is more like that of calcination than that of petrification, as the most solid parts of them generally admit of being cut and scraped down with the same ease as chalk.

Bones combined in such concretions are not peculiar to Gibraltar: they are found in such large quantities in the country of Dalmatia, and upon its coasts in the islands of Cherso and Osero, that some naturalists have been induced to go so far as to assert, that there has been a regular stratum of such matter in that country, and that its present broken and interrupted appearance has been caused by earthquakes or other convulsions experienced in that part of the globe. But of late years, a traveller (Abbé Alberto Fortis) has given a minute description of the concretion in which the bones are found in that country: and by his account it appears, that with regard to situation, composition, and colour, it is perfectly similar to that found at Gibraltar. By his description it also appears that the two mountain rocks of Gibraltar and Dalmatia consist of the same species of calcareous stone; from which it is to be presumed that the concretions in both have been formed in the same manner and about the same periods.

Perhaps, if the fissures and caves of the rock of Dalmatia were still more minutely examined, their former communications with the surface might yet be traced, as in those described above; and in that case, there would be at least a strong probability that the materials of the concretions of that country have been brought together by the same accidental cause, which in my opinion has collected those found in the caverns of Gibraltar. I have traced in Gibraltar this concretion, from the lowest part of a deep perpendicular fissure up to the surface of the mountain. As it approached to the surface, the concretion became less firmly combined; and when it had no covering of the calcareous rock, a small degree of adhesion only remained, which was evidently produced by the argillaceous earth in its composition having been moistened by rain and baked by the sun.

The depth at which these materials had been penetrated by that proportion of stalaetitical matter, capable of giving to the concretion its greatest adhesion and solidity, I found to vary according to its situation, and to the quantity of matter to be combined. In fissures narrow and contracted, I found the concretion possessing a great degree of hardness at six feet from the surface; but in other situations more extended, and where a larger quantity of the materials had been accumulated, I found it had not gained its greatest degree of adhesion at double that depth. In one of the caves where the mass of concretion is of considerable size, I perceived it to be divided into different beds, each bed being covered with a crust of the stalaetitical spar from one inch to an inch and a half in thickness; which seems to indicate, that

that the materials had been carried in at various periods, and that those periods have been very remote from each other.

At Rosia Bay, upon the west side of Gibraltar, this concretion is found in what has evidently been a cavern, originally formed by huge unshapely masses of rock, which have tumbled in together. The fissure or cavern formed by the disruption and subsidence of those masses has been entirely filled up with the concretion, and is now exposed to full view by the outward mass having dropped down in consequence of the encroachments of the sea. It is to this spot that strangers are generally led to examine the phenomenon; and the composition having here attained to its greatest degree of hardness and solidity, the hasty observer, seeing the bones enclosed in what has so little the appearance of having been a vacuity, examines no farther, but immediately adopts the idea of their being incased in the solid rock. The communication from this former chasm to the surface, from which it has received the materials of the concretion, is still to be traced in the face of the rock; but its opening is at present covered by the base of the line wall of the garrison. Here bones are found that are apparently human; and those of them that appear to be of the legs, arms, and vertebræ of the back, are scattered among others of various kinds and sizes, even down to the smallest bones of small birds. I found here the complete jaw-bone of a sheep; it contained its full complement of teeth, the enamel of which was perfect, and its whiteness and lustre in no degree impaired. In the hollow parts of some of the large bones was contained a minute crystallization of pure and colourless calcareous spar; but in most the interior part consisted of a sparry crust of a reddish colour, scarcely in any degree transparent.

At the northern extremity of the mountain the concretion is generally found in perpendicular fissures. The miners there employed upon the fortifications, in excavating one of those fissures, found, at a great depth from the surface, two skulls, which were supposed to be human; but to me one of them, if not both, appeared to be too small for the human species. The bone of each was perfectly firm and solid; from which it is to be presumed that they were in a state of maturity before they were enclosed in the concretion. Had they appertained to very young children, perhaps the bone would have been more porous, and of a less firm texture. The probability is, that they belonged to a species of monkey which still continues to inhabit in considerable numbers those parts of the rock which are to us inaccessible.

This concretion varies in its composition according to the situation in which it is found. At the extremity of the Prince's lines, high in the rock which looks towards Spain, it is found to consist only of a reddish calcareous earth, and the bones of small birds cemented thereby. The rock around this spot is inhabited by a number of hawks, that in the breeding season nestle here and rear their young: the bones in this concretion are probably the remains of the food of those birds. At the base of the rock below King's lines the concretion consists of pebbles of the prevailing calcareous rock. In this concretion, at a very considerable depth under the surface, was found the under part of a glass bottle, uncommonly shaped, and of great thickness; the colour of the glass was of a dark green.

In many parts of the rock I have found concretions in which there are no bones of any kind; and on the elevated parts of the mountain, where the slopes are rapid, I have found a breccia (if I may so call it) entirely consisting of snail-shells combined in a mass of opaque flinty spar of a yellowish brown colour. The various progressive augmentations of this  
matter

matter were to be traced in various shades of the same colour, which, like the zones of the antique alabaster, curve round and follow the form of the shell. The purer matter of this spar has penetrated the shells, and in their interior hollows has formed a lining of small crystals, generally colourless, and perfectly transparent.

I have bestowed more time in endeavouring to describe the composition and the real situation in this concretion of bones than the subject, in the estimation of many, will seem to deserve, and, indeed, more than it deserves in my own opinion; but where an erroneous opinion has obtained a footing in consequence of inaccurate observations and partial description, it is the duty of every new observer to endeavour to correct it.

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## VI.

### *Accounts of the Discovery of Native Gold in Ireland.*

THE public attention was a few years ago greatly excited by the important mineralogical discovery of native gold in Ireland; soon after which accounts were sent to the Royal Society. (Phil. Transf. 1796.) It may easily be imagined, that these works must have been suspended by the convulsions which at present agitate that unhappy kingdom. But as I hope by enquiry to ascertain what has been done since the original discovery, which from its interest and curiosity is highly deserving of attention, I shall here present the substance of these accounts to my readers with very little abridgement.

John Lloyd, Esq. of Havodynos, gave the following account, dated November the 4th, 1795:

About seven miles westward of Arklow, in the county of Wicklow, there is a very high hill, perhaps six or seven hundred yards above the sea, called Croughan Kinshelly, one of whose N. E. abutments or buttresses is called Balinnagore, to which the ascent may be made in half or three quarters of an hour. In Jacob Nevill's map of the county of Wicklow, published in 1760, by casting your eye on the river Ovo, which runs by Arklow, at about four miles above the latter place, you will perceive the conflux of two considerable streams, and of a third about half a mile higher up, close to a bridge. By tracing this last to its source, you will come to a place set down in the map Ballinvally; this is a ravine between two others, that run down the side of the hill into a semi-circle, or more properly semi-elliptical valley, which extends in breadth from one summit to the other of the boundary of the valley, and across the valley three quarters of a mile or somewhat less. The hollow side of the hill forms the termination of the valley, and down which the three ravines abovementioned. At their junction the brook assumes the name of Ballinasloge: at this place the descent is not very rapid, and so continues a hanging level for about a quarter of a mile or somewhat more, when the valley grows narrower and the sides of the brook become steeper; and it should seem that some rocky bars across the course of the brook have formed the gravelly beds, above, over, and through which the stream flows, and in which the gold is found. The bed of the brook, and the adjacent banks of gravel on each side, for near a quarter of a mile in length, and for 20 or 30 yards in breadth, have been entirely stirred and washed by the peasants of the country, who amounted to many hundreds at work at a time whilst they were permitted to search for the metal.

A gentleman who saw them at work assured Mr. Lloyd, that he counted above three hundred women at one time, besides great numbers of men and children.

The stream runs down to the N. E. from the hill, which seems to consist of a mass of shistus and quartz; for, on examination of the principal ravine, which is now washed clean by the late heavy rains, the bottom consisted of shistus intersected at different distances and in various places by veins of quartz, and of which substances the gravelly beds at the bottom, where the gold is found, seem to consist.

Large tumblers of quartz are thickly scattered over the surface of the top of the hill, under a turbary of considerable thickness, upon the removal of which these tumblers appear.

The gold has been found in masses of all sizes, from those of small grains to that of a piece of the weight of five ounces; which beautiful specimen is intended for the cabinet of a nobleman adored in this country, and not less respected by his friends in England, and which I dare to say you will shortly have an opportunity of seeing in London. One piece of twenty-two ounces has been taken up, and which I am told is to be presented to his Majesty.

Mr. Graham of Ballycoage informed Mr. Lloyd, that about twenty-five years ago, or more, one Dunaghoo a schoolmaster, resident near the place, used frequently to entertain them with accounts of the richness of the valley in gold; and that this man used to go in the night and break of day to search for the treasure; and these gentlemen with their school-fellows used to watch the old man in his excursions to the hill, to frighten him, deeming him to be deranged in his intellects. However, the idea of this treasure did at last actually derange him.

Mr. Lloyd learned also from John Byrne, that about eleven or twelve years ago, when he was a boy, he was fishing in this brook, and found a piece of gold of a quarter of an ounce, which was sold in Dublin; but that, upon one of his brothers telling him it must have been dropped into the brook by accident, he gave over all thoughts of searching for more. Charles Toole, a miner at Cronbane, said, that he heard of this discovery at the time, but gave no credit to it, as he never found any gold, and lives very near the place; and Mr. Lloyd was also credibly informed, that a goldsmith in Dublin has every year for eleven or twelve years bought four or five ounces of gold brought constantly by the same person, but not John Byrne.

The name of the brook where the gold is found is in Irish Aughatinavought.

The account of the mineralogy and other circumstances by Abraham Mills, Esq. is as follows:

The workings, which the peasantry recently undertook, are on the north-east side of the mountain Croughan Kinshelly, within the barony of Arklow and county of Wicklow, on the lands of the Earl of Carysfort, wherein the Earl of Ormond claims a right to the minerals, in consequence of a grant in the reign of King Henry the Second by Prince John, during his command of his father's forces in Ireland; which grant was renewed and confirmed by Queen Elizabeth, and again by King Charles the Second\*.

The summit of the mountain is the boundary between the counties of Wicklow and Wexford; seven English miles west from Arklow, ten to the south-westward of Rathdrum, and six south-westerly from Cronbane mines; by estimation about six hundred yards above

\* It was afterwards found that this grant had been annulled by an act of the Irish Parliament near the beginning of the present century. Mr. Mills has since worked this mine on account of Government. N.



the level of the sea. It extends W. by N. and E. by S. and stretches away to the north-eastward to Ballycoage, where shafts have formerly been sunk, and some copper and magnetic iron ore has been found; and thence to the N. E. where extends a tract of mineral country eight miles in length, running through the lands of Ballymurtagh, Ballygahan, Tigrony, Cronebane, Connery, and Kilmacoe, in all which veins of copper ore are found; and terminating at the slate quarry at Balnabarny.

On the highest part of the mountain are bare rocks, being a variety of argillite\*, whose joints range N. N. E. and S. S. W. hade to the S. S. W. and in one part include a rib of quartz three inches wide, which follows the direction of the strata. Around the rocks for some distance, is found ground, covered with heath; descending to the eastward there is springy ground abounding with coarse grass; and below that a very extensive bog, in which the turf is from four to nine feet thick, and beneath it in the substratum of clay are many angular fragments of quartz, containing chlorite and ferruginous earth. Below the turbary the ground falls with a quick descent, and three ravines are observed. The central one, which is the most considerable, has been worn by torrents, which derive their source from the bog; the others are formed lower down the mountain by springs, which uniting with the former, below their junction the gold has been found. The smaller have not water sufficient to wash away the incumbent clay so as to lay bare the substratum; and their beds only contain gravel consisting of quartz, with chlorite and other substances of which the mountain consists. The great ravine presents a most interesting aspect; the water in its descent has in a very short distance from the bog entirely carried off the clay, and considerably worn down the substrata of rock, which it has laid open to inspection.

Descending along the bed of the great ravine, whose general course is to the eastward, a yellow argillaceous shistus is first seen; the laminæ are much shattered, are very thin, have a slight hade to the S. S. W. range E. S. E and W. N. W. Included within the shist is a vein of compact barren quartz, about three feet wide, ranging N. E. and S. W.; below this is another vein about nine inches wide, having the same range as the former, and hading to the northward, consisting of quartz including ferruginous earth. Lower down is a vein of a compact aggregate substance, apparently compounded of quartz, ochraceous earth, chert, minute particles of mica, and some little argillite of unknown breadth, ranging E. and W. hading fast to the southward, and including strings of quartz from one to two inches thick, the quartz containing ferruginous earth. The yellow argillaceous shistus is again seen with its former hade and range; and then adjacent to a quartz vein is laminated blue argillaceous shistus, ranging N. E. and S. W. and hading S. E.; which is afterwards seen varying its range and hade, running E. N. E. and W. S. W. and hading N. N. W. Lower down, the blue shist is observed more compact, though still laminated. The ground, less steep, becomes springy, is inclosed, and the ravine, shallower, has deposited a considerable quantity of clay-sand and gravel. Following the course of the ravine, or, as it may now more properly be called, the brook, you arrive at the road which leads to Arklow; here is a ford, and the brook has the Irish name of Aughatinavought (the river that drowned the old man); hence it descends to the Aughrim river, just above its confluence with that from Rathdrum, which after their junction take the general name of the

\* Kirwan, edit. 1794, p. 234.

Ovo, that, discharging itself into the sea near the town of Arklow, forms an harbour for vessels of small burthen.

The lands of Ballinvalley are to the southward, and the lands of Ballinagore to the northward of the ford, where the blue shistus rock, whose joints are nearly vertical, is seen ranging E. N. E. and W. S. W. including small strings of quartz which contain ferruginous earth. The same kind of earth is also seen in the quartz, contained in a vein from ten to twelve inches wide, ranging E. N. E. and W. S. W. and fading to the southward, which has been laid open in forming the Arklow road.

There the valley is from twenty to thirty yards in width, and is covered with substances washed down from the mountain, which on the sides have accumulated to the depth of about twelve feet. A thin stratum of vegetable soil lies uppermost; then clay mingled with fine sand composed of small particles of quartz, mica, and shist; beneath which the same substances are larger, and constitute a bed of gravel that also contains nodules of fine grained iron-stone, which produces 50 per cent. of crude iron; incumbent on the rock are large tumblers of quartz, a variety of argillite and shistus; many pieces of the quartz are perfectly pure; others are attached to the shistus; others contain chlorite, pyrites, mica, and ferruginous earth; and the arsenical cubical pyrites frequently occurs imbedded in the blue shistus. In this mass of matter, before the workings began, the brook had formed its channel down to the surface of the rock, and between six and seven feet wide, but in times of floods extended itself entirely over the valley.

Researches have been made for the gold amidst the sand and the gravel along the run of the brook for near half a mile in length; but it is only about one hundred and fifty yards above, and about two hundred yards below the ford, that the trials have been attended with much success: within that space the valley is tolerably level, and the banks of the brook have not more than five feet of sand and gravel above the rock: added to this, it takes a small turn to the southward, and consequently the rude surfaces of the shistus rock in some degree cross its course, and form natural impediments to the particles of gold being carried further down the stream, which still lower has a more rapid descent. Besides, the rude manner in which the country people worked seldom enabled them to penetrate to the rock in those places where the sand and gravel were of any material depth. Their method was to turn the course of the water wherever they deemed necessary, and then with any instruments they could procure to dig holes down to the rock, and, by washing in bowls and sieves the sand and gravel they threw out, to separate the particles of gold which it contained; and from the slovenly and hasty way in which their operations were performed, much gold most probably escaped their search: and that indeed actually appears to have been the case; for, since the late rains washed the clay and gravel which had been thrown up, gold has been found lying on the surface. The situation of the place, and the constant command of water, do however very clearly point out the great facility with which the gold might be separated from the trash, by adopting the mode of working practised at the best tin stream works in the county of Cornwall; that is, entirely to remove (by machinery) the whole cover off the rock, and then wash it in proper buddles and sieves. And by thus continuing the operations, constantly advancing in the ravine towards the mountain, as long as gold should be found, the vein that forms its matrix might probably be laid bare.

The discovery was made public, and the workings began early in the month of September last,

last (1795), and continued till the 18th of October, when a party of the Kildare militia arrived, and took possession by order of government; and the great concourse of people, who were busily engaged in endeavouring to procure a share of the treasure, immediately desisted from their labour, and peaceably retired.

Calculations have been made, that, during the foregoing period, gold to the amount of three thousand pounds Irish sterling was sold to various persons; the average price was three pounds fifteen shillings per ounce; hence eight hundred ounces appear to have been collected within the short space of six weeks.

The gold is of a bright yellow, perfectly malleable; the specific gravity of an apparently clean piece 19,000. A specimen assayed here by Mr. Weaver in the moist way produced from 24 grains,  $22\frac{53}{101}$  grains of pure gold, and  $1\frac{43}{101}$  of silver. Some of the gold is intimately blended with, and adherent to quartz; some (it is said) was found united to the fine-grained iron-stone, but the major part was entirely free from the matrix; every piece more or less rounded on the edges, of various weights, forms and sizes, from the most minute particle up to 2 oz. 17 dwt.; only two pieces are known to have been found of superior weight, and one of those is five and the other twenty-two ounces.

The bearings are all taken by the compass, without allowing for the variation.

William Moleworth, Esq. of Dublin, in a letter to Richard Moleworth, Esq. F. R. S. writes, that he weighed the largest piece of gold in his balance, both in air and water, and that its weight was 20 oz. 2 dwt. 21 gr. and its specific gravity to that of sterling gold, as 12 to 18. Also, that Richard Kirwan, Esq. F. R. S. found the specific gravity of another specimen to be as 13 to 18. Hence, as the gold was worth 4l. an ounce, Mr. William Moleworth concludes that the specimens are full of pores and cavities which increase their bulk, and that there are some extraneous substances, such as dirt or clay, contained in those cavities.

This opinion was discovered to be well founded, by cutting through some of the small lumps.

Stanley Alchorne, Esq. his Majesty's assay-master at the Tower of London, assayed two specimens of this native gold. The first appeared to contain in 24 carats,

$21\frac{6}{8}$  of fine gold.

$1\frac{7}{8}$  of fine silver.

$\frac{1}{8}$  of alloy, which seemed to be copper tinged with a little iron.

The second specimen differed only in holding  $21\frac{5}{8}$  instead of  $21\frac{6}{8}$  of fine gold.

Major John Brown, of the royal engineers, transmitted to the Right Hon. Thomas Pelham a sketch of the spot where the gold was found, which Mr. Pelham permitted to be engraved for the use of the Royal Society, and of which fig. 5. plate IX. is a copy, one third of the size of the original.

## VII.

*An Account of the Principles and Effect of Steam Engines, which act by means of a Piston. With Descriptions of the Atmospheric Engine of NEWCOMEN and CAWLEY; the Engine of WATT, which works in Vacuo; and a new Engine by Mr. SADLER, in which the direct Action of Steam and the Pressure of the Atmosphere are combined.*

SOME account of the original steam-engine of the Marquis of Worcester, and its subsequent improvements, has been given in a former paper in this work \*. One of the chief imperfections of that engine was then shewn to consist in the direct action of the steam, for forcing, being necessarily more than equal to the weight of the column of water required to be raised; in consequence of which, it became in most cases an indispensable condition, that the boiler and vessels should be very strong, as well as that a large quantity of fuel should be consumed, to produce steam sufficiently dense. It is probable that these inconveniences may have early directed the thoughts of various ingenious men to the application of a piston, though the difficulties of the undertaking seem to have retarded this pursuit for a considerable time. The first steam-engine, with a piston, made in 1707 by Papin †, was little calculated to remove these difficulties; and it is to Newcomen and Cawley that we are indebted for the application of a piston with machinery, by which the indirect action of steam little stronger than the atmosphere, or rather the direct action of the atmosphere, is made to act with safety and effect against the most severe pressures. It appears that they had brought their engine, about the year 1713 ‡, to a degree of perfection little different from those which are to be seen at present. A particular description of this engine, with drawings of its parts, and a considerable portion of the history of its invention, are to be met with in Desaguliers's Course of Experimental Philosophy, and it has also been described in many other books. For these reasons, I shall in this place give merely a verbal account of its principles and mode of operation.

Suppose a very large syringe to be set upright, and a piston or plug inserted at the upper end, the usual aperture being supposed to be at the lower extremity. If this last aperture be open, the piston will descend by its own weight, neglecting the effect of friction at its circumference. But let it be imagined, that the piston is supported by a counter-weight at the opposite extremity, by a lever or by any other means. In this case the piston will not descend unless more weight be added to it. Among the various ways of applying such a weight, there is one which consists in exhausting the air from the internal part of the cylinder beneath the piston. For, if this were done, it is evident that the whole pressure of the atmosphere, which amounts to about twelve pounds on every circular inch, will become active upon the upper surface. If the vacuum were to be produced by means of an air-pump, it may easily be allowed, that the labour of effecting it would be at least equal to that of any work which might be performed by the subsequent descent of the piston. We have seen that, in Savery's engine, the operation of steam is twofold; namely, by the direct pressure from its

\* Philosophical Journal, I. 419.

† Loco citato, p. 421.

‡ Desaguliers in his Lectures, ii. 467, says that it had been in use near thirty years at the time he wrote, and the imprimatur to his work bears date November 17, 1743. See also p. 532 of the same volume.

elasticity, and by the indirect consequence of its condensation, which affords a vacuum. This last is the only principle displayed in Newcomen's engine. In order to produce the vacuum at pleasure, it becomes requisite that various apertures should be formed at the bottom of the cylinder or syringe we have been speaking of:—one to communicate steam from a boiler; another to admit a jet of cold water, to condense that steam during the interval in which the communication from the boiler is cut off; a third provided with a pipe called the *eduction-pipe*, to carry off the condensed steam and injection water; and lastly, a small lateral aperture or valve through which the permanently elastic fluid, which cannot descend through the *eduction-pipe*, may be driven out. This last is called the *snifting clack*. By these provisions the operation is made to take place as follows: The piston being up, the steam-cock is opened, and steam issues from the boiler, which being less than half the weight of common air, rises to the top and expels the air through the *eduction-pipe*, of which the lower extremity is covered with a flap valve in a trough of water. When the noise of its escape is heard the steam-cock is shut, and the injection-pipe being opened throws a stream of cold water against the bottom of the piston. The steam becomes immediately condensed, and the pressure of the atmosphere forces the piston down into the vacuum. Upon its progress downwards the injection pipe is closed; and when it has arrived nearly to the bottom of the cylinder, the steam-cock is again opened. The elastic steam then not only fills the small space between the cylinder and the bottom, but its pressure assists the *eduction* water to pass off through its pipe, and drives the disengaged elastic fluid through the snift. In this state therefore the steam is somewhat stronger than the atmosphere, and counterpoises its action on the upper surface of the piston; whence the piston itself rises by the action of the counter-weight, and regains its original position at the top of the cylinder. A second repetition of the process, namely, of shutting off the steam and injecting cold water, causes it again to descend, and in this manner the alternations may be continued without limit.

As the pressure requisite to work an engine of this kind with speed, and to overcome the friction of its parts, may be estimated at about  $7\frac{1}{2}$  pounds for every round inch, the effect of a piston three feet in diameter, with ten strokes of six feet each per moment, will be to raise a weight of near half a ton at the rate of one foot per second. Such a power will therefore be very considerable.

It is to be understood, that the opening and shutting of the steam and injection cocks are performed by apparatus fixed to the working beam, in such a manner as to strike the levers of those cocks at the precise instants of time when their effects are required to be produced. The attendant has no other office to perform than keeping up the fire.

This curious and most eminently useful engine became an object of the researches of James Watt, then of Glasgow, but since associated with Matthew Boulton of Birmingham. It would be superfluous for me to enter into any general account of the science and intelligence of either of these engineers, or of the great advantages which society must ever derive when the spirit of enterprise is joined to the exertions of mental ability. Neither can I here attempt to detail the variety of new results which are exhibited in the specifications lodged by Watt in the Chancery offices concerning this object. My present limits and the attention which the world has already paid to these eminent men, and the products of their en-  
lightened

lightened activity, render both unnecessary. I shall therefore confine myself to remark, that the great features of improvement made in the engine of Newcomen by Watt are, first, that the elasticity of the steam itself is used as the active power in his engine; and secondly, that besides various other judicious arrangements for the economy of heat, he condenses the steam, not in the cylinder, but in a separate vessel.

In the great cylinder or syringe, concerning which we have spoken, in describing the engine of Newcomen, let us suppose the upper part to be closed, and the piston-rod to slide air-tight through a collar of leathers. In this situation, it is evident, that the piston might be depressed by throwing the steam upon its upper surface, through an aperture at the superior end of the cylinder. But if we suppose the external air to have access to the lower surface of the piston, we shall find, that steam no stronger in its elasticity than to equal the weight of the atmosphere would not move the piston at all; and consequently that this new engine would require much denser steam, and consume much more fuel than the old engine. The remedy for this evil is to maintain a constant vacuum beneath the piston. If such a vacuum were originally produced by steam, it is certain that its permanency could not be depended on, unless the engine contained a provision for constantly keeping it up. Mr. Watt's contrivance in his simplest engine is as follows: The steam is conveyed from the boiler to the upper part of the cylinder through a pipe, which also communicates occasionally with the lower part, and beyond that space with a vessel immersed in a trough of water; in which vessel the condensation is performed by an injected stream of cold water. This water is drawn off, not by an eduction-pipe but by a pump, of which the stroke is sufficiently capacious to leave room for the elastic fluid, separated during the injection, to follow and be carried out with the injection water. Suppose now the piston to be at its greatest elevation, and the communication from the boiler to the upper as well as to the lower parts of the cylinder to be opened. The steam will then pass into the whole internal part of the engine, and will drive the air downwards into the condenser, and thence through the valves of the air-pump. In this situation, if the communication from the boiler to the lower part of the cylinder be stopped, and an injection be made into the condenser, a vacuum will be produced in that vessel, and the steam contained in the lower part of the cylinder and communication pipe will expand itself with wonderful rapidity towards the condenser, so that in a period of time too minute to be appretiated, the whole of the steam beneath the piston will be practically condensed. The steam which continues to act above the piston will immediately depress it into the vacuum beneath; at the same time that by connection with the external apparatus the piston of the air-pump also descends in its barrel. When the stroke is nearly completed downwards, the requisite part of the apparatus shuts the communication with the boiler; opens that between the upper and lower parts of the cylinder and condensing vessel; and turns the injection-cock. At this very instant the piston loses its tendency to descend, because the steam presses equally on both surfaces, and continues its equality of pressure while the condensation is performed. It therefore rises; the injection is stopped; and the air-pump making its stroke suffers the injection water and a considerable part of the elastic fluid to pass through its lower valve. The vacuum is thus kept up through the whole internal capacity of the engine. As soon as the piston has reached the upper part of the cylinder, the communication to the under part of the cylinder is stopped, and that with

the boiler opened as before; the consequence of which is, that the piston again descends, and in this manner the alternations repeatedly take place.

The principal augmentation of power in this engine, compared with that of Newcomen, appears to arise from the cylinder not being cooled by the injection water, and its being practicable to use steam somewhat stronger than the pressure of the atmosphere. In general these engines are worked by steam, which would support a column of four or five inches of mercury besides the pressure of the atmosphere, and I have sometimes seen the gage as high as eight inches. But whether the comparative profit of the additional work was such as to repay the extraordinary consumption of fuel in such cases may be doubted; and indeed I suppose this practice is never adopted but when the work of a manufactory is such as to exceed the power or rate of the engine constructed for performing it.

The most ample information respecting these improved steam engines, which is to be met with in print, is found in the *Nouvelle Architecture Hydraulique* of Prony.

When we contemplate these engines together with the simpler engine of Savery, one of the most striking differences, is, that the latter is not embarrassed with the weight and expence of the beam and counterpoise. I believe that the fly was first introduced by Messrs. Watt and Boulton, as an equalizer of the action in steam engines, which in various constructions renders a counterpoise unnecessary, but is said to diminish the power by loading the work with friction and resistance. Of this however I can say nothing from my own experience or observation. The counterpoise is also unnecessary in the engines called double engines, in which the steam is made to act alternately on each side of the piston, by proper communication from the boiler, while the space into which the piston is to move is made to communicate with the condensing vessel, where a constant injection is maintained. I was also informed, in the year 1794, that some engines had been constructed without the beam; but the architect from whom I had the information did not state the particulars.

The injection water and elastic fluid are disposed of in Newcomen's engine with scarcely any loss of power, because the former flows spontaneously through the eduction-pipe, and the latter is driven out by a very slight effort of the steam against the atmosphere. Watt's engine could not have an eduction-pipe unless the column were at least 34 or 35 feet long, to exclude the atmosphere, and even in that case there could be no shift to deliver the elastic fluid. It was therefore necessary there should be an air-pump to carry off both; and this is a load upon the engine equal to the amount of the friction of these additional parts and the whole weight of the eduction water. I am not well informed of this last quantity; but suppose it to be about one eightieth \* part of the power of the engine.

The steam-engine for which a patent has lately been granted to Mr. Sadler, is worked without a lever or beam, and consequently has not the inertia of that heavy mass to overcome. The greatest part of its action is performed by the elasticity of steam; but the most remarkable character of his invention is, that part of the steam previous to its condensation is employed a second time in another cylinder, the piston of which is subject to the pressure of the atmosphere. By this second application it not only performs the office of an air-pump in a very accurate manner, but likewise adds to the total amount of force in the machine.

\* Supposing the injection water to be about  $\frac{1}{128}$  of the magnitude of the cylinder within, and that the stroke would raise  $\frac{1}{2}$  of the same magnitude of water through its own length.



Plate X. exhibits a projection of part of this engine, the boiler and the working parts being left out. R S U T represent a frame of wood. A is the larger cylinder, of which the piston-rod is kept vertical by a wheel I, which runs between parallel plates with a very small degree of shake. The rise and fall of this wheel moves the arm N O of a crank, which drives the fly P Q, and gives motion to the pumps or other working parts. B is the second cylinder open at top, the piston-rod of which is kept vertical in like manner by a wheel K. A connection is formed between the two pistons by the lever L M, which causes them to make their strokes together. D is a valve in the piston of A, which, by means of its tail beneath, is opened when that piston is down, and continues so during the whole ascent, until it becomes again closed by the pressure of its crown against the top of the cylinder.

Suppose the pistons to be near the tops of their respective cylinders, and the valve D to be open: if C be opened, the steam will pass through the cylinder A, and enter B by the passage E, driving the air before it through the valve G. If then the valve D be closed by raising the piston or otherwise, and an injection of cold water be made through F into B, the steam in that cylinder and beneath the piston of A will be condensed, and both pistons will descend; that in A by the action of the steam, and that in B by the pressure of the atmosphere. Near the termination of this descent the valve D will be opened, and the piston in A will be left at liberty to rise, in consequence of the equal action of the steam on both sides, at the same time that, the valve C being shut, there will be no farther supply from the boiler. The steam which occupied the space above the piston in A will expand through E, and assist the rise of the piston in B. If we overlook the effect of refrigeration in the passage E, and against the piston at B, the steam will begin to act beneath this piston with a force not greatly different from its last action upon that in A. It would not be easy to calculate the whole effect of the steam in its second application, not only for want of an accurate statement of the elasticity of this fluid during its expansion; but because the practical effect of the temperature of the second cylinder in diminishing that force is still more difficult to be appretiated. I shall therefore only observe, that the piston in B having descended so as to touch the surface of the injection water, and the elastic fluid resting upon it, not only causes that air but a portion of the hot water to pass through the valve W, which it carries up and discharges through a conveyance at the upper part of the cylinder to feed the boiler. The steam which follows it through E, drives all the elastic fluid through the valve of that passage, whence it remains in B till the next stroke carries it off. The quantity of water which shall rise above the piston through W, may be regulated by the height of the external surface of the cistern into which the pipe G discharges itself. The excess of the action of the steam beneath the piston in B, beyond the weight of that piston and the water it carries, will be an acquisition to the power of the engine. When we consider the facts, we may observe, that, if B were too small, the action of the steam under its piston would approach more nearly to equality with the last action in A; but that the stroke would be insufficient to carry off the elastic fluid, and the addition to the force of the engine inconsiderable; and on the other hand, if it were too large, the greatest part of the rising stroke would be performed against the pressure of the atmosphere, with very little assistance from the expanded steam. Whence it follows, that there is a certain definite proportion under like circumstances of pressure, temperature and work, between the two cylinders, which will produce a greater effect than any other. To determine this would be the object of computation, if the data were at hand; but it is probable



that an experimental result with regard to those dimensions might be obtained with as little trouble as the mere data for computation.

## VIII.

*On the supposed Improvement of Achromatic Lenses, when rendered more transparent by cementing them together.*

AN achromatic glass, consisting of one concave and two convex lenses, has six surfaces at which the reflexion of light can take place. Two evils of considerable magnitude are produced by this reflexion. The focal image becomes obscure from the loss of the light so diverted, and the ground or field of view is rendered misty by the irregular illumination afforded by part of this light, which enters the tube. These consequences are strikingly observed by holding an achromatic lens in such a manner as to throw the image of a window upon a sheet of paper, at the same time that a like image is afforded by a simple convex lens of the same focal length. The latter image will be bright and clear, while the former is very faint. As the reflexion of light is strongest at surfaces which confine mediums most different in density, it may naturally be imagined that the mischievous effect of the four surfaces, at which the lenses are applied to each other, might be in a great measure removed by the interposition of a denser fluid than air. \* Many years ago the Abbé Rochon made some experiments to improve achromatic lenses by this invention, and Mr. Grateloup afterwards made use of a transparent solid for the same purpose. He took a very pure tear of mastic, and interposed a piece of sufficient magnitude between the lenses of an achromatic glass. These being immersed under water of which the temperature was gradually raised, the mastic at length became soft, and by pressure of the glasses was made to occupy the whole of the two spaces between the concave and the exterior lenses. After this process the compound lens was beautifully transparent; the reflected images could scarcely be discerned; and upon trial in its tube the telescope was found to be greatly improved.

My attention to this contrivance was excited by the approbation bestowed upon a telescope of this kind, of 27 inches long and 23 lines aperture, by Citizen Lalande, in the *Connaissance de Temps* for the fourth year of the French Republic, page 364. It appeared to me, as it certainly will to the reader, to be a matter of no small surprise, that an improvement

\* Count Cassini, in the *Mémoires* of the Parisian Academy for 1787, p. 29, informs us, that a memoir *sur les moyens de perfectionner les lunettes achromatiques par l'interposition d'un fluide entre les objectifs*, by the Abbé Rochon, was read before that Academy in January 1774, and inserted in the *Recueil des Mémoires de Mécanique et de Physique*, which I have not seen; and in a note on this passage he quotes the Report of the Commissioners of the Academy, who express their satisfaction at the result of their trials for correcting great part of the defects arising from the surfaces of the glasses by the interposition of a fluid between them, but recommend trials with the telescopes of astronomers on celestial objects; which it should therefore seem had not then been made. The same author proceeds to acquaint us, that Mr. Grateloup used mastic in 1785, after which the Sieur Putois made excellent lenses of this kind; and that the mastic answered well with compound lenses, whose interior surfaces were only ground (*doncies*) but not polished. In this last experiment, however, it can hardly be imagined that the lens would have supported an high power; because the mastic, though it might afford transparency, must have left the confines of the mediums as imperfect as before.

of such value should never have been adopted in this kingdom. As it is no where said that lenses were constructed with a particular regard to the density and figure of the interposed mastic; but there seems reason to conclude, that in every case an achromatic lens of the usual construction was stated to have been rendered more perfect by this treatment,—I thought it best to overlook the theoretical objections, and confine myself in the first instance to experiment and enquiry. For this purpose I took an achromatic lens, consisting of two convex lenses and one concave, of which the radii need not here be detailed. When together they formed a compound lens of  $7\frac{3}{4}$  inches focus, with an aperture of nearly  $1\frac{1}{2}$  inch, which, upon a rough trial with a magnifying power of 30 times, exhibited scarcely any colour. Upon filling the interfices of this lens with water, the focal image became much brighter; but the focal distance was diminished to somewhat less than six inches, and much colour was produced with as low a magnifying power as ten times. It is evident, therefore, that the lens was greatly injured by the expedient made use of to prevent the loss by reflexion; and it may easily be inferred that a denser substance, such as mastic, would have shortened its focus and disturbed its compensations in a still greater degree.

Some time after making this experiment, I applied to the celebrated Mr. Ramsden, to whose unequalled ability and invention the arts of philosophical observation are so highly indebted. I did not expect to receive any information tending to shew that a combination of lenses, differing in curvature at the surfaces of apposition and achromatic in the air, could retain the same property when either partially or totally immersed in another transparent medium of different refractive or dispersive power. My principal object was, to know whether the French philosophers had constructed lenses of six parts duly adapted to each other; that is to say, considering the two portions of mastic as lenses, and not merely as a medium to prevent loss by reflexion. From him I learned that the supposed improvement had long since been communicated to him by some gentlemen of the Paris academy, who affirmed that an achromatic lens of Dollond's make had been much improved by the interposition of mastic between its parts, at the same time that they asserted in general terms that the same might be done with any other object glass of this kind. He put a good object glass into their hands for this purpose. The experiment was made, and the result proved similar to that which I have already related. The aberrations from figure as well as colour became very perceptible;—and in a word the lens, though much clearer, had lost its most valuable qualities.

Such is the result of a process which has been in the possession of our learned neighbours for about four-and-twenty years, and is still esteemed by them; but which has never been adopted by our opticians:—a process, the value of which, it should appear, might be ascertained in an instant by mere inspection through the telescope in question. We seem as if driven to the supposition that something might yet remain unexplained. Has the mischief of this process been overlooked by men so acute and enlightened?—Or has it happened that the interior curvatures of the lenses, on which they operated, were nearly parallel to each other; so that the addition of light (with the magnifying power commonly used with an index telescope) might be thought to offer an advantage, in angular observations, more than sufficient to compensate for the other defects?

## IX.

*Description of a very simple Apparatus for performing the great Experiment of producing Water by the Combustion of Hydrogen Gas. Invented by Mr. JOHN CUTHBERTSON<sup>\*</sup>.*

**A** D B C, fig. 6, plate IX., represents the instrument standing in a vessel, a b c d, nearly filled with water.

A D is a large glass bottle, holding about 1000 cubic inches of water, mounted at the top with a brass cap, which screws off at A. The rising part at the bottom is bored through and mounted with brass, in which screws a piece represented by a b, fig. 7, having a part at one end which fits into the hole, and the other end a shank with a screw at the bottom. E F, fig. 6, is a straight bar of brass; B C are two glass receivers, mounted at the top with brass, each having a shank which passes through the brass bar, and by means of a female screw is screwed fast to it: these mountings are perforated perpendicularly, and have also a side hole, which corresponds with a hole in the brass bar, passing from the shank of each receiver; which hole also corresponds with two holes represented in fig. 7, communicating with the inside of the large bottle. At L, M, in the brass bar, are two air cocks, with holes drilled through them in such a direction, that when the blades of the cocks stand lengthwise with the bar they correspond with the holes in it. F R and E N are two straight flat pieces of brass; in each of them is a hole, through which pass the ends of two screws, that are screwed fast into the sides of the vessel containing the water, and confine the bars by means of two female screws. O P is a long brass wire, with a piece of platina wire at the end P: this wire is fixed to the mounting at the top of the bottle, and is bended so that the end P stands as near as possible, but not to touch the small hole which passes through the piece a b, fig. 2.

When this instrument is to be used, the large glass must be first filled with oxygen gas (or common air when mere exhibition is intended): this may be done either by means of an air pump, or by filling it with water, inverting it and suffering the air to ascend. When either of these operations is to be performed, it must first be detached from the receivers by unscrewing the two female screws Q Q. All the other parts remain annexed to it, and care must be taken that the two air-cocks are shut. When it is filled with the air required, it must be set upon the receivers, the mountings of which are marked, as well as the bar, and care must be taken that they are not misplaced: the whole instrument must then be placed in the water vessel, as is represented. The receiver B has a hole about the middle at t: This receiver must be filled with oxygen gas, and C with hydrogen gas. When the airs or gases are to be lighted, electric sparks must be made to pass from the end of the platina to the hole in a continual succession; and while these sparks are passing, the cock L must be opened by degrees till the air is seen to take fire; the electric sparks must then cease, and the size of the flame may be regulated by turning the cock either one way or the other. The other-cock must then be set either quite open, or partly so, accordingly as you find the flame affected by it. As you find the gases decrease in the receivers, fill them by certain known measures, and continue the operation †.

## SCIENTIFIC

\* Communicated by George Pearson, M.D. F.R.S. &c.

† The above description was drawn up by Mr. Cuthbertson. I find, upon examination of the apparatus at Dr. Pearson's house, that the hole through which the hydrogen is introduced is made very small, but that the other hole

## SCIENTIFIC NEWS.

*Account of the Public Sitting of the National Institute of Sciences and Arts, held at Paris the 15th Messidor, in the Year VI. (July 3, 1798.)*

**C**ITIZEN Bitaubé took the chair at half past five o'clock, the citizens Villars and Andrieux performing the office of secretaries. Citizen Villars ascended the tribune, and gave an account of the operations of the class during the last trimestre, of which the following is an abstract :

A memoir of Cit. Camus on the typographic art, polytipage, and the art of composing geographical charts, such as have been executed by the celebrated Haas at Basle.

A memoir of Cit. Langles on the Arabian poets who preceded Mahomet. This young and laborious student shewed that, even at a period so remote, the greater part of these writings possessed considerable poetical merit.

Cit. Monges read a memoir on the ruins of Persepolis, in which he proves that the destruction by Alexander was not total, but that it subsisted long afterwards.

Cit. Papon, associate correspondent of the class of moral and political sciences, read to the class of literature a memoir on the advantages which may be obtained from the study of Greek and Roman inscriptions, in order to ascertain a variety of historical facts.

Cit. Lefevre-Gineau gave a short account of the object of the various mathematical memoirs which had been read during the last trimestre.

A memoir of Cit. Lamarck concerning the action of the moon upon the atmosphere. It has been observed, that when this satellite is to the north of the equator, the winds blow, for the most part, from the north ; and that they pass to the west or south-west, which in these climates are attended with rain, when the moon is to the south of the equator.

Cit. Flangaques, associate correspondent at Viviers, transmitted to the class a memoir on the refraction of light.

Cit. Lebossut communicated a memoir on the integral calculus, which, besides the perspicuous disposition of its parts, exhibits some original methods, invented by the author.

A memoir of the laborious and indefatigable Messier, on the comet of the 23d Germinal, (April 12). The orbit of this comet, which, after having been seen for 43 days, disappeared in the constellation Ursa Major, has been calculated according to the method of Laplace. Fifty comets have already been observed by Messier, of which 21 were discovered by him.

Cit. Lassus, who was appointed to give an account of the memoirs on natural philosophy and chemistry, mentioned a memoir of Cit. Guyton on the affinities and the decomposition of salts at a temperature beneath the freezing point.

through which the oxygen passes is of considerable magnitude. I conclude that these dimensions of the apertures were ascertained by experiment, as the best suited to the complete though slow combustion of the hydrogen. As the bulk of hydrogen gas required to be introduced, for perfect combustion, is more than twice that of the oxygen, and the friction through the small aperture is very great, it becomes necessary that the pressure to extrude the former should be the strongest. The hole renders it impossible to extrude the oxygen by the reaction of any longer column of water, than from that hole to the surface of the water in the tub ; but the pressure which can be exerted in the vessel B is nearly twice as much. N.

Cit.

Cit. Clouet, associate correspondent at Ouzour, near Gien, transmitted a memoir to the class, describing a method of converting iron into steel without cementation. He makes use of silex, alumine, &c.

Cit. Baumé, likewise associate correspondent, read to the class a memoir on the material of which artillery is made, and the cause of the degree of heat they acquire after a few discharges. He offers a new composition less susceptible of becoming heated.

Cit. Gibert treated of fleece-bearing animals, or sheep, and the advantages which commerce may derive from their produce. Lamarck gave an account of the cuttle fish; and Cit. Cuvier, of the internal disposition of the throat of birds, by means of which they utter sounds more or less acute. Cit. Defessarts was the author of an excellent memoir on the small-pox, with a short account of Galvanism.

The class of mathematical and physical sciences had proposed as the subject of a prize, to be determined on the 15th Vendemiaire, in the year VII, (October 6, 1798) for which the productions of candidates were to be received until the first of Germinal last, (March 21, 1798) the following question :

What are the uses of the liver in the various classes of animals?

One memoir only has been received, which does not apply to the question. The class has therefore thought proper to divide it, in order to facilitate the solution. In this form it becomes the object of two prizes. The question for the first prize is,

To ascertain the form, situation, magnitude, comparative weight, texture of the vessels, communications, and appendices to the liver, considered in the principal classes of animals from man to insects, molluscæ, and worms.

The object of the second prize is,

The analysis of the hepatic or cystic bile in the several classes of animals here mentioned.

Memoirs will be received till the first Nivose, in the year VIII, (December 21, 1799) and the prize will be ascertained on the 15th of Germinal following (April 4). The prize for each question will be a medal of gold weighing one kilogramme (22966 grains, or 47 oz. 17 dwts. troy). The Institute has published a program, exhibiting the conditions to which candidates must conform.

A prize was decreed. It was divided between two different works; but in effect it proved but one, because the same person was entitled to both. In the year IV the class of mathematical and physical sciences proposed the following object :

To construct a pocket watch proper to determine the longitude at sea, by observing such divisions as indicate the decimal parts of the day, namely, tenths, thousandths, and ten thousandths; or the system in which the day shall be divided into ten hours, the hour into one hundred minutes, and the minute into one hundred seconds.

The Institute divided the prize between two watches, No. 1. bearing the epigraph "Ma liberté fait ma constance;" and No. 2. having the inscription "Au tems qui instruit."

Cit. Louis Berthout constructed both watches. His name was proclaimed amidst the applause of the assistants.

The class of moral and political sciences proposed the two following questions, one relative to the social science, and the other to geography :

What ought to be the extent and the limits of power in the father of a family in a well-constituted republic?

The reception of memoirs will terminate on the 15th Messidor, in the year VII, (July 3, 1799) and the prize will be distributed the 15th Vendemiaire, in the year VIII (October 6, 1799).

To determine the great changes which have taken place in the globe of the earth, and are either indicated or proved by history.

The concurrence will cease on the 5th Vendemiaire, in the year VIII, (October 6, 1799) and the prize will be distributed on the 15th Nivose following (January 4, 1800).

The same class had two prizes to distribute at this sitting. One on the question, 1. For what objects, and on what conditions, is it proper that a republican state should open a public loan? The other on this question: 2. What are the institutions most proper to establish the morals of a people?

The memoirs transmitted not having complied with the condition of the program, the Institute proposes again the question respecting loans for the year VII. The concurrence will close on the 7th Messidor (June 25, 1799), the prize will be given on the 15th Vendemiaire following (October 6, 1799); and the class of moral and political sciences, at its next public sitting, will give a new program for the development of the question respecting institutions. Among the memoirs which have been received, three are noticed as worthy of distinction, respectively bearing the epigraphs, 1. *Si forte necesse est . . . fingere . . . non exaudita . . . contingent; dabiturque licentia sumpta prudenter.* 2. *Nec enim ulla res vehementius rempublicam continet, quam fides.* 3. *On n'honore pas la vertu; on la respecte.* The prize proposed for these four questions will be a gold medal of the weight of five hectogrammes (11483 grains, or 23 oz. 18 dwts. troy).

Cit. Lacuée, secretary, in a short account of the labours of his class, spoke of two memoirs of Cit. Delisle-Defalles, and another of Cit. Rœderer, all three concerning the government of China. It cannot be dissembled, that it will always be very difficult in France to acquire a perfect knowledge of the government of a nation, of which the laws cannot be studied in the country itself, or at least in the writings of that people. Depending on the report of other writers, who themselves may not have been perfectly informed, the two members of the Institute have maintained different opinions, and each refers to his authorities. Cit. Defalles affirms that this government is tyrannical, and brings facts in proof of this assertion. Cit. Rœderer, from the stability of the government, deduces a proof that its basis is republican. He establishes, with much acuteness, a difference between the patriarchal government, in which a single chief influences the whole family, and the paternal government, in which the authority is divided into as many branches as there are married individuals. He likewise treats of the system of the Chinese writing.

Cit. Villetreque, associate correspondent, read a memoir somewhat abstracted on the difference between experiment and reasoning in philosophy.

Cit. Gosselin was mentioned as the author of another memoir, in which he has developed the geographical system of Polybius. It is known that this expression denotes the ideas which that historian entertained respecting the mathematical divisions of the surface of the globe, and the situations of places. The same skilful geographer has before proved that Eratosthenes, Strabo, and Ptolemy, had very incorrect notions of this science.

Certain

Certain memoirs of Cit. Bouchad, on the legislation of the Romans, and their numismatic history, were mentioned; and after these accounts the president successively announced the readings.

Cit. Lefevre-Gineau read for Cit. David Leroy a memoir on the ships of the ancients, in which he proves that we may derive useful instruction from them. Cit. Delambre read a memoir on the measure of a base taken in the neighbourhood of Melun, for determining the length of the meridian.

Cit. Fleurieu read a memoir on the civilization of the north-west parts of America, where the traces are found of acquisitions which do not belong to nations absolutely savage; such as ingenious constructions, sculptures and indifferent paintings representing animals, the parts of the human body, &c. He concludes that their ancestors fled to these countries from the ravages of the Spaniards during the conquest of Mexico.

Cit. Chaptal read a memoir on the yellow colour obtained from vegetables.

Cit. Monges discussed the question, whether the two antique statues known by the names of the gladiator and the dying gladiator do really represent gladiators. He clearly establishes the difference which existed between the gladiators of the Romans and the athletics of the Greeks. The first were barbarians, whose lives were mercilessly exposed; the second, Greeks, and sometimes heroes, who distinguished themselves by their ability in personal activity. The author does not believe that the statues in question are entitled to the denominations they bear.

Cit. Colin d'Harleville himself read a dialogue, entitled, "Man and his conscience," a very well written philosophical production. Cit. Lalande read a piece on the variation of the magnetic pole.

The sitting was terminated by Cit. Molé, who delivered a very happy tale written by Cit. Andrieux, entitled, The Dean of Badajoz.

THE public attention has lately been much excited by a project of Mr. R. Dodd, engineer, for a communication between the counties of Kent and Essex, by a tunnel or subterraneous road beneath the Thames, from Gravesend to Tilbury. The particulars as they appear in the newspapers, inserted as I apprehend by the engineer himself, are as follow:

The tunnel is to be cylindrical, and lined on all sides with stone keyed together in the manner of an arch; the internal diameter to be sixteen feet, which Mr. Dodd imagines will be sufficient for foot, horse, and carriage passengers. The passage must of course be illuminated with lamps, and a steam-engine is proposed to be erected in a proper situation to draw off the drainage water if any should accumulate. The estimate is stated as follows:

To 900 yards (running measure) of tunneling, including excavations, vaulting	
with key-stones, &c. at 12l. per yard	£. 10,800
To relaying the bottom with new made ground 900 yards at 1l. each	900
To placing lamps and lamp irons through the tunnel, collectors' rooms, and gates	
at each end	400
To making good the entrance roads at each end of the tunnel	160
To a steam-engine to draw off drainage water	1,780
Necessary machinery during the execution	500
To ten per cent. upon the whole for contingencies	1,415
9	Total
	£. 15,955

I do not find, from the enquiries I have yet been able to make, that any effective steps have been taken towards carrying this plan into execution. It may, as Mr. D. observes, be done either at the expence of the two counties, or by a private sharing company under an act of Parliament of the same tenor as is usually obtained for canals. But whatever may be the mode, there can be no doubt of the very great commercial advantages of such a communication. It would demand a greater degree of attention and research into the circumstances of local situation; such as the depth and position of the main channel, the elevations of the banks, the nature of the ground, with the prices of materials and workmanship, than on the present occasion can be thought of, to form any judgment of the estimate or the difficulties which may attend this important work. To the great public, Mr. Dodd's proposal will appear interesting in a more general point of view. When it is considered that the practice of making tunnels has now been known for a considerable number of years in this kingdom; that in many, if not most instances, a tunnel will be much cheaper, and full as safe, durable and convenient as a bridge; that the tunnel may be made (at least in favourable circumstances of the ground) in situations where a bridge would be impracticable, and will leave that most valuable desideratum the clear navigation above unimpeded:—this happy thought of Mr. Dodd may be allowed to promise beneficial consequences of the greatest magnitude to the community.

Dr. HERSCHEL has discovered four additional satellites of the Georgium Sidus. His paper on this object is inserted in the late publication of the Royal Society \*. By recurring to his former communication on this subject †, the two old satellites are found to revolve, the first in 8 days 17 h. 1 m. 17 sec. at the distance of 33'' from its primary, and the second in 13 d. 11 h. 5 m. 1,5 sec. at the distance of 44'', 23. The planes of their orbits form such large angles with that of the planet itself, and consequently to the ecliptic, as to be almost perpendicular to it. To this remarkable departure from the analogy of the old planets another still more singular is now announced. *They move in a retrograde direction!* The new satellites revolve as follows, the periodical times being inferred from their greatest elongations: The interior satellite in 5 d. 21 h. 25 m. at the distance of 25'', 5.—A satellite intermediate between the two old ones in 10 d. 23 h. 4 m. at the distance of 38'', 57.—The nearest exterior satellite at about double the distance of the farthest old one, and consequently its periodical time 38 d. 1 h. 49 m.—And the most distant satellite full four times as far from its primary as the old second satellite. Whence it will take at least 107 d. 16 h. 40 m. to complete its revolution. Whether the motions of these four be direct or retrograde, is, I suppose, not yet determined.

From some observations of the Doctor, with an excellent seven foot telescope, certain appearances resembling that of two rings surrounding the planet, and crossing each other at right angles, were seen on several different days. They were not altered in position by turning the speculum in its cell; but there is little doubt that they were optical deceptions, because they kept their position with respect to the tube, after the relative position of the parallel had been much changed by the earth's rotation, and because they did not appear with larger telescopes applied during the course of ten years. The disk of the Georgium Sidus is flattened. It therefore revolves with considerable rapidity on its axis. From the very faint light of the satellites, they are observed to disappear in those parts of their orbits which bring them apparently nearest the planet. This does not arise from an atmosphere; for the effect is the same, whether the satellite be within or beyond the planet.

\* Phil. Transf. 1798, p. 47.

† Ibid. 1788.



# The Economical Laboratory of Gungton.

Philos. Journal Vol. II. Pl. LX (aving) p. 210.

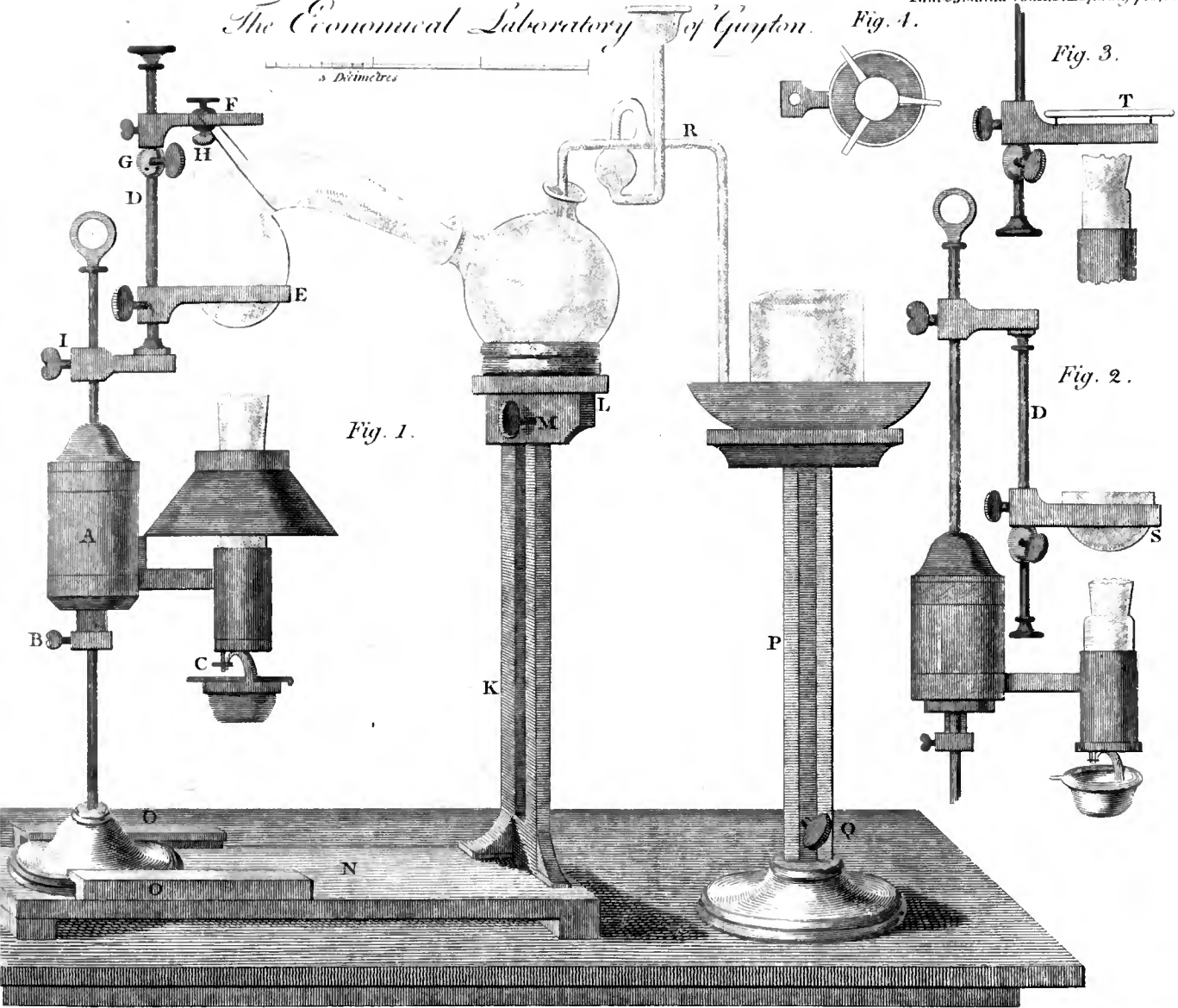


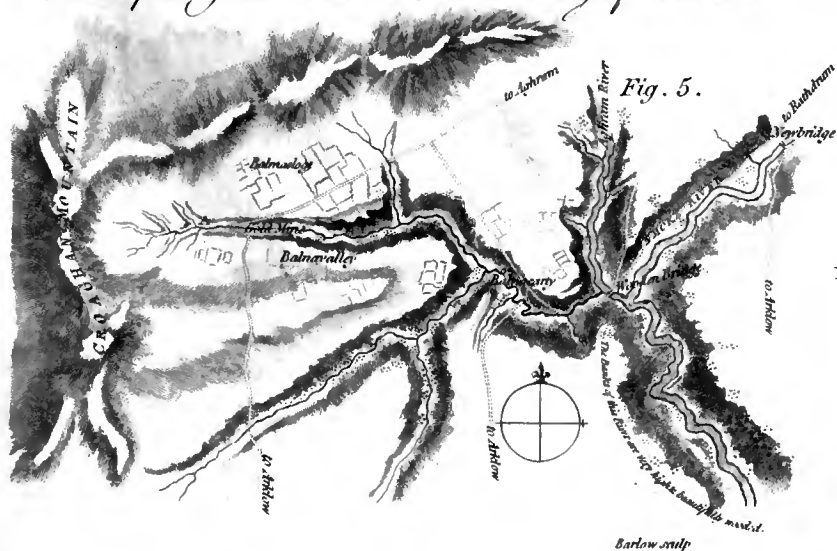
Fig. 1.

Fig. 4.

Fig. 3.

Fig. 2.

## Sketch of the Gold Mine in the County of Wicklow.



Barlow's map

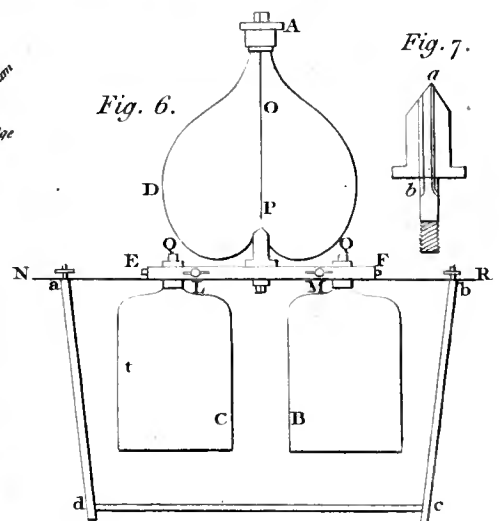
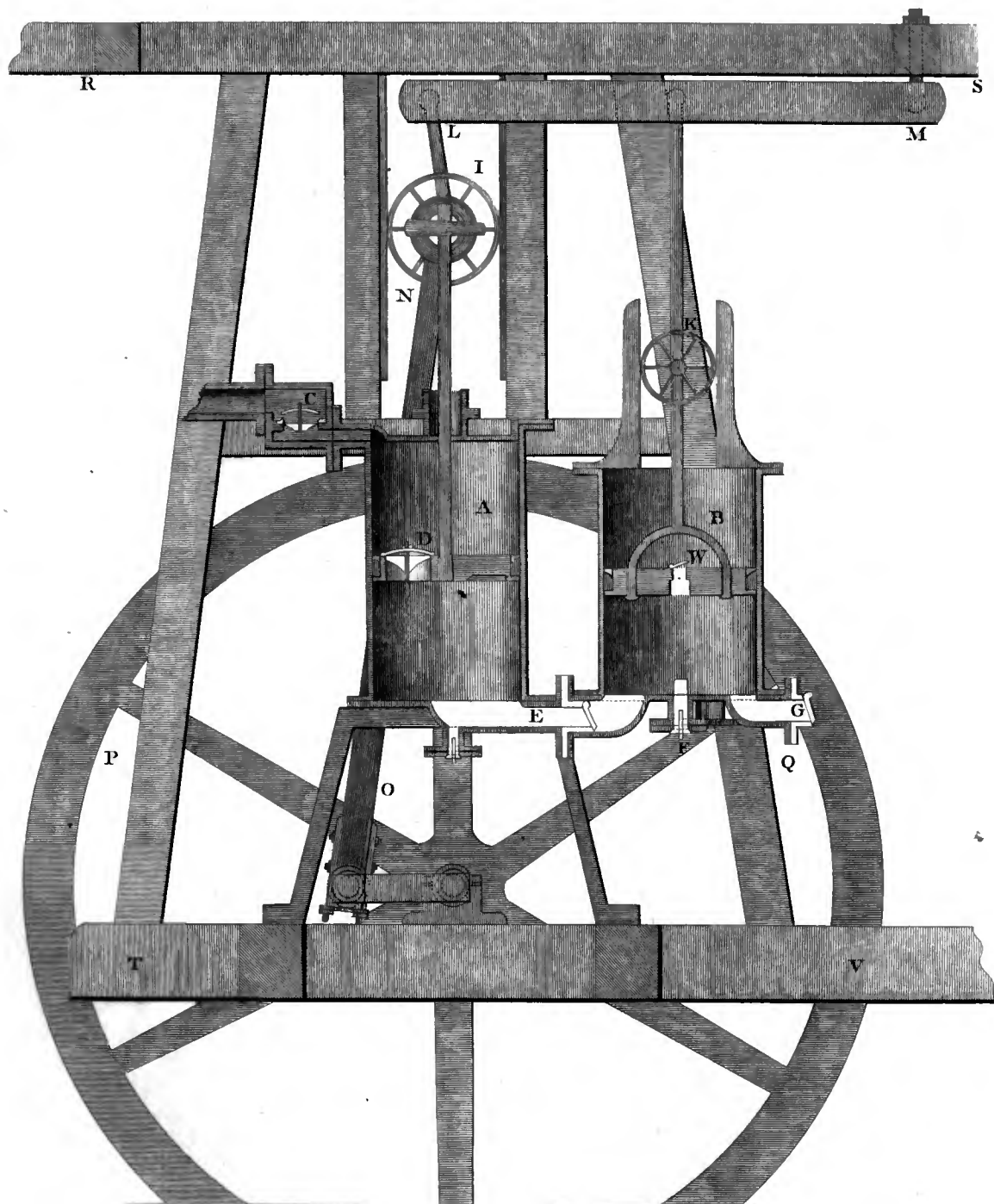


Fig. 6.

Fig. 7.



*Steam Engine by J. Sadler Esq.*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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SEPTEMBER 1798.

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ARTICLE I.

*Experiments on carbonated Hydrogenous Gas; with a View to determine whether Carbon be a simple or a compound Substance\*. By Mr. WILLIAM HENRY.*

THE progress of chemical science depends not only on the acquisition of new facts, but on the accurate establishment, and just valuation, of those we already possess: for its general principles will otherwise be liable to frequent subversions; and the mutability of its doctrines will but ill accord with the unvaried order of nature. Impressed with this conviction, I have been induced to examine a late attempt to withdraw from its rank among the elementary bodies, one of the most interesting objects of chemistry. The inferences respecting the composition of charcoal, deduced by Dr. Austin from his experiments on the heavy inflammable air †, lead to changes so numerous in our explanations of natural phenomena, that they ought not to be admitted without the strictest scrutiny of the reasoning of this philosopher, and an attentive repetition of the experiments themselves. In the former, sources of fallacy may, I think, be easily detected; and in the latter there is reason to suspect that Dr. Austin has been misled by inattention to some collateral circumstances. Several chemists, however, of distinguished rank have expressed themselves satisfied with the evidence thus produced in favour of the composition of charcoal; and amongst these it may be sufficient to mention Dr. Beddoes, who has availed himself of the theory of Dr. Austin in explaining some appearances that attend the conversion of cast into malleable iron ‡.

The heavy inflammable air, having been proved to consist of a solution of pure charcoal

\* From the Phil. Transf. 1797.

† Phil. Transf. vol. lxxx. p. 51.

‡ Phil. Transf. vol. lxxxi.

in light inflammable air, is termed in the new nomenclature, carbonated hydrogenous gas. By repeatedly passing the electric shock through a small quantity of this gas, confined in a bent tube over mercury, Dr. Austin found that it was permanently dilated to more than twice its original volume. An expansion so remarkable could not, as he observes, be occasioned by any other known cause than the evolution of light inflammable air.

When the electrified air was fired with oxygenous gas, it was found that more oxygen was required for its saturation than before the action of the electric fluid; which proves that by this process an actual addition was made of combustible matter.

The light inflammable air disengaged by the electrization proceeded without doubt from the decomposition of some substance within the influence of the electric fluid, and not merely from the expansion of that contained in the carbonated hydrogenous gas: for, had the quantity of hydrogen remained unaltered, and its state of dilatation only been changed, there would not, after electrization, have been any increased consumption of oxygen.

The only substances in contact with the glass tube and mercury, in these experiments, besides the hydrogen of the dense inflammable gas, were carbon and water; which last, though probably not a constituent of gases, is however copiously diffused through them. If the evolved hydrogen proceeded from the decomposition of the former of these two substances, it is evident that a certain volume of the carbonated hydrogenous gas must yield, after electrization, on combustion with oxygen, less carbonic acid than an equal volume of non-electrified gas; or, in other words, the inflammation of 20 measures of carbonated hydrogen expanded by electricity from 10, should not afford so much carbonic acid as 10 measures of the unelectrified.

From the fact which has been before stated, respecting the increased consumption of oxygen by the electrified air, it follows, that in determining the quantity of its carbon by combustion, such an addition of oxygen should be made, to that necessary for the saturation of the gas before exposure to the electric shock, as will completely saturate the evolved hydrogen. For, if this caution be not observed, we may reasonably suspect that the product of carbonic acid is diminished, only because a part of the heavy inflammable air has escaped combustion. It might indeed be supposed, that in consequence of the superior affinity of carbon for oxygen, the whole of the former substance contained in the dense inflammable gas would be saturated and changed into carbonic acid, before the attraction of hydrogen for oxygen could operate in the production of water. But I have found that the residue, after inflaming the carbonated hydrogenous gas with a deficiency of oxygen, and removing the carbonic acid, is not simply hydrogenous, but carbonated hydrogenous gas.

In the 2d, 5th and 6th of Dr. Austin's experiments, in which the quantity of carbon in the electrified gas was examined by deflagrating it with oxygen, the combustion was incomplete because a sufficiency of oxygen was not employed; and Dr. Austin himself was aware that in each of them "a small quantity of heavy inflammable air might escape unaltered." It is observable also, that the product of carbonic acid from the electrified gas increased in proportion as the combustion was more perfect. We may infer, therefore, that if it had been complete there would have been no deficiency of this acid gas, and consequently no indication of a decomposition of charcoal. A strong objection, however, is applicable to these as well as to most of Dr. Austin's experiments, that the residues were not examined with sufficient attention. In one instance, we are told, that the remaining gas was inflammable,

mable, and in another that it supported combustion like vital air. I need hardly remark, that a satisfactory analysis cannot be attained of any substance, without the most scrupulous regard not only to the qualities, but to the precise quantities of the products of our operations.

To the 8th and 9th experiments the objection may be urged with additional weight, which has been brought against the preceding ones, that the quantity of oxygen, instead of being duly increased in the combustion of the electrified gas, was on the contrary diminished. Thus, in the 8th experiment 2,83 measures of carbonated hydrogen were inflamed with 4,58 measures of oxygenous gas; but in the 9th, though the 2,83 measures were dilated to 5,16, and had therefore received a considerable addition of combustible matter, the oxygen employed was only 4,09. To the rest of Dr. Austin's experiments, either one or both of the above objections are applicable.

The first and most important step, therefore, in the repetition of these experiments, is to determine whether the carbonated hydrogenous gas really sustains by the process of electrization a diminution of its quantity of carbon; because, should this be decided in the negative, we derive from the fact a very useful direction in ascertaining the true source of the evolved hydrogen. The following experiments were therefore made with a view to decide this question, and the error of Dr. Austin in employing too little oxygen was carefully avoided\*.

*Experiment 1.* In a bent tube standing inverted over mercury, 94,5 measures of carbonated hydrogenous gas from acetite of pot-ash were mixed with 107,5 of oxygen. The total, 202, was reduced by an explosion to 128,5, and was further contracted by lime water to 54. A solution of hepar sulphuris left only 23 measures.

The diminution by lime water, viz. 74,5 measures, makes known to us the quantity of carbonic acid afforded by the combustion of 94,5 measures of carbonated hydrogenous gas. And the residue after the action of hepar sulphuris, viz. 23 measures, gives the proportion of azotic gas contained in the carbonated hydrogen; for the oxygenous gas employed, which was procured from oxygenated muriate of pot-ash, was so pure, that the small quantity used in this experiment could not contain a measurable portion of azotic gas.

*Experiment 2.* The same quantity of carbonated hydrogen was expanded by repeated electrical shocks to 188 measures. The addition of hydrogenous gas therefore amounted to 93,5. The gas thus dilated was fired at different times with 392,5 measures of oxygenous gas; and the residue after these several explosions was 203 measures. Lime water reduced it to 128,5, and sulphure of pot-ash to 19,5. In this instance, as in the former one, the product of carbonic acid is 74,5 measures.

\* The apparatus employed in these experiments was the ingenious contrivance of Mr. Cavendish, and is described in the lxxvth vol. of the Philosophical Transactions. In dilating the gas, I sometimes used a straight tube, furnished with a conductor in the manner of Dr. Priestley (see his Experiments on Air, vol. i. plate 1. fig. 16.) The bulk of the gases introduced, and their volume after the various experiments, were ascertained by a moveable scale, and by afterwards weighing the mercury which filled the tube to the marks on the scale; by which means I was spared the trouble of graduating the syphons. Each grain of mercury indicates one measure of gas; and though the smallness of the quantities submitted to experiment may be objected to, yet this advantage was gained, that the electrified gas could be fired at one explosion, as was done in the 4th, 6th and 8th experiments. Errors from variations of temperature and atmospheric pressure were carefully avoided.

Finding from the first experiment and other similar ones, that the carbonated hydrogenous gas which was the subject of them contained a very large mixture of azotic gas, I again submitted to distillation a quantity of the acetite of pot-ash, with every precaution to prevent the adulteration of the product with atmospherical air. Such an adulteration, I have observed, impedes considerably the dilatation of the gas, and for a time even entirely prevents it. This explains the failure, which some experienced chemists have met with in their attempts to expand the carbonated hydrogenous gas by electricity. Gas which is thus vitiated becomes, however, capable of expansion after exposure to the sulphure of pot-ash.

*Experiment 3.* Carbonated hydrogen 340 measures were exploded with the proper proportion of oxygenous gas. The carbonic acid produced amounted to 380 measures, and the residue of azotic gas was 20 measures.

*Experiment 4.* The same quantity, when expanded to 690, gave on combustion 380 measures of carbonic acid, and 19,8 of azotic gas.

*Experiment 5.* Three hundred and fifteen measures of carbonated hydrogen yielded 359 measures of carbonic acid, and 18,5 measures of azote.

*Experiment 6.* The same quantity, after expansion to 600, afforded the same products of carbonic acid and azotic gases.

*Experiments 7 and 8.* As much carbonic acid was obtained by the combustion of 408 measures of carbonated hydrogenous gas, expanded from 200, as from 200 measures of the non-electric fired gas; and the residue of azotic gas was the same in both cases.

It is unnecessary to state the particulars of several other experiments similar to those above related, which were attended with the same results. They sufficiently prove that the action of the electric spark, when passed through carbonated hydrogenous gas, is not exerted in the decomposition of carbon; for the same quantity of this substance is found after as before electrization. Even granting that charcoal is a compound, the constituents of which are held together by a very forcible affinity, it does not appear likely that the agency of the electric shock, which seems in this instance analogous to that of caloric, should effect its decomposition under the circumstances of these experiments. For it is a known property of charcoal to decompose water, when aided by a high temperature; and its union with oxygen is a much more probable event when this body is present, than a separation into its constituent principles. As an argument also that water is the source of the light inflammable air in this process, it may be observed that the dilatation in Dr. Austin's experiments could never be carried much farther than twice the original bulk of the gas\*. This fact evidently implies that the expansion ceased only in consequence of the entire destruction of the matter whose decomposition afforded the light inflammable air; and this substance could not be carbon, because Dr. Austin admits that a large portion, and I have shewn that the whole of it, still remains unaltered.

If the dilatation of the carbonated hydrogenous gas arose from the decomposition of water, the effect should cease when this fluid is previously abstracted. To ascertain whether

\* "After the inflammable air has been expanded to about double its original bulk," says Dr. Austin, "I do not find that it increases further by continuing the shocks. Conceiving that the progress of the decomposition was impeded by the mixture of the other airs with the heavy inflammable, I passed the spark through a mixture of the heavy inflammable air and light inflammable; but the expansion succeeded nearly as well as when the heavy inflammable was electrified alone." Phil. Trans. vol. lxxx. p. 52.



this consequence would really follow, I exposed a portion of the gas for several days before electrization to dry caustic alkali. On attempting its expansion, I found that it could not be carried beyond one sixth the original bulk of the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80 more produced not the least effect; though the former number would have been amply sufficient to have dilated the gas in its ordinary state to more than twice its original volume. A drop or two of water being admitted to this portion of gas, the expansion went on as usual; and I may here observe, that when a little water gained admission into the tube along with the gas, in any experiment, which often happened before I had acquired sufficient expertness in transferring the air from water to mercury, the dilatation went on with remarkable rapidity.

Carbonic acid gas, according to the discovery of M. Monge \*, undergoes, when submitted to the electric shock, a change similar to that effected on the carbonated hydrogen; and the expansion has been shewn by Messieurs Landriani, and Van Marum †, to be owing to the same cause, viz. the extrication of light inflammable air. The added gas, M. Monge ably contends, cannot proceed from any other source than the water held in solution by all aeriform bodies, the oxygen of which he supposes to combine with the mercury. That the decomponent of the water, however, in the experiments which I have described, is not a metallic body, will appear highly probable, when we reflect that there is present in them a combustible substance, viz. charcoal, which attracts oxygen much more strongly than metals; and the following experiments evince, that the mercury by which the air was confined had no share in producing the phenomena.

*Experiment 9.* A portion of carbonated hydrogenous gas was introduced into a glass tube closed at one end, into which a piece of gold wire was inserted, that projected both within and without the cavity of the tube. The open end of the tube was then closed by a stopper perforated also with gold wire, so that electric shocks could be passed through the confined air without the contact of any metal that has the power of decomposing water. On opening the tube with its mouth downwards under water, a quantity of air immediately rushed out.

*Experiment 10.* The dilatation of the gas was found to proceed very rapidly when standing over water, and exposed to the action of the electric fluid, conveyed by gold conductors.

We have only, therefore, in the two preceding experiments, one substance in contact with the gas which is capable of decomposing water, viz. charcoal. The union of this body with the oxygen of the water would be rendered palpable by the formation of carbonic acid; but Dr. Austin did not observe that any precipitation was occasioned in lime water by agitating it with the electrified gas. On passing up syrup of violets to the electrified air, with the expectation of its indicating the volatile alkali, as in the experiments of Dr. Austin, no change of colour took place, though the test was of unexceptionable purity. On examining, however, whether any alteration of bulk had been produced in the air by the contact of this liquid, it appeared that of 709 measures 100 had been absorbed. Suspecting that the absorption was owing to the presence of carbonic acid, I introduced some lime water to a volume of the expanded gas, amounting to 556 measures, when they were immediately reduced to 512. The contraction would probably have been still more remark-

\* Journal de Physique, xxix. 277.

† Annales de Chimie, ii. 273.

able, if the gas had been farther expanded before the admission of the liquid. The change in the lime water was very trifling; but my friend Mr. Rupp, who witnessed this as well as several of the other experiments, and who is much conversant in the observation of chemical facts, was satisfied that after a while he saw small flocculi of a precipitate on the surface of the mercury. This contraction of bulk cannot be ascribed to any other cause than the absorption of carbonic acid; for, besides the fact, that the colour of syrup of violets and of turmeric, which I also tried, were not affected by exposure to the electrified gas, I have this objection to the absorbed gas being ammoniac, that no diminution either of bulk or transparency occurred on the admixture of muriatic acid gas with the electrified air; whereas ammoniac would have been exhibited under the form of a neutral salt. When water was passed up to this mixture of the two gases, there was an absorption not only of the muriatic gas but of something more.

Conceiving that the demolition of charcoal, by the action of the electric fluid, was sufficiently proved by his experiments, Dr. Austin assigns the evolved hydrogen as one of its constituents, and the other he concludes to be azote. This inference, however, rests almost entirely upon estimates in which material errors may be discovered. Some of these it may be well to point out for the satisfaction of such as have acquiesced in Dr. Austin's opinion.

The carbonated hydrogenous gas submitted to Dr. Austin's experiments, clearly appears from his own account to have been largely adulterated with azotic gas. One source of its impurity he has disclosed, by informing us that the gas "had been very long exposed to water\*," for Dr. Higgins has somewhere shewn, that the heavy inflammable air, after standing long over water, leaves a larger residue of azote, on combustion, than when recently prepared†. It is probable also, that the proportion of azote derived from the water would increase with the time of its exposure; and thus a fertile source of error is suggested, which appears wholly to have escaped Dr. Austin's attention. In repeating his experiments, I was careful that comparative ones, on two equal quantities of the electrified and unelectrified gas, should be made without the intervention of any time that could vary the proportion of azote in either of the gases.

To the 9th experiment, in which the quantity of azote seems to have been increased to electrization, I must repeat the objection, that a sufficiency of oxygenous gas was not used in the combustion. In the 8th experiment, 2,83 measures of the unelectrified air were fired with 4,17 oxygenous gas, and only 0,15 of the latter remained above what was sufficient for saturation; but in the 9th, though the 2,83 measures were expanded to 5,16, the quantity of oxygen employed was 0,08 less than in the former experiment; and it may therefore be presumed, that a small quantity of inflammable air might escape unaltered, and might add apparently to the product of azote. In the 8th experiment, also, the portion of oxygenous gas that was more than sufficient to saturate the carbonated hydrogen, would probably combine in part with the remaining azote, as in the experiments of Dr. Higgins‡ and Dr. Priestley§. But in the 9th, the quantity of oxygenous gas was hardly sufficient to saturate

\* Phil. Transf. lxxx. 54.

† Similar facts respecting the deterioration of other gases by standing over water may be seen in Dr. Priestley's Experiments on Air, vol. i. p. 59. 158. I found that oxygenous gas from oxygenated muriate of pot-ash acquired by exposure a few weeks to water, .125 its bulk of azotic gas.

‡ Experiments and Observations on Acetous Acid, &c. p. 295.

§ Phil. Transf. lxxix. 7.

both kinds of inflammable air after electrization, and could not therefore diminish the azotic gas. When the proportion of oxygen is duly increased, and the inflammation of the electrified air is performed in small portions, there is no augmentation, but on the contrary a decrease of the quantity of the azote, as will appear on comparing the 1st and 2d of the experiments which I have related.

Two circumstances were observed in the experiments of Dr. AUSTIN, which have not been noticed in the preceding account of the repetition of them, viz. the appearance of a deposit from the carbonated hydrogenous gas during its electrization, and the formation of ammoniac by the same process. In some experiments which I made on the first portion of gas, both these facts were sufficiently apparent; but neither of them occurred on electrifying the gas which was afterwards procured. Suspecting that the cessation of them arose from the superior purity of the latter portion from azotic gas, I passed the electric shock through a mixture of carbonated hydrogen, with about one fourth of its bulk of azote, and thus again produced the precipitate, which would have been of a white colour, if it had not been obscured by minute globules of mercury, that were driven upwards by the force of the explosion. An infusion of violets was tinged green, when admitted to the electrified gas; but the change of colour did not occur instantly, as happens from the absorption of ammoniacal gas; and required for its production, that the liquid should be brought extensively into contact with the inner surface of the tube. From this effect on a blue vegetable colour, we may infer that the precipitate was an alkaline substance, and probably the carbonate of ammoniac; but the quantity was much too minute to be the subject of a more decisive experiment.

I shall conclude this memoir with a brief summary of the facts that are established by the preceding experiments\*. Those included under the first head are deducible from the experiments of Dr. AUSTIN.

1. Carbonated hydrogenous gas, in its ordinary state, is permanently dilated by the electric shock to more than twice its original volume; and as light inflammable air is the only substance we are acquainted with, that is capable of occasioning so great an expansion, and of exhibiting the phenomena that appear on firing the electrified gas with oxygen, we may ascribe the dilatation to the production of hydrogenous gas.

2. The hydrogenous gas, evolved by this process, does not arise from the decomposition of charcoal; because the same quantity of that substance is contained in the gas after as before electrization.

3. The hydrogenous gas proceeds from decomposed water; because, when this fluid is abstracted as far as possible from the carbonated hydrogenous gas, before submitting it to the action of electricity, the dilatation cannot be extended beyond one-sixth its usual amount.

4. The decomponent of the water is not a metallic substance, because carbonated hydrogenous gas is expanded when in contact only with a glass tube and gold, a metal which has no power of separating water into its formative principles.

5. The oxygen of the water (when the electric fluid is passed through carbonated hydrogenous gas, that holds this substance in solution) combines with the carbon, and forms car-

\* Since this paper was written, I have extended the inquiry to phosphorated hydrogenous gas, which expands equally with the carbonated hydrogen; loses its property of inflaming when brought into contact with oxygenous gas, and affords evident traces of a production of phosphorous or phosphoric acid.

bonic acid. This production of carbonic acid, therefore, adds to the dilatation occasioned by the evolution of hydrogenous gas.

6. There is not, by the action of the electric matter on carbonated hydrogenous gas, any generation of azotic gas.

7. Carbon, it appears, therefore, from the united evidence of these facts, is still to be considered as an elementary body; that is, as a body with the composition of which we are unacquainted, but which may nevertheless yield to the labours of some future and more successful analyst.

## II.

*Observations on Bituminous Substances, with a Description of the Varieties of the Elastic Bitumen.* By CHARLES HATCHETT, Esq. F.R.S. Lond. and Edin. F.L.S. &c.

[Concluded from Page 209, Vol. II.]

THE characters of bitumen are much more apparent in turf and peat, than in the greater part of the fossil woods. Turf is well known to be composed of the parts of vegetables, such as small roots, twigs, &c. mixed with a portion of petroleum; and peat is the same, excepting that it generally contains more of earthy matter, or that the vegetables have undergone a more complete decomposition.

The boggy nature of the places in which they are found, proves that a certain degree of maceration is necessary to form the bituminous matter which they contain; and I have already noticed, that every fact appears to demonstrate, that the bitumen is a product of those vegetables, the remains of which constitute the other ingredient of turf and peat.

The different proportion of vegetable matter, of bitumen, and of earth, together with the different state of the bitumen, as well as the degree of perfection respecting the formation of it from the vegetable principles, contribute to alter the properties and characters of the compound, and thus produce varieties. It is believed that these substances have been materially concerned in the formation of pit-coal, and some eminent mineralogists maintain that there is an uninterrupted series which connects the varieties of turf and peat with those of coal\*.

## SECT. V.

LITTLE need be said concerning those mixtures of bitumen with metals or their oxides which are sometimes called the bituminous ores of mercury, copper and iron, for they should rather be arranged with the adulterated or impure bitumens. Few of them contain the metallic ingredient in a proportion sufficient to cause the compound to be worked as an ore; and the only exception with which I am acquainted, is the substance found at Idria, in Carniolia, composed of mercury mixed with bituminous matter, a quintal of which, according to Mr. de Born, affords from fifteen to twenty pounds of mercury†.

\* Man findet in der natur einen ununterbrochenen übergang von dem rasen und papiertorf durch den moor oder sumpftorf in den pechtorf, und von diesem in die braun schiefer und pechkohle.—Widenmann, p. 630.

† Catalogue de la Collection des Fossiles de Mlle. de Raab, tom. ii. p. 294, 348, and 400.

## SECT. VI.

## SECT. VI.

FROM the preceding observations it will appear, that although I have first mentioned naphtha in order that I might be better understood in respect to the degree of connection prevailing between the bituminous substances; yet, to have followed them from their origin and the period of their formation, I should rather have begun with those substances which most clearly point out how much the vegetable kingdom has contributed to the production of them, with the probable occasional concurrence of animal substances.

That the latter have contributed in some measure to the forming of bitumen, we can only infer from the vestiges and exuviae of animals, which so commonly accompany bituminous substances: but no doubt can be entertained in respect to vegetables; for it appears that bitumen is formed from them by long maceration, and by other processes at present unknown to us:

That when certain portions of vegetable matter remain undecomposed, and are mixed with the petroleum thus produced, the varieties of turf and peat are formed:

That wood in general contributes to the production of bitumen; but does not seem to retain it, after the formation of it, in so considerable a proportion as the foregoing substances:

That the bituminous matter thus formed, and occasionally separated, is in different states according to the degree of inspissation:

And lastly, with various proportions of carbonic and earthy matter, it forms jet, coal, and bituminous schistus; and with metallic substances it produces those compounds called bituminous ores.

## SECT. VII.

ABOUT the year 1786 a new species of bitumen was discovered near Castleton, in Derbyshire, which much resembles, in elasticity and colour, the substance known by the name of cahout-chou, or Indian rubber.

M. de Born was, I believe, the first who mentioned it\*; but, as he appears to have known only one variety of this singular substance, I am induced to hope that a description of many other varieties, which have since been found, will not be unacceptable to this Society.

The elastic bitumen, which resembles the cahout-chou, was first discovered in the cavities of a vein in the lead-mine called Odin, which is near the base of Mamtor, to the north of Castleton. The ore of this mine (which is supposed to be one of the most ancient in England) is galena, accompanied by fluor, calcareous and heavy spars, quartz, blende, calamine, selenite, asphaltum, and the elastic bitumen, although the latter is now rarely found†. Another species of the elastic bitumen has within about three years been found in a neighbouring rivulet; but I shall not at present notice it, as I intend first to describe the varieties of that which was first discovered, and which resembles the cahout-chou. In order to do this with more perspicuity, I shall describe the specimens belonging to my collection, according to the mode in which I have arranged them.

\* Catalogue de la Collection de Mlle. de Raab, tom. ii. p. 77.

† I am indebted to the ingenious Mr. White Watson, of Bakewell, for much information respecting the local circumstances which attend this bitumen.

## SPECIES THE FIRST.

A, No. 1.—Elastic bitumen of a yellowish brown colour, part of which is almost liquid like petroleum, and adheres to the fingers; the other part is of a darker colour, of a mammillary form, does not adhere to the fingers, and is soft and elastic. This is on a grey bituminous limestone, with white calcareous spar in the figure of hexaedral pyramids, forming that which is called the dog-tooth spar.

A, No. 2.—Bitumen of a yellowish brown, partly liquid, and partly elastic, which, however, adheres to the fingers; on pale grey limestone, with crystals of white fluor spar, blende, and galena.—On another part of the limestone are some globules of bitumen of a reddish brown, perfectly hard and brittle.

A, No. 3.—Dark brown bitumen of a stalactitical form, hard, but in some degree elastic.

A, No. 4.—Bitumen of a reddish brown, in the form of globules, some of which are elastic, and others hard: on brownish-grey limestone, accompanied by crystallized white fluor, dogtooth calcareous spar, and pyrites in small crystals, some of which are on the surface of the globules of bitumen.

A, No. 5.—The same of a darker brown, of a stalactitical form, hard and brittle; on pale brown calcareous spar, impregnated with bitumen.

A, No. 6.—Bitumen of a dark reddish brown, very hard; on pale brown sparry flintstone, with grey limestone, in which are some coralloides.

A, No. 7.—Bitumen of a dark yellowish brown, elastic, but very soft, so that it adheres to the fingers.

A, No. 8.—The same thinly spread over grey sparry flintstone.

A, No. 9.—Bitumen of a brownish olive colour, which becomes reddish brown by the air, but when exposed to the light it appears semi-transparent, and of a yellowish brown inclining to orange. It is soft, very elastic, and (when recently cut) adheres to the fingers.

A, No. 10.—The same of a darker brown, and harder in a small degree. The specific gravity of this specimen is 0,9053; water being estimated at 10,000 (q. 1,000?) at temp. 60°.

A, No. 11.—Bitumen of a dark brown, harder than the former. This exactly resembles the cahout-chou in the degree of elasticity, and in the property which it possesses of removing the traces of black-lead.

A, No. 12.—The same, but rather harder.

A, No. 13.—The same of a blackish brown, which is slightly elastic when the weather is warm, but is brittle when cold.

A, No. 14.—The same of a blackish brown, nearly black, which scarcely possesses any elasticity; it breaks, and resembles asphaltum in lustre, colour and fracture.

A, No. 15.—The same of a reddish brown, perfectly hard and brittle. The characters of asphaltum are complete in this specimen. The specific gravity is 10,233. (q. 1,233?)

The other species of elastic bitumen, which I shall distinguish by the letter B, has been found during the last three years in a rivulet which runs at the base of Mamtor, from west to east, at a small distance from Odin mine.—The varieties of it, in my possession, are as follow:—

SPECIES THE SECOND.

B, No. 1.—Elastic bitumen, which, recently cut, exactly resembles fine close cork in colour and texture, but, by the air, in a few days it becomes of a pale reddish brown.—This forms a thin coat, which completely covers a mass of elastic bitumen, which is soft, and of a brownish olive colour, like A, No. 9.

B, No. 2.—The same, excepting that the coat or crust is much thicker.

B, No. 3.—The same, but the coating is thicker than that of No. 2, and the brownish olive-coloured bitumen much less in quantity.

B, No. 4.—The same, excepting that the greater part of the mass resembles cork, so that only a very small nucleus of the brown bitumen remains\*.

B, No. 5.—The same, excepting that the bitumen, which is coated, is in the state of asphaltum. The specific gravity of this specimen is 0,9881.

B, No. 6.—Elastic bitumen, the whole mass of which resembles fine cork.—The specific gravity is 0,9748.

B, No. 7.—The same, but friable, and apparently passing by decomposition into an ochraceous coloured powder.

THE varieties of the first species of the elastic bitumen, or that which is like the cahout-chou, evidently appear to be formed from a naphtha or petroleum, which, like that which produces the other simple bituminous substances formerly mentioned, is susceptible of various degrees of inspissation.

All the varieties of the first species, from No. 1, to No. 15, may be regarded as thus formed; for in these we can trace all the modifications comprehended between petroleum and asphaltum: with this difference, that the intermediate modifications of this species have the remarkable property of elasticity which is the most complete in the variety which occupies the middle place between petroleum and asphaltum.

The second species B, or that which resembles cork, appears so different from that marked A, that it is not at first easy to conceive how they are connected, or at least the difficulty must appear great to those who have only seen specimens of each species complete in their respective characters. But, from an attentive examination of many specimens, and particularly of those which I have described, I am convinced that the varieties of the species B are only modifications of the species A, produced probably by long maceration in the water of the rivulet in which this species is found, to the effects of which we may, with some appearance of reason, add the vicissitudes of the seasons, of air, and of the weather in general, as well as those of reiterated moisture and dryness occasioned by the rise and fall of the water of the rivulet; and what seems to corroborate this opinion is, that the substance, like cork, incrusts the species A, and appears to be only a change which has penetrated deeper into the substance of it in proportion to the duration of the causes which I have mentioned, so that at length the original substance no longer remains in its primitive state. I do not believe, however, that this change arises from any alteration in the constituent principles, but merely

\* One of the specimens in my possession, similar to B, No. 4. weighs between 13 and 14 pounds.

from a partial and minute dis-union or disintegration of the particles of the original substance, as both species melt into one which is perfectly similar. I must also add, that the species A burns easily, and with rapidity; but the species B burns with some difficulty, and crackles as if it had imbibed a quantity of water.

I have remarked, when the different varieties of the elastic bitumen were melted, that they completely lost the elastic property, and a quantity of air or gas appeared to be disengaged, particularly from the species B. I also observed, that the substances which remained after this operation, corresponded, in respect to consistence, with those which had been employed, as the following table will shew :

A, No. 7 and 8 . . . . . produced a thick liquid petroleum, not apparently different from that which is commonly known.

A, No. 9 . . . . . produced a thicker petroleum, approaching to mineral tar.

A, No. 11 and 12 . . . produced mineral tar.

B, No. 6 . . . . . produced the same, approaching to mineral pitch.

A, No. 13 . . . . . produced mineral pitch.

A, No. 14 and 15 . . . did not suffer any change, but remained as at first, with all the characters of asphaltum.

From what I have related, I suspect that the elastic property is occasioned by the interposition of very minute portions of air or some other elastic fluid between the parts of the bitumen, and that this takes place by reason of some unknown cause at the time of formation; but when these bitumens are melted, the elastic fluid is liberated, and the mass loses that fine spongy texture which I suspect to have been the cause of the elastic property\*.

Derbyshire is well known as a country which exhibits, in the most striking manner, the remarkable changes which our globe has suffered. In every part of it, the most indisputable evidences appear of some great and extraordinary revolution; and there is not any place where extraneous fossils, such as the remains and impressions of vegetables and animals, are more abundant.

Bitumen, in other countries, is most commonly found where these present themselves; and, in like manner, there are few countries which abound so much with bitumen as Derbyshire.

Whoever has examined the limestone rocks about Matlock, and most other places in this county, must be convinced of the truth of this assertion.

The limestone and calcareous spars also, where the elastic bitumen is found, are, for the greater part, in the same state; so that no doubt can be entertained but that this bitumen has had the same origin as those which are more generally known; and it would undoubtedly have been confounded with them, had it not been discovered when passing from the liquid to the solid state.

\* The elastic bitumen, A, No. 9. when digested in sulphuric ether in a temperature of about 55°, is partly dissolved. The solution is yellowish brown when opposed to the light; but, when otherwise viewed, is like the bitumen, that is, of a brownish olive colour. By spontaneous evaporation, the etheric solution leaves a yellowish brown bitumen, which is totally devoid of elasticity. The undissolved portion (like the *cahout-chou* under similar circumstances) is softened, and is much increased in bulk.

The species B, No. 6. cut into very thin slices, communicates a yellow tinge to sulphuric ether; in other respects it is but little affected.

The



The elementary principles of bitumen are, hydrogen, carbon, sometimes azote, and probably some oxygen, which, by its action on the other principles, tends to form the concrete bitumens, and also produces that portion of acid obtained by chemical operations. These same principles, hydrogen and carbon, constitute the vegetable oils and resins; and the same, with some azote, form the oils and grease of animals. Now it is known that very small changes in the respective proportions of these ingredients, and in the circumstances which attend the combination of them, will cause considerable variations in the nature of the products; and in like manner, it appears very probable, that when the organized bodies in their recent state, and in the full possession of the above-mentioned principles, have been buried in a situation where these principles have been long elaborated under certain favourable circumstances, and subjected to the action of mineral bodies; I say that it appears highly probable, that a new combination, which we call bitumen, may be formed, which, although different in some respects from the vegetable and animal products, still, however, retains many characters of those substances from the principles of which it has been formed.

HAMMERSMITH,

April 26, 1797.

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### III.

#### *Observations on the Physical and Political Geography of North Africa.*

By JAMES RENNEL, Esq. F. R. S. \*

TO our view, North Africa appears to be composed of three distinct parts, or members. The first and smallest is a fertile region along the Mediterranean, lying opposite to Spain, France, and Italy (commonly distinguished by the name of Barbary); and which, could we suppose the western basin of the Mediterranean to have once been dry land (bating a lake) or recipient for the surrounding rivers), might be regarded as a part of Europe; as possessing much more of the European than the African character.

The second part is what may be deemed the body of North Africa, comprised between Cape Verd and the Red Sea, on the east and west; and having the Great Desert (or Sahara) and its members, on the North; the Ethiopic ocean and South Africa, on the opposite side. The prominent feature of this immense region is a vast *belt of elevated land* of great breadth, often swelling into lofty mountains, and running generally from west to east, about the tenth degree of latitude. Its western extremity seems to be Cape Verd; the mountains of Abyssinia, the eastern. To the north, its ramifications are neither numerous nor extensive, if we except the elevated tract which turns the Nile to the northward beyond Abyssinia. Towards the south no particulars are known, save that a multitude of rivers, some of them very large, descend from that side and join the Atlantic and Ethiopic seas, from the Rio Grande on the west to Cape Lopez on the east; proving incontestably that by far the greatest proportion of rain water falls on that side during the periodical season of the

\* Copied by permission from his "Geographical Illustrations of Mr. Park's Journey," in the Proceedings of the African Association, 1798. On this subject see likewise our Account of Books in the present Number N.

S. W. winds; which corresponds in all its circumstances with the same monsoon in India\*.

To the north of this belt, with the exception of the Egyptian Nile, the waters conform generally to the direction of the high land; passing at no great distance (comparatively) from its base to the right and left; as if the surface of the Sahara had a general dip to the southward†. These rivers moreover receive all their supplies from the south; no streams of any bulk being collected in the Desert.

In order to produce this effect, there must necessarily be a vast hollow in the interior of Africa, between the high land of Nubia on the east, and Manding on the west; and of which the mountains and desert form the other two sides. Nor is this state of things unexampled in the other continents. In Asia, the hollow, to whose waters the Caspian and Aral serve as recipients, is no less extensive than the one just mentioned; reckoning from the sources of the Wolga to those of the Oxus; (which latter has ever communicated with the Caspian, either throughout the year or during a part of it:) the difference is, that in Asia a greater portion of the hollow is filled up with water than in Africa.

The third part is of course the Great Desert (or Sahara) and its members; consisting of the lesser deserts of Bornou, Bilma, Barca, Sort, &c. This may be considered as an OCEAN OF SAND‡, presenting a surface equal in extent to about *one half of Europe*, and having its gulfs and bays; as also its islands fertile in groves and pastures, and in many instances containing a great population subject to order and regular government. The great body or western division of this OCEAN, comprised between Fezzan and the Atlantic, is no less than 50 caravan journeys across from north to south; or from 750 to 800 G. miles; and double that extent in length: without doubt the largest desert in the world. This division contains but a scanty portion of islands (or oases), and those also of small extent: but the eastern division has many; and some of them very large. Fezzan, Gadamis, Taboo, Ghanat, Agadez, Augela, Berdoa, are amongst the principal ones: besides which there are a vast number of small ones. In effect this is the part of Africa alluded to by Strabo§, when he says from Cneius Piso, that Africa may be compared to a leopard's skin. I conceive the reason why the oases are more common here than in the west, is, that the stratum of sand is shallower from its surface to that of the earth which it covers. In other words, that the water contained in that earth is nearer to the surface; as in most of the oases it springs up spontaneously§. Can any part of the cause be assigned to the prevalent easterly winds, which, by driving the finer particles of sand to leeward, may have heaped it up to a higher level in the Sahara than elsewhere?

The

\* A ridge stretches to the south through the middle of South Africa, and forms an impenetrable barrier between the two coasts. M. Correa de Serra informs me, that the Portuguese in Congo and Angola have never been able to penetrate to the coast of the Indian Ocean.

Mr. Bruce learned (vol. iii. p. 668.) that a high chain of mountains from 6° runs southward through the middle of Africa. He supposes the gold of Sofala to be drawn from these mountains. (p. 669.)

† Circumstances have shewn, that it declines to the eastward also.

‡ "A wild expanse of lifeless sand and sky!" THOMSON.

§ Page 130.

§ Water is found at the depth of a few feet in Fezzan (African Assoc. Q. p. 96. O. p. 146.) The same is said

The springs no doubt have produced the oases themselves, by enabling useful vegetables to flourish, and consequently population to be established. That the Desert has a dip towards the east as well as the south, seems to be proved by the course of the Niger also. Moreover the highest points of North Africa, that is to say, the mountains of Mandinga and Atlas, are situated very far to the west.

The Desert for the most part abounds with salt. But we hear of salt mines only in the part contiguous to Nigritia, from whence salt is drawn for the use of those countries as well as of the Moorish states adjoining; there being no salt in the Negro countries south of the Niger \*. There are salt lakes also in the eastern part of the Desert.

The great ridge of mountains and its branches are very productive in gold; but more particularly in the quarters opposite to Manding and Bambouk on the west, and Wangara on the east. It may perhaps admit of a doubt, whether the gold is brought down at the present time by the numerous fountains that form the heads of the Niger and Senegal rivers; or whether it has been deposited in the lower parts of their beds at an earlier period of the world; and that the search, instead of being facilitated by the periodical floods, is on the contrary only to be pursued with effect when the waters are low.

Tombuctoo is reckoned the mart of the Mandinga gold, from whence it is distributed over the northern quarters of Africa by the merchants of Tunis, Tripoly, Fezzan, and Morocco; all of whom resort to Tombuctoo. Most of it no doubt afterwards finds its way into Europe. It may be remarked also, that the gold coast of Guinea (so called doubtless from its being the place of traffic for gold dust) is situated nearly opposite to Manding: but whether the gold brought thither has been washed out of the mountains by the northern or southern streams, I know not: it may be both †. Degombah, another country said to be very productive

said by Pliny, concerning this quarter of Africa; lib. v. c. 5. But farther to the N. W. on the edge of the Desert, and in the country of Wadraag in particular, (Shaw, p. 135.) wells are dug to an amazing depth, and water mixed with fine sand springs up suddenly, and sometimes fatally to the workmen. The Doctor tells us that the people call this abyss of sand and water, "the sea below ground." Exactly the same state of things exists in the country round London, where the sand has in several cases nearly filled up the wells. (See Phil. Trans. for 1797.) The famous well lately dug by Earl Spencer (at Wimbledon), of more than 560 feet in depth, has several hundred feet of sand in it.

\* This quality of the African Desert was familiarly known to Herodotus (Melpom. c. 181, et seq.) He knew also that there was salt in abundance in the northern parts. But, as the inhabitants in that quarter can furnish themselves with salt of a better quality from the sea, the mines are not wrought.

† Some writers have said, that there are gold mines in the neighbourhood of Mina, on the gold coast; others that the gold is rolled down by the rivers to that neighbourhood. Both may be true. But, on the other hand, it is said that the gold of Wangara is also brought for sale to the southern coast.

It is difficult to conceive any other adequate cause, than the exchange of the gold of the inland countries for the introduction of so vast a quantity of kowry shells, which are carried from Europe to the coast of Guinea, and pass for small money in the countries along the Niger from Bambara to Kassinna, both inclusive.

I am informed from authority, that about 100 tons of kowries are annually shipped from England alone to Guinea. These are originally imported from the Maldivé islands into Bengal; and from Bengal into England. In Bengal 2400 more or less are equal to a shilling; and yet, notwithstanding the incredible smallness of the denomination, some article in the market may be purchased for a single kowry. But in the inland parts of Africa they are about ten times as dear, varying from 220 to 280. Mr. Beaufoy was told, that in Kassinna they were at the rate of about 250. And Mr. Park reports, that they are about the same price at Sego: but cheaper

ductive in gold \*, must by its situation lie directly opposite to the gold coast: for it lies immediately to the east of Kong (the Gonjah of Mr. Beaufoy and the Conche of D'Anville †.) The people of Fezzan trade to Kong.

The triangular hilly tract above commemorated, (p. 71 of the "Illustrations") which projects northward from the highest part of the belt, and contains Manding, Bambouk, &c. is also abundant in gold; particularly in the quarter towards Bambouk, where it is found in mines; and that chiefly in the middle level ‡. (See also p. 71.)

Wangara appears to have been in its time nearly as rich as Manding in this metal. The Arabs name it *Belad al Tebr*; or the *country of gold* ||. Edrifi, Ibn Al Wardi, and Leo, bear testimony to its riches. They say that the gold is found in the sands after the periodical inundation of the Niger (which is generally over the country) is abated §. Leo alone \*\* says, that the gold is found in the southern quarter of the kingdom; which appears very probable, as the mountains lie on that side: so that it may be concluded, that the gold sand has not been brought there by the Niger, but by smaller rivers that descend immediately from these mountains. That a part of Wangara is bounded by mountains, we learn from Edrifi: for the lake on which Reghebil stands has mountains hanging over its southern shore ††.

It is supposed that most of the countries bordering on these mountains share in the riches contained within them, by means of the rivulets ‡‡. But considering how amazingly productive in gold the streams of this region are, it is wonderful that Pliny should not mention the Niger, as one of the rivers that rolls down golden sands: for although he speaks of the Tagus and others in different quarters, no African river is mentioned |||. And yet Herodotus knew, that the Carthaginians bartered their goods for gold, with the Africans on the sea coast beyond the pillars of Hercules; which was contrived without the parties seeing each other §§.

The common boundary of the Moors and Negroes in Africa forms a striking feature, as well in the moral as the political and physical geography of this continent. The Moors descendants of Arabs, intermixed with the various colonists of Africa from the earliest to the

cheaper at Tombuctoo, which is about the centre of the kowry country; dearer towards Manding, which is the western extremity of it. Hence they are probably carried in the first instance to Tombuctoo, the gold market; and thence distributed to the east and west. Their circulation seems to be confined between Bornou and Manding. In Bornou they have a coinage of base metal.

\* African Assoc. Q. p. 176. O. p. 264.

† Mr. Park says, that Kong signifies mountain in the Mandinga language; which language is in use from the frontier of Bambara to the western sea.

‡ Labar, vol. iv. ch. 2.

|| Bakui, and Herbelot; article Vankara.

§ See Edrifi in particular, pages 11 and 12.

\*\* Page 254.

†† Edrifi, page 12.

‡‡ Mr. Bruce, vol. iii. p. 647, says the same of the mountains of Dyre and Tegla, which are a continuation of the great belt, towards Abyssinia.

||| Pliny, lib. xxxiii. c. 4.

§§ Melpomene, c. 196.

Dr. Shaw (p. 302) speaks of the same mode of traffic at present between the Moors and Negroes; whence the place of traffic ought to be very far removed from the Mediterranean. There is a similar story related by Cadamosta of the exchange of salt for gold in Melli; and by Dr. Wadstrom on the windward coast of Guinea.

latest times overspread the habitable parts of the Desert, and the oases within it; and have pushed their conquests and establishments southward; pressing on the Negro aborigines, who have in several instances retired to the southward of the great rivers; but in others preserve their footing on the side towards the Desert; according to the strength or openness of the situation. It is probable, however, that the Negroes, who are an agricultural people, never possessed any considerable portion of the Desert, which is so much better suited to the pastoral life of the Moors. It appears as if matters had not undergone much change in this respect since the days of Herodotus; who fixes the boundary of the Libyans and Ethiopians, in other words, of the Moors and Negroes, near the borders of the Niger; and he apparently pointed to the quarter in which Kassina or Ghana are now situated\*.

The Negroes in the western quarter of the continent are of two distinct races, of which the least numerous are named Foulahs or Foolahs. These, although they partake much of the Negro form and complexion, have neither their *jetty* colour, *thick* lips, or *crippled* hair. They have also a language distinct from the Mandinga, which is the prevailing one in this quarter.

The original country of the Foulahs is said to be a tract of no great extent along the eastern branch of the Senegal river; situated between Manding and Kasson; Bambouk and Kaarta: and which bears the name of Foola-doo, or the country of the Foulahs. But whether this be really the case, or whether they might not have come from the country within Serra Leona (called also the Foulah country), may be a question; of which, more in the sequel. The Foulahs occupy at least as sovereigns several provinces or kingdoms, interspersed throughout the tract comprehended between the mountainous border of the country of Serra Leona on the west, and that of Tombuctoo on the east; as also a large tract on the lower part of the Senegal river; and these provinces are insulated from each other in a very remarkable manner. Their religion is Mahomedanism, but with a great mixture of Paganism; and with less intolerance than is practised by the Moors.

The principal of the Foulah states is that within Serra Leona; and of which Teemboo is the capital. The next in order appears to be that bordering on the south of the Senegal river, and on the Jaloffs: this is properly named Siratik. Others of less note are Boudou, with Foota-Torra adjacent to it, lying between the rivers Gambia and Falemé; Foola-doo and Brooko along the upper part of the Senegal river; Wassela beyond the upper part of the Niger; and Massina lower down on the same river, and joining to Tombuctoo on the west.

The Moors have not in any instance established themselves on the south of the great rivers. They have advanced farthest to the south in the western quarter of Africa; so that the common boundary of the two races passes, in respect of the parallels on the globe, with a considerable degree of obliquity to the north, in its way from the river Senegal towards Nubia and the Nile. Mr. Park arranges the Moorish states, which form the *frontier* towards Nigritia, together with the Negro states opposed to them on the south, in the line of his progress, in the following order:

The small Moorish state of Gedumah, situated on the north bank of the Senegal river, and the last that touches on it†, is opposed to the small Negro kingdom of Kajaaga, on the

\* See Euterpe, c. 32; and Melpomene, c. 197.

† The Moors appear to be masters of the northern bank of the Senegal, through the greatest part of its navigable course; the Foulahs the southern bank.

south. This latter occupies the extremity of the navigable course of the Senegal, terminated in this place by the cataract of F'low.

From this point the Negro and Foulah states occupy both banks of the Senegal river to its source; and beyond that both banks of the Niger (or Joliba) likewise, to the lake Dibble, situated beyond the term of Mr. Park's expedition. This space is divided unequally between Kaffon, a hilly strong country, but of small extent; and which has the Moors of Jaffnoo on the north: Kaarta a considerable state, which has Ludamar for its opposite (a country held by Ali, a Moorish prince, who is loaded with infamy on the score of maltreatment of the only two Europeans who appear to have entered his country in latter times): Bambara of still more consideration, having the Moorish kingdom of Beeroo to the north: and Massina, a Foulah state bordering also on the south of Beeroo.

Herc Mr. Park's personal knowledge ends; but he learnt that Tombuctoo and Houssa, which succeed in order to Massina, and occupy both sides of the Niger, are Moorish states, though with greatest proportion of Negro subjects: so that the river may be considered as the boundary of the two races in this quarter\*.

Of the countries between Houssa and Kaffina we are ignorant. The Desert seems to approach very near the river (Niger) in that quarter, whence a Moorish population may be inferred. South of the river, we hear of Kaffaba, Gago, and other Negro countries; but without any distinct notices of position; and beyond these Melli.

Kaffina and Bornou, two great empires on the north of the river, appear to divide the largest portion of the remaining space to the borders of Nubia; and extend a great way to the north; this region being composed of desert and habitable country intermixed; but perhaps containing the largest proportion of the latter. In both these empires, the sovereigns are Mahomedans, but the bulk of their subjects are said to adhere to their ancient worship; that is to say, the lower orders are almost universally Negroes†.

From what has appeared, perhaps the boundary of Nigritia as it respects the Negro population may be expressed generally, and with a few exceptions, as follows: beginning from the west, the extent upwards of the navigable course of the Senegal river, generally--thence a line drawn to Silla; from Silla to Tombuctoo, Houssa, and Berissa, along the river Niger; and thence through Afouda, Kanem, and Kuku, to Dongola on the Nile.

Leo‡ enumerates 12 states or kingdoms of Nigritia: but amongst these he includes Gualata, a tract only 300 miles S. of the river Nun: as also Cano (Ganat), adjacent to Fezzan; and Nubia. Kaffina, Bornou, and Tombuctoo, are included of course§.

The kingdom of the Foulahs, before mentioned, situated between the upper part of the Gambia river and the coast of Serra Leona, and along the Rio Grande, has also a Maho-

\* The emperor of Morocco is said to have held at one period the sovereignty of some of the countries on the northern banks of the Senegal and Niger rivers. Labar, vol. iii. p. 339, speaks of incursions made by his troops.

† African Assoc. Q. p. 126. O. p. 191.

‡ Page 4.

§ The Arabs and Moors call Nigritia by the general name of Soudan. By Belad Soudan, or the country of Soudan, Abulfeda includes all the known part of Africa, south of the Great Desert and Egypt. With him Soudan is the southern quarter of the globe. D'Herbelot also allows it a wide range. Affnoo is another term for Nigritia, in use among the natives themselves. (See also Proceedings Afric. Assoc. Q. p. 164. O. p. 246.)

medan sovereign, but the bulk of the people appear to be of the ancient religion. It has been already said, that although they are a black people, they are less black than the Negroes generally, and have neither crisped hair nor thick lips: as also that they have a language distinct from the Mandinga. From these circumstances, added to that of situation, they appear clearly to be the *Leucæthiopes* of Ptolemy and Pliny. The former places them in the situation occupied by the Foulahs; that is, in the parallel of nine degrees north; having to the north the mountains of *Ryffadius*, which separate the courses of the Stachir and Nia rivers (Gambia and Rio Grande), and which therefore answer to the continuation of the great belt of high land in our geography; in which there is moreover another point of agreement, the *Caphas* of Ptolemy being the *Caffaba* of the map\*.

Ptolemy by the name evidently meant to describe a people less black than the generality of the *Ethiopians*; and hence it may be gathered that this nation had been traded with, and that some notices respecting it had been communicated to him. It may also be remarked, that the navigation of Hanno terminated on this coast; probably at Sherbro' river, or sound. And as this was also the term of the knowledge of Ptolemy, it may be justly suspected that this part of the coast was described from Carthaginian materials†.

Those who have perused the Journal of Messrs. Watt and Winterbottom, through the Foulah country in 1794, and recollect how flattering a picture they give of the urbanity and hospitality of the Foulahs, will be gratified on finding that this nation was known and distinguished from the rest of the Ethiopians at a remote period of antiquity‡.

The contrast between the Moorish and Negro characters is as great as that between the nature of their respective countries; or between their form and complexion. The Moors appear to possess the vices of the Arabs without their virtues; and to avail themselves of an intolerant religion, to oppress strangers: whilst the Negroes, and especially the Mandingas, unable to comprehend a doctrine that substitutes opinion or belief for the social duties, are content to remain in their humble state of ignorance. The hospitality shewn by these good people to Mr. Park, a destitute and forlorn stranger, raises them very high in the scale of humanity: and I know of no fitter title to confer on them than that of the Hindoos of Africa: at the same time by no means intending to degrade the Mahomedans of India by a comparison with the African Moors.

\* The *Soluentii* of Ptolemy may also be meant for the *Solimani* of Mr. Park.

† And it may also have been the scene of traffic mentioned in page 155; as Dr. Wadstrom speaks of such a custom in this quarter at the present day.

‡ Pliny (lib. v. c. 8.) also speaks of the *Leucæthiopes*, but seems to place them on this side of Nigritia. May it not be that certain tribes of Foulahs were then established, as at present, along the Senegal river?

## IV.

*Observations on Metallic Money; chiefly directed to ascertain the most advantageous Distribution and Figure of Gold, Silver, and Copper in Coins.*

**I**F the value of science be measured by its utility, there is no part of human knowledge that will rank higher than political economy. If we justly applaud the inventor, who by the construction of a machine, or the improvement of a chemical process, has added to the sum of our enjoyments, in one small part of the great scheme of social life; how much more is due to those, who, by investigating the processes upon which that scheme itself depends, have added to the facilities of mutual intercourse, and given vigour to every department of active industry?

When we contemplate the state of man, supported merely by the art of the hunter, or by the immediate products of the earth; associated less for the purpose of mutual assistance in the arts than for predatory enterprise or direct resistance against the oppression of his neighbours; we are almost inclined to think him of a different species from the same creature in the civilized state. Upon examination, however, it appears, that the inevitable necessity of events has produced the difference. The variety of productions, of wants and of fabrications, has given rise to barter or exchange. Mutual supply has increased the subdivision of labour, and improved the means of conveyance. Streams, roads, ships and carriages, have extended this beneficial intercourse. Confidence between man and man has advanced the moral principles of society, and afforded a progression, of which the past gradations may indeed be traced, but to the future part of which the imagination can scarcely afford a probable outline.

Among the impediments to commerce, the greatest undoubtedly is the charge of conveyance from place to place. This is the great obstacle which limits the exchange of commodities from one extremity of the world to the other. Whenever the charges of carriage arise to such an amount as to equal the effectual return in any remote market, the motive for conveying merchandize to that place ceases. If goods were always exchanged for goods, it is clear that the conveyance, under the uncertainty of disposal, would take place to a very small distance indeed; and the labour required to discover the persons willing to exchange would greatly enhance the charge. It would require a volume to enumerate and describe the expedients, moral as well as mechanical, by which these difficulties are in part subdued, and still more to deduce their origin and general effects. One of the chief of these expedients consists in the use of some article of merchandize, as the medium of exchange, which shall be acceptable to every man, and will therefore be received and held by the seller of any commodity until he shall meet with another individual, who he knows will again take it for the article he wants.

In the island of Madagascar, it is said, that the exchangeable value of goods is reckoned in hatchets, bullocks, and slaves; these commodities being universally vendible, and for that reason every where received. Smith affirms, that nails answer the same purpose in some parts of Great Britain. These and other instances may serve to shew how a preferable medium of exchange becomes adopted, and it will without difficulty be seen that the scarcest and least destrucible metals must have at length become the universal substitutes. For their value does not depend on their figure; they may be subdivided and joined again without



loss; they receive no injury by keeping; and the labour of conveying them from place to place forms a less part of their value than of almost every other article.

The first moneys were mere quantities of metal ascertained by weight, as the names of most species still indicate. The interference of governments was found necessary to assure the weight, and more particularly the fineness of determinate portions of metal; and this has given rise to an opinion, that a part of the value of coin must depend on the edict of the State which issues it. Whether statesmen themselves have in reality thought this to be the case, is little to the purpose; but it is certain that they have, from time to time, yielded to the temptation of diminishing the quantity of precious metal issued under a given denomination, either by openly deducting from the weight, or secretly debasing the coin\*. Transactions of this nature must have operated to the loss of all the creditors in the State; but they have never deceived the sellers, who have always regulated their prices by their knowledge of the real quantities of the metal, and not by the denomination or the supposed weight or fineness it might denote. The imaginary coin, or money of account, to be found in the mercantile books of almost every commercial nation, must have arisen partly from this cause.

I was led to the present examination by hearing that a committee of the Royal Academy has been appointed to take into consideration a proper design for a new coinage; and, upon enquiry, I find that his Majesty's Privy Council have repeatedly deliberated on this subject, and referred the ornamental part to the Royal Academy for their discussion and report. On such occasions it has always, and, I think, rightly, been considered as a becoming transaction on the part of individuals to give their thoughts to the world by the medium of the press; and under this conviction it is that I have ventured to place the subject in the light it appears to me to require.

The metals used for coinage are gold, silver and copper. According to the exchangeable value of gold, half a grain of this metal would purchase as much bread as a man could eat at one meal. This small piece of gold, if as thin as paper, would not measure above the tenth part of an inch in breadth, and would therefore be perfectly inconvenient for use. It has, in fact, been found, that the gold coin of the weight of 32 grains (or the quarter guinea) was too small to be conveniently used. The same observations will apply to the smaller sub-divisions of the shilling of silver; whence upon the whole it appears, that coins of all the three metals are required to facilitate our commerce of buying and selling.

Gold, silver and copper, like every other produce of human industry, depend for their value principally on the labour employed in producing and bringing them to market, and

\* This diminution has taken place throughout Europe. With us the pound of money, which about the year 1087 contained a pound weight of silver, has continued at less than one-third (or  $\frac{2}{3}$ ) of that quantity ever since the reign of Elizabeth. Our neighbours however have universally exceeded us in this respect. Thus the pound Flemish is less than eleven shillings; the French livre is ten pence, and the Italian lire is less than 2½d.

The Chinese still use fine silver, which they actually cut and weigh at every single payment. They are said to have formerly possessed silver coin; but whether they were urged to their present practice by uncertain variations in its value caused by their rulers, or by the difficulty of otherwise resisting the artifices of coiners, I know not.

in a considerable degree upon the actual demand. As these articles are not employed merely in the fabrication of coins, the demand will vary in each according to circumstances, which admit of no permanent ratio of exchange between them. If the State were to coin certain pieces of known weight and fineness out of each of these metals, and determine that a certain number of the silver pieces, for example, should in all cases be equivalent to one piece of the gold, it would naturally follow, supposing the individual to pay nothing for the coinage, that a debt might be discharged with most facility to the debtor, and consequently loss to the creditor, in the cheapest of these two metals, whenever by the fluctuation of the market either of them should come to represent a larger portion of the other than the edict of the government had determined. This consequence of fixing the relative value of coins would show itself in a variety of ways, which need not be enumerated; because it is certain that the dearer metal would occupy the greater part of the circulation, while the cheaper pieces would either be melted down or diminished, if their rated value were too high, or they would be fabricated by individuals if it were too low, in defiance of every public regulation which might be adopted. If we therefore admit, from considerations of this nature, that no government does in reality possess the means of fixing a ratio between two articles of commerce, intended to be applied as the tickets of transfer or mediums of exchange, we shall be naturally led to the adoption of one of the metals only, as the representative sign, while the two others are applied merely as instruments of accommodation for the convenient sub-divisions of value.

With regard to the question of preference in these three metals, experience has shewn that society is disposed to assume the dearest; namely, gold. With a single standard of value, the fluctuations of the market price of the metal, when compared with other commodities, will be nearly imperceptible, because they confound themselves with the rise and fall in the prices of all other articles to which the standard is thus applied. If a cheaper metal were to be adopted by the State, and gold were left to circulate at the election of individuals, the changes of price in this metal of high value would operate so as to produce an uncertainty in the amount of large sums, and greatly disturb the general transactions of commerce. Merchants would therefore consider the gold coinage as mere bullion, and the community would in a great measure be deprived of its use as a coin; as is actually the case in Holland and other countries where silver is the legal medium\*. Hence it appears most eligible, that gold in pieces of determinate weight and fineness should constitute the effective coin of the State, or legal tender of payment; that silver and copper should be formed into money for the purpose of representing fractions of the smallest gold coin; and that the creditor or seller should have the option to refuse all payments in these last metals for any sum exceeding the smallest unity of the gold coin.

By this distribution, though the coins of silver and copper would in strictness be subject to fluctuations arising from the state of the market with regard to those metals, yet the dis-

\* A still more defective scheme was proposed in the Report presented by Prieur, de la Côte d'Or, from a Committee of the Council of Five Hundred, of which a very full abstract is given in the *Moniteurs* of 6 and 7 Floreal in the year VI. Nos. 216, 217. It is, that silver coin should be unchangeable in weight and denomination of value; but that the price of gold (also coined) should be settled every six months by a declaration from the National Treasury, deduced from the medium price of that metal during the preceding half-year. It was rejected by the Council of Ancients.

ference would be disregarded in the discharge of accounts, because it could never amount to a sum of any importance. The only inconvenience which offers itself under such an arrangement is, that these subordinate coins would also be melted and sold when the metal was dear, or they would be fabricated if the metal ever happened to be so cheap as to afford an adequate motive of profit to the illegal coiner. The State, in its deliberations on this subject, might determine that the coins of silver and copper should pass either for less than the medium market price of the metal, or for more, or for that value precisely. It is evident that the first of these dispositions would afford coin which would continually vanish in the melting-pot, and is therefore altogether inadvisable. The medium rate of intrinsic value would produce a similar effect whenever the market price was low. Whence it follows, that the metal contained in such auxiliary money ought to be of less value than the gold it represents; and to prevent the introduction of a similar coinage from private manufacturers, it would be necessary that the difference between the value of the metal and that represented by the coin should be somewhat less than the cost of workmanship. Under these circumstances the public would be supplied with an useful implement or ticket of exchange, which would operate as a pledge of value, very nearly to the amount of its denomination, and would be afforded cheaper from the extensive manufactories of government than it could possibly be made by private workmen.

Coin, like every other utensil or tool, is subject to wear, and will in process of time be more or less deprived of its distinctive figure, and rendered less valuable by the loss of weight. When new, it is the real pledge or measure it pretends to be; but, if it be suffered to circulate after its weight is considerably diminished, it may become a desirable object to the coiner to fabricate pieces apparently in the worn state, or otherwise he may exercise his industry in speedily reducing the new coin to that state, for the sake of the precious metal he may thus acquire.

If, on the contrary, the Legislature should forbid the currency of pieces worn beyond a certain small or moderate loss, the consequence will be, that all such pieces will return to the Mint to be recoined; and the charge of coinage may become so heavy as to absorb a considerable part of the value of the whole circulating medium in the course of a few years.

To diminish this last inconvenience as much as possible, it becomes necessary to attend to the nature of the metal as well as to the figure of the piece. Whether the Dutch ducat, of fine gold, or the English guinea, of twenty-two carats, may, under like circumstances, be most disposed to lose by wear, has not I believe been determined; but it seems to be generally understood, that our standard gold in watch-cases and other trinkets is less durable than the coarser and harder gold allowed to be wrought in France and Geneva. If this be true, it should seem as if there existed no motive for raising the standard of our gold, and perhaps the same argument may apply still more to our silver: and the advantage, if any, in lowering the standard without diminishing the intrinsic value, has not yet been shewn with sufficient evidence to justify the offence against established use and public prejudice which such a proceeding might afford. Admitting these observations to be conclusive against altering the standard, it would follow, that the greater durability of coin must be sought for in its figure.

Let us imagine a coin to possess the figure of an equilateral triangle; let it be thin, in order

order that it may present a large surface; let its edges have the figure of a saw, and its faces that of a file. Under these conditions we should fabricate one of the worst or least durable coins that could be chosen. For the angles would be easily broken and worn, and the edges and faces would mutually operate on each other, with a degree of rapidity which, it may be concluded, would very soon take away all the sharp prominences, and greatly diminish the weight. On the other hand, let us suppose the least possible surface, and we shall obtain the spherical figure \*. Against this it appears to be an objection, that if it be nearly perfect, the impressions distinctive of its purity and denomination must be indented, and will not therefore sufficiently limit its apparent magnitude; and if they be prominent it will no longer be a sphere, but a figure presenting sharp angular parts with small bearings very liable to destruction.—What then is the figure which shall partake so much of the plane, as to present surfaces of broad contact or bearing, and afford the least quantity of angular prominence? It is evidently the cylinder: and this is the figure most generally adopted for money. The edge of the cylinder affords the smallest bearing. It must therefore be very short or flat, in order that the weight of the piece may be disposed to rest on the base, and not on the edge.

If the whole surface of a piece of metal were covered with figures or impressions, it would be immediately seen whether any part had been abraded by accident or design. If the impressions were concave, they might easily be renewed by the punch or the graver; but if they were in relief, it would be almost impossible to restore them when once worn or obliterated. For this reason, the preference in coinage has mostly been given to figures in relief.

It is however a very serious inconvenience, that when the distinctive marks are thus rendered prominent, the face of the coin no longer sustains the pressure and wear of the piece; but the marks themselves are made to support the whole. Thus, in our gold money, particularly of the last recoinage, the edge is a saw, and the numerous minute prominencies on the face constitute a file, the operations of both which are severely felt in the rapid destruction of the piece †.

Hence we may observe, that neither kind of mark alone is suited to a coin intended to possess durability, and at the same time to be difficult either to imitate or diminish. A com

\* The pagoda and fanam of India are the only coins I recollect which approach towards this figure.

† To place this in a more striking light, it may be observed that the amount of gold coined between the years 1762 and 1772, both inclusive, was 8,203 l. 15s. 6d. and between 1782 and 1792, both inclusive, was 19,675,666 l. 14s. 6d. and between 1773 and 1777, both inclusive, was 10,591,833 l. 1s. During the middle period last mentioned the great recoinage of gold took place. I am aware that other causes may have occasioned a demand for coin besides the mere wear of the old pieces, and that the increase of commerce and manufactures has in fact produced such a demand; but as this last event (distinguishable by its gradual progress) does not appear, from the numbers in the account, to have influenced the coinage in any great proportion, I shall disregard it in the present rough statement. With this liberty, we may proceed to remark, 1st. That as most of the old pieces disappeared during the middle term of time the number of nineteen, or say twenty millions must nearly represent the whole of our gold money. 2d. That the national loss by wear in the first period, when the gold was old and smooth, reckoned at 1 per cent. on the sum recoined, was 3708 l. per annum; and in the latter period 8943 l. per annum: and 3d. That the whole national stock of gold coin, under the regulations and figure of the last period, wears out and is recoined every eleven years. The account of coinage is to be found in the *Report of the Lords' Committee of Secrecy*, printed April 28, 1797.

bination of both methods is necessary. If a coin be struck with indentations or parts depressed beneath the common surface, and in these there be prominent objects or designs, not more elevated than that surface, the general advantage, with regard to wear, will approach towards that of the plain surface itself, and the impression will be at least as difficult to imitate, if not more so than that of a design rising totally above the common surface \*. The late copper coinage of pieces of one and of two pennies are of this kind †.

To sum up the foregoing conclusions in a few words, we may remark, that, 1. The State is unable (from the natural impracticability of the thing) to appoint two distinct articles of commerce as the circulating mediums of exchange. 2. The measure of value or legal tender ought to consist in the metal which bears the highest price, namely, gold. 3. Coin of silver and copper are required for smaller fractions than the actual subdivisions of the gold coin, but should be optional in the receipt for any larger sums. 4. These last-mentioned coins ought to represent a value in gold equal to their own quantity of metal, at the highest (or perhaps medium) market price added to the charge of fabrication. 5. No sufficient reason has yet been given, to shew that the standard of gold coin should be changed in order to render it more durable. 6. The best figure of coin is a short cylinder or flat round plate; and 7. The distinctive marks or impressions should be made neither altogether hollow nor altogether in relief, but by a combination of both forms, so as to leave a flat bearing surface on each side.

## V.

*An easy Method of cleaning and bleaching Copper-Plate Impressions or Prints. Extracted from a Letter of Sig. GIO. FABBRONI, Subdirector and Superintendant of the Royal Cabinet of Philosophy and Natural History of his Royal Highness the Grand Duke of Tuscany, to Sig. D. LUIGI TARGIONI at Naples ‡.*

SINCE the happy invention of engraving in copper, which no doubt owes its origin to the revival of the art of chasing and ornamenting plate, collectors have availed themselves of this means to accumulate and preserve copies of the most valuable pictures and drawings. This object of research becomes every day more prevalent, and prints of the early and most celebrated masters are now sought for with the utmost avidity.

Ancient prints are valuable, not only for their own intrinsic merits, but as monuments of

\* Few coins have been made of this figure. The Chinese coin of mixed copper called the cash is the most remarkable, and perhaps the only one of extensive circulation.

† Of copper, by M. Boulton, Esq. for Government. The penny is rather more than 1.4 inch in diameter, and about 0.13 inch thick at the edge, and weighs 1 oz. avoirdupois. A circular part of the face on each side rather more than 1.1 inch in diameter is depressed by the stroke of the dye, in one of which is seen the head of the King in relief, and in the other a figure of Britannia. Upon the prominent rim on one side are the words "Georgius III. D. G. Rex," and on the other "Britannia 1797" in sunken letters. The edge or cylindrical surface is plain. The two-penny piece resembles the penny; but its diameter is 1.4 inch: sunk face 1.25 inch, and weight 2 oz. avoirdupois wanting 20 grains in the piece before me. I suppose the average weight to be 2 ounces.

‡ Translated from the Italian. Communicated by Andrew Duncan, jun. M. D. of Edinburgh, who received it from the author. The original is inserted in the *Giornale Letterario di Napoli*, No. 83.

the history of the art. But their scarcity renders them still more valuable. Most of those which are still extant are defaced by negligence, during the time of their remaining suspended against walls exposed to smoke, vapor, and the excrements of insects. Collectors of prints have not, however, shewn the same partiality as antiquarians for the patina; but on the contrary they have sought and practised a method of clearing prints from these impurities.

This method consists in simple washing with clear water, or a ley made of the ashes of vine stalks or reeds, and lastly by a long exposure to the dew. Aqua fortis is also used for the same purpose, but with a degree of risque at least equal to its advantages. The ley dissolves not only the impurities but likewise the oil of the printing ink, and either discharges it totally, or leaves a cloudy appearance. The aqua fortis acts on the vegetable fibre, of which the paper itself is composed, and produces a dark colour, which cannot be removed by means of this liquid, but by an action which would considerably injure the paper itself.

The discovery of Priestley, of the fluid erroneously named by him, but since known by the name of oxygen; and the information we have obtained from Scheele, of the effects of its combination with muriatic acid, have led Berthollet to the useful application of its properties to the act of bleaching cloths, Chaptal to that of bleaching prints and books, and Giobert to the art of painting. But the method of making this preparation is too inconvenient for a mere amateur and collector of prints, and the oxygenated muriatic acid is not yet to be purchased ready prepared in Italy. It may not, therefore, be unacceptable to describe an easy method of effecting this purpose without the difficulties of chemical processes, and within the ability of any person to perform.

It is known that oxygen is abundantly contained in the combinations called metallic calces, though in a state of inactivity; and it is equally well ascertained, that these substances have a very strong attraction for it. On the other hand it is a fact, that some of the metallic calces of very moderate price are capable of easily yielding the whole or the greatest proportion of this constituent part. Manganese is not very well adapted for this purpose; but minium is much better. Nothing more is required to be done, but to provide a certain quantity of the common muriatic acid, for example, three ounces, in a glass bottle, with a ground-stopper, of such a capacity that it may be only half full. Half an ounce of minium must then be added; immediately after which the stopper is to be put in, and the bottle set in a cold and dark place. The heat, which soon becomes perceptible, shews the beginning of the new combination. The minium abandons the greatest part of its oxygen with which the fluid remains impregnated, at the same time that it acquires a fine golden yellow, and emits the detestable smell of oxygenated muriatic acid. It contains a small portion of muriate of lead; but this is not at all noxious in the subsequent process. It is also necessary to be observed, that the bottle must be strong, and the stopper not too firmly fixed, otherwise the active elastic vapor might burst it. The method of using this prepared acid is as follows:

Provide a sufficiently large plate of glass, upon which one or more prints may be separately spread out. Near the edges let there be raised a border of soft white wax half an inch high, adhering well to the glass and flat at top. In this kind of trough the print is to be placed in a bath of fresh urine, or water containing a small quantity of ox gall, and kept in this situation for three or four hours. The fluid is then to be decanted off, and pure warm water poured on, which must be changed every three or four hours until it passes limpid and clear.

clear. The impurities are sometimes of a resinous nature, and resist the action of pure water. When this is the case the washed print must be left to dry, and alcohol is then to be poured on and left for a time. After the print is thus cleaned, and all the moisture drained off, the muriatic acid prepared with minium \* is to be poured on in sufficient quantity to cover the print; immediately after which another plate of glass is to be laid in contact with the rim of wax, in order to prevent the inconvenient exhalation of the oxygenated acid. In this situation the yellowest print will be seen to recover its original whiteness in a very short time. One or two hours are sufficient to produce the desired effect; but the print will receive no injury if it be left in the acid for a whole night. Nothing more is necessary to complete the work, than to decant off the remaining acid, and wash away every trace of acidity by repeated affusions of pure water. The print being then left to dry (in the sun if possible) will be found white, clear, firm, and in no respect damaged either in the texture of the paper or the tone and appearance of the impression.

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## VI.

*On the Propagation of the Zebra with the Afs †.*

AN experiment was made in the year 1773 with a zebra, in the collection of the late Lord Clive, the result of which, though of considerable interest to the natural historian, is nowhere upon record in any public journal or printed work. A set of questions were proposed at the time of the event to Mr Parker ‡ by Sir Joseph Banks; which, together with the answers, he has at my request permitted me to make use of.

The zebra was first covered by an Arabian horse. For this purpose it was found necessary to bind her, and she shewed great disgust. As she did not conceive, an English afs was procured; to which she shewed a degree of aversion, scarcely if at all less than to the horse, and was subjected to him by the same means. The result of this trial not being more favourable than the other, recourse was had to the extraordinary expedient of painting another afs so as to resemble the zebra. Complete success attended this deception. When the animals were put together, the zebra at first appeared shy; but she received the embraces of the painted afs, and conceived. The offspring was a fine large male foal, which was just turned of six months old at the time of enquiry, namely, December 1773. It resembled both parents; the father as to make, and the mother as to colour; but the colour was not so strong, and the stripes on the shoulders were more conspicuous than on any other part. In answer to a question directed to that object, the relator states it as his opinion, that it would very probably propagate its species, as it did not appear at all like a mule.

In the course of the year after this information was received, his lordship died suddenly, and

\* As I have not repeated this process, I cannot estimate how far the presence of the lead may weaken the corrosive action of the acid on the paper; but I should be disposed to recommend a previous dilution of the acid with water. Whoever uses this process will of course make himself master of the proportion of water required to dilute the acid, by making his first trials with an old print of no value. N.

† Communicated by the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. &c. &c.

‡ He was either Steward or in some other confidential employ to Lord Clive.

the collection of animals was disposed of. Sir Joseph Banks was then absent from town; and upon his return he was prevented, by this circumstance, either from purchasing the animals or acquiring any further information respecting the foal. I have lately endeavoured to obtain some intelligence on this behalf among the dealers in animals, but hitherto without success.

## VII.

*On the Process of Bleaching with the Oxygenated Muriatic Acid; and a Description of a new Apparatus for Bleaching Cloths with that Acid dissolved in Water without the Addition of Alkali.* By THEOPHILUS LEWIS RUPP\*.

THE arts which supply the luxuries, conveniences and necessities of life have derived but little advantage from philosophers. A view of the history of arts will evince the justice of this observation. In mechanics, for instance, we find that the most important inventions and improvements have been made, not through the reasonings of philosophers, but through the ingenuity of artists, and not unfrequently by common workmen. The chemist in particular, if we except the pharmaceutical laboratory, has but little claim on the arts: on the contrary, he is indebted to them for the greatest discoveries, and a prodigious number of facts, which form the basis of his science. In the discovery of the art of making bread, of the vinous and acetous fermentations, of tanning, of working ores and metals, of making glass and soap, of the action and applications of manures, and in numberless other discoveries of the highest importance, though they are all chemical processes, the chemist has no share. But no branch of the useful arts is less indebted to him than that of changing the colours of substances. The art of dyeing has attained a high degree of perfection without the aid of the chemist, who is totally ignorant of the rationale of many of its processes, and the little he knows of this subject is of a late date. The process of dyeing the Turkey red has been known and practised from time immemorial by the most uncultivated nations, but its theory is not yet understood by philosophers. The manufacture of indigo and its application have been long known to the planter and the dyer; but it is not more than ten years since a true theory of them has been formed. The art of printing or topical dyeing is of the greatest antiquity; but the theory of this process, and of adjective colours in plain dyeing, was unknown till Mr. Henry developed it in the Memoirs of this Society†. The bleaching or whitening of vegetable substances has been long practised; but the knowledge of its theory could not be antecedent to the æra of pneumatic chemistry. We might even at this moment have been unacquainted with the cause of the destruction of the colouring matter of vegetable substances, if the discovery of the oxygenated muriatic acid, and its effects on colouring matter, had not pointed it out to us. For this discovery, and its inestimable advantages, the arts are indebted to the justly celebrated Scheele; and I am happy to pay this tribute to chemistry after the mortifying truths which I have stated above.

M. Berthollet lost no time in applying the properties of this curious and highly interesting substance to the most important practical uses. His experiments on bleaching with the oxygenated muriatic acid proved completely successful, and he did not delay to communi-

\* Manchester Memoirs, vol. v. part i.

† Manchester Memoirs, vol. iii.



state his valuable labours to the public. The new method of bleaching was quickly and successfully introduced into the manufactures of Manchester, Glasgow, Rouen, Valenciennes, and Courtray; and it has since been generally adopted in Great Britain, Ireland, France, and Germany. The advantages which result from this method, which accelerates the process of whitening cottons, linens, paper, &c. to a really surprising degree in every season of the year, can be justly appreciated by commercial people only, who experience its beneficial effects in many ways, but particularly in the quick circulation of their capitals.

Great difficulties for a time impeded its progress, arising chiefly from prejudice and the ignorance of bleachers in chemical processes. These obstacles were, however, soon removed by Mr. Watt at Glasgow, and by Mr. Henry and Mr. Cooper at Manchester. Another difficulty presented itself, which had nearly proved fatal to the success of the operation. This was the want of a proper apparatus, not for making the acid and combining it with water (for this had been supplied in a very ingenious manner by Mr. Watt and Mr. Berthollet \*), but for the purpose of immersing and bleaching goods in the liquor. The volatility of this acid, and its suffocating vapours, prevented its application in the way commonly used in dye-houses. Large cisterns were therefore constructed, in which pieces of stuff were stratified; and the liquor being poured on them, the cisterns were closed with lids. But this method was soon found to be defective, as the liquor could not be equally diffused; the pieces were therefore only partially bleached, being white in some parts and more or less coloured in others. Various other contrivances were tried without success, till it was discovered that an addition of alkali to the liquor deprived it of its suffocating effects without destroying its bleaching powers. The process began then to be carried on in open vessels, and has been continued in this manner to the present period. The bleacher is now able to work his pieces in the liquor, and to expose every part of them to its action without inconvenience. This advantage is unquestionably great; but it is diminished by the heavy expence of the alkali, which is entirely lost. It is moreover to be feared, that the alkali which is added to the liquor, though it does not destroy its power of bleaching, may diminish it; because a solution of the oxygenated muriatic acid of potash, which differs from the alkaline bleaching liquor in nothing but in the proportion of alkali, will not bleach at all. This is a well-known fact; from which we might infer, that the oxygenated muriatic acid will lose its power of destroying the colouring matter of vegetable substances in proportion as it becomes neutralized by an

\* M. Berthollet's apparatus, however, is too complex for the use of a manufactory; Mr. Watt's is better; but a range of four, five or six hogheads or rum puncheons connected with one another in the manner of Woulfe's distilling apparatus is preferable to either of them. Agitators on M. Berthollet's principle may be applied. The retort or matrafs should be of lead, standing in a water bath; its neck should be of sufficient length to condense the common muriatic acid, which always comes over; and it should form an inclination towards the body of the retort, so that the condensed acid may return into it. I beg leave to observe here, that I always found the liquor to be strongest when the distillation was carried on very slowly. I have also found that the strength of the liquor is much increased by diluting the vitriolic acid more than is usually done. The following proportions afforded the strongest liquor:

Three parts manganese.  
Eight parts common salt.  
Six parts oil of vitriol.  
Twelve parts water.

The proportion of manganese is subject to variation according to its quality.

alkali.

alkali. But as we should not content ourselves with inferences, however plausible, when the truth may be established by experiment, and as I thought the matter of sufficient importance, I made the following experiments on the subject :

I beg leave to premise, that in all these experiments I made use of one and the same acid, which was kept in a bottle with a ground glass stopper, and secured from the influence of light. The manner in which I made the experiments was simply this: I weighed first of all a bottle filled with the colouring substance which I meant to employ: I then weighed in a large and perfectly colourless bottle half an ounce of the acid, to which I immediately, but very gradually, added of the colouring substance contained in the former bottle till the acid ceased to destroy any more of its colour. The bottle with the colouring substance was then weighed again, and the difference between its present and original weight was noted. The same method was observed in all the experiments.

*Experiment I.* To half an ounce of oxygenated muriatic acid I added a solution of indigo in acetic acid \*, drop by drop, till the oxygenated acid ceased to destroy any more colour. It destroyed the colour of 160 grains of the acetite of indigo.

*Experiment II.* A repetition of Experiment I. The colour of 165 grains of acetite of indigo was destroyed in this experiment.

*Experiment III.* A repetition of Experiments I and II. The colour of 160 grains of the acetite was destroyed.

*Experiment IV.* To half an ounce of the oxygenated muriatic acid were added 8 drops of pure potash in a liquid state. This quantity of alkali was about sufficient to deprive the acid of its noxious odour. This mixture destroyed the colour of 150 grains of the acetite of indigo.

*Experiment V.* A repetition of Experiment IV. The colour of 145 grains of the acetite was destroyed.

*Experiment VI.* To half an ounce of the oxygenated muriatic acid, 10 drops of the same alkali were added. It destroyed the colour of 145 grains of the acetite of indigo.

*Experiment VII.* A mixture of half an ounce of the oxygenated acid, and 15 drops of the alkali, destroyed the colour of 120 grains of the acetite of indigo.

Though I had taken the precaution of avoiding the sulphuric acid for the reason stated in the foregoing note, I was not quite satisfied with these experiments, on account of errors which might have taken place through a double affinity. I therefore made the following experiments, in which I employed a decoction of cochineal in water instead of the acetite of indigo.

*Experiment VIII.* To half an ounce of the oxygenated muriatic acid, a decoction of cochineal was added till the acid ceased to act on its colour. It destroyed the colour of 390 grains of the decoction.

\* It has been usual to estimate the strength of the oxygenated muriatic acid by a solution of indigo in sulphuric acid. This method was inadmissible in these experiments on the comparative strength of the bleaching liquor with and without alkali; because the sulphuric acid would have decomposed the muriatic potash, and thereby produced errors. I therefore added to a solution of indigo in sulphuric acid after it had been diluted in water, acetite of lead, till the sulphuric acid was precipitated with the lead. The indigo remained dissolved in the acetic acid.

*Experiment IX.*

*Experiment IX.* A repetition of Experiment VIII. The colour of 385 grains of the decoction was destroyed in this experiment.

*Experiment X.* To half an ounce of the acid six drops of the liquid alkali were added. This mixture destroyed the colour of 315 grains of the decoction.

*Experiment XI.* Eight drops of the alkali were mixed with half an ounce of the acid. This mixture destroyed the colour of 305 grains of the decoction.

On a comparative view of the results of these experiments, it will appear that an addition of potash to the bleaching liquor impairs its strength considerably. This diminution of power and the expence of potash are a serious loss in an extensive manufacture. It would therefore be desirable to have an apparatus for the use of the pure oxygenated muriatic acid simply dissolved in water, which is at once the cheapest and best vehicle for it. This apparatus must be simple in its construction, and obtained at a moderate expence; it must confine the liquor in such a manner as to prevent the escape of the oxygenated muriatic acid gas, which is not only a loss of power, but also an inconvenience to the workmen and dangerous to their health; and it must at the same time be so contrived, that every part of the stuff which is confined in it shall certainly and necessarily be exposed to the action of the liquor in regular succession. Having invented an apparatus capable of fulfilling all these conditions, I have the pleasure of submitting a description of it to the Society by means of the annexed drawing.

*Explanation of Plate XI.*

Fig. 1, is a section of the apparatus. It consists of an oblong deal cistern, A B C D, made water-tight. A rib, E E, of ash or beech wood, is firmly fixed to the middle of the bottom C D, being mortised into the ends of the cistern. This rib is provided with holes at F F, in which two perpendicular axes are to turn. The lid, A B, has a rim G G which sinks and fits into the cistern. Two tubes H H are fixed in the lid, their centres being perpendicular over the centres of the sockets F, F, when the lid is upon the cistern. At I, is a tube by which the liquor is introduced into the apparatus. As it is necessary that the space within the rim G G be air-tight, its joints to the lid and the joints of the tubes must be very close, and, if necessary, secured with pitch. Two perpendicular axes, K, L, made of ash or beech wood, pass through the tubes H, H, and rest in the sockets, F, F. A piece of strong canvas, M, is sewed very tight round the axis K, one end of it projecting from the axis. The other axis is provided with a similar piece of canvas. N, are pieces of cloth rolled upon the axis L. Two plain pulleys, O, O, are fixed to the axes in order to prevent the cloth from slipping down. The shafts are turned by a moveable handle P. Q, a moveable pulley, round which passes the cord R. This cord, which is fastened on the opposite side of the lid (see fig. 2) and passes over the small pulley S, produces friction by means of the weight T. By the spigot and faulset V, the liquor is let off when exhausted.

Fig. 2. A plan of the apparatus with the lid taken off.

*The Manner of using the Apparatus.*

The dimensions of this apparatus are calculated for the purpose of bleaching twelve or fifteen pieces of 4-4 calicoes, or any other stuffs of equal breadth and substance. When the goods are ready for bleaching, the axis L is placed on a frame in a horizontal position, and one of the pieces N being fastened to the canvas M, by means of wooden skewers in the manner represented in fig. 1, it is rolled upon the axis by turning it with the handle P. This

operation

operation must be performed by two persons; the one turning the axis, and the other directing the piece, which must be rolled on very tight and very even. When the first piece is on the axis, the next piece is fastened to the end of it by skewers, and wound on in the same manner as the first. The same method is pursued till all the pieces are wound upon the axis. The end of the last piece is then fastened to the canvas of the axis K. Both axes are afterwards placed into the cistern with their ends in the sockets F, F, and the lid is put on the cistern by passing the axes through the tubes H, H. The handle P is put upon the empty axis, and the pulley Q upon the axis on which the cloth is rolled; and the cord R with the weight T is put round it, and over the pulley S. The use of the friction produced by this weight is to make the cloth wind tight upon the other axis. But as the effect of the weight will increase as one cylinder increases and the other lessens, I recommend that three or four weights be suspended on the cord, which may be taken off gradually as the person who works the machine may find it convenient. As the weights hang in open hooks which are fastened to the cord, it will be little or no trouble to put them on and to remove them.

Things being thus disposed, the bleaching liquor is to be transferred from the vessels in which it has been prepared into the apparatus, by a moveable tube passing through the tube J, and descending to the bottom of the cistern. This tube being connected with the vessels by means of leaden or wooden pipes provided with cocks, hardly any vapours will escape in the transfer. When the apparatus is filled up to the line a, the moveable tube is to be withdrawn, and the tube I closed. As the liquor rises above the edge of the rim G, and above the tubes H, H, it is evident that no evaporation can take place except where the rim does not apply closely to the sides of the box: which will, however, form a very trifling surface if the carpenter's work be decently done. The cloth is now to be wound from the axis L upon the axis K, by turning this; and when this is accomplished, the handle P and pulley Q are to be changed, and the cloth is to be wound back upon the axis L. This operation is of course to be repeated as often as necessary. It is plain, that by this process of winding the cloth from one axis upon the other, every part of it is exposed in the most complete manner to the action of the liquor in which it is immersed. It will be necessary to turn at first very briskly, not only because the liquor is then the strongest, but also, because it requires a number of revolutions, when the axis is bare, to move a certain length of cloth in a given time, though this may be performed by a single revolution when the axis is filled. Experience must teach how long the goods are to be worked; nor can any rule be given respecting the quantity and strength of the liquor in order to bleach a certain number of pieces. An intelligent workman will soon attain sufficient knowledge of these points. It is hardly necessary to observe, that if the liquor should retain any strength after a set of pieces are bleached with it, it may again be employed for another set.

With a few alterations, this apparatus might be made applicable to the bleaching of yarn. If, for instance, the pulley O were removed from the end of the axis K, and fixed immediately under the tube H; if it were perforated in all directions, and tapes or strings passed through the holes, skains of yarn might be tied to these tapes underneath the pulley, so as to hang down towards the bottom of the box. The apparatus being afterwards filled with bleaching liquor, and the axis turned, the motion would cause every thread to be acted upon by the liquor. Several axes might thus be turned in the same box, and, being connected with each other by pulleys, they might all be worked by one person at the same time; and as all would  
turn

turn the same way, and with the same speed, the skains could not possibly entangle each other.

In order to shew the usefulness of this apparatus still more clearly, I request the society to attend to the following statement of the expence of a given quantity of bleaching liquor, with and without alkali, but of equal strength.

<i>With ALKALI *.</i>					£.	s.	d.
80 lb. of salt, at 1½d. per lb.	—	—	—	—	0	10	0
60 lb. of oil of vitriol, at 6½d. per lb.	—	—	—	—	1	12	6
30 lb. of manganese	—	—	—	—	0	2	6
20 lb. of pearl-ashes, at 6d. per lb.	—	—	—	—	0	10	0
					<hr/>		
					2	15	0

But it appears by the foregoing experiments, that the liquor loses strength by an addition of alkali. The value of this loss, which on an average amounts to 15 per cent. must be added to the expence

<i>Without ALKALI.</i>					£.	s.	d.
80 lb. of salt	—	—	—	—	0	10	0
60 lb. of oil of vitriol	—	—	—	—	1	12	6
36 lb. of manganese	—	—	—	—	0	2	6
					<hr/>		
					3	3	3
					<hr/>		
					£.	2	5
					<hr/>		
					0	8	3

It appears from this calculation, that a certain quantity of the liquor for the use of any apparatus costs only 2*l.* 5*s.*; but, that the same quantity of the alkaline liquor costs 3*l.* 3*s.* 3*d.* which is 40 per cent. more than the other. The aggregate of so considerable a saving must form a large sum in the extensive manufactures of this country.

## VIII.

*Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURY, Professor of Experimental Philosophy at Modena, Member of the Italian Society of the Institute of Bologna, the Agrarian Society of Turin, &c.*

(Continued from Page 179 of the present Volume.)

*Proposition V.*

**I**N an additional conical tube, the pressure of the atmosphere increases the expenditure in the proportion of the exterior section of the tube to the section of the contracted vein, whatever may be the position of the tube, provided its internal figure be adapted throughout to the lateral communication of motion.

We have seen (Proposition III.) that the pressure of the atmosphere increases the expen-

\* I make no mention of the expence attending the preparation of the liquor, it being the same in both cases.

diture through additional tubes, whatever may be their position. We shall in the next place examine the mode of action by which the atmosphere produces this augmentation, and determine the result from its cause. I shall begin with the case best adapted to favour the action of the atmosphere, which is, that of conical diverging tubes of a certain form, which we have not yet considered.

Let the extremity AB, fig. 10, Plate VIII. of the tube AB EF be applied to an orifice formed in a thin plate. The part ABCD is nearly of the figure of the contracted vein, which form has been shewn to make no perceptible alteration in the expenditure (Experiment IV.) The fluid which issues through CD is disposed to continue its course in the cylindrical form CDHG. But if the lateral parts of the diverging conical tube CEG, DFH, contain a mass of the fluid at rest, the cylindrical stream CDHG will communicate its motion to the lateral parts (by Prop. I.) successively from part to part. And provided the divergence of the sides CE, DF, be such as is best adapted to the speedy and complete lateral communication of motion, all the fluid contained in the truncated cone CDEF will at length acquire the same velocity as that of the stream which continues to issue through CD. On this supposition, while the fluid stratum CDQR, preserving its velocity and thickness, would pass into RQTS, a vacuum would be formed in the solid zone Rm rSQnoT. Or otherwise, if it be supposed that the stratum CDQR, preserving its progressive velocity, should enlarge in RQTS, this cannot happen without its becoming thinner, and detaching itself from the stratum which follows, and by that means leaving a vacuum equal in magnitude to the zone last mentioned. A similar effect would take place through the whole of the tube CE; and if the quantity Cm be supposed to be invariable, the sum of all these void spaces will be equal to the solid zone VExGzYFH.

From this consideration, we see that the lateral communication of motion causes the same effect in a conical tube, whether horizontal or vertical, as gravity produces in the descending tube of Proposition IV. The atmosphere in this case also renders part of its pressure active on the reservoir, and at EF. If the action of the atmosphere upon the reservoir increases the velocity of the section CD, this velocity will communicate itself likewise to the whole fluid CDFE, and the tendency to a vacuum will take place as before; but since the action of the atmosphere is exerted equally at EF, it will take away at EF all the velocity which it added at CD; so that, being deducted from the same mass, and in the same time, at EF, the fluid will not cease to be continuous in the pipe. It is found by computation, that this will happen when the velocity of CD is increased in the ratio of  $CD^2$  to  $EF^2$ .

By applying the general laws of motion to the lateral fluid filaments of the stream which issues through AB, it is found that they tend to describe a curve which commences within the reservoir, for example, at A, and continues towards CSE. To determine the nature of this curve, it is requisite to know, and to combine together by calculation, the mutual convergency of the fluid filaments in AB, the law of the lateral communication of motion between the filaments themselves and their divergent progression from C to E. These combinations and calculations are perhaps beyond the utmost efforts of analysis. While the tube ABFE possesses a different figure from this natural curve, the results of experiment will always differ more or less from the theory.

*Experiment XIII.* The compound tube ABFE of the same fig. 10, having the following dimensions

dimensions in lines  $AB = EF = 18$ ;  $AC = 11$ ;  $CD = 15,5$ ;  $CG = 49$ ; and this tube being applied to the orifice P, fig. 1, under a charge of 32,5 inches, the four cubical feet of water were emitted in 27",5.

We have seen that, in the third experiment, under like circumstances, the orifice through a thin plate afforded four cubic feet of water in 41". The contracted vein was 0,64 of the orifice. Consequently, by following the enunciation of the theorem, the expence through the pipe ABF ought to be made in 26",24. The experiment falls short in the quantity 1",26.

*Experiment XIV.* Between the two conical tubes of the preceding experiment is interposed a cylindrical tube three inches long and 15,5 lines in diameter. The interposition of the cylinder between the two cones was as in fig. 13. This addition retarded the expenditure 1", the time now being 28",5.

*Experiment XV.* The charge of the reservoir being constantly 32,5 inches, the portion of the tube ABCD, fig. 11, had the same dimensions as before; the tube CDFE was 78 lines in length, and its diameter 23 lines. To this horizontal tube I added three glass tubes; the first DX at CD; the second NY at the distance of 26 lines from the first; and the third OZ at 26 lines distance from the second. The lower extremities of these three tubes were plunged in the mercury of the vessel Q. When the water was suffered to flow through the tube AEFB the mercury rose 53 lines in the tube DX; 20,5 in NY, and 7 in OZ. These quantities correspond with 62 inches height of water in DX; 24 inches in NY; and 8,1 in OZ. The expenditure of four cubic feet was effected in 25".

I cut off the portion PNFE of the tube, and the remaining pipe ABNP emitted the same quantity in 31".

In the truncated conical tube ACPBDN, the section PN is to the section of the contracted vein (namely 0,64 of the section AB) as 41" to 30". In the experiment with this last truncated tube the retardation is consequently no more than 1" less than the theory.

In the entire tube CDFE we have  $\sqrt{62 + 32,5} : \sqrt{32,5} = 41" : 24"$ . The difference of 38 inches elevation of water in the two tubes DX, NY, must arise from the motion of the fluid from C to P; it is 1-13th less than by the theory. The loss is successively greater in the two portions PQ, QE. The reason of this is, that the stream descends as it moves from CD, so that the lateral communication not being made uniformly through the whole of any one section, the different parts of the current acquire irregular motions, and even eddies within the tube; whence the jet comes forth by leaps and irregular scattering.—These uncertain motions cannot be reduced to the theory, and manifest themselves the more, the longer or the more diverging the sides of the tube. The effects consequently remain to be ascertained by experiment.

*Experiment XVI.* I constructed a tube CDFE as before, (fig. 11) 148 lines long, and 27 lines in diameter at EF, the rest of the apparatus being the same as in the foregoing experiment. The expenditure of four cubical feet was effected in 21"; the inequality and irregularity of motion in the stream were greater in this experiment than in the foregoing.

It was useless to prolong the tube CDFE beyond 148 lines; for the stream did not in that case fill the portion of tube added beyond that length, and the expenditure remained constantly at 21". This expenditure is nearly double what took place through the simple aperture in a thin plate; and it is the greatest I have been able to obtain by additional tubes, the axis of which had an horizontal position under a charge of 32,5 inches.

It is true, that by prolonging the tube C D F E to the length of 204 lines in the horizontal position, the four cubic feet flowed out in 19". But to obtain this effect, I found it necessary to fix a prominence within the tube at O, which forced the fluid to fly upwards, and by that means to fill the whole tube.

*Experiment XVII.* In this experiment the horizontal tube C D F E, fig. 11, was more divergent than in the foregoing trials. It was 117 lines long, and 36 lines in diameter at E F. The rest of the apparatus was the same as before. The expenditure was made in 28"; the stream did not fill the whole section E F. The result was the same when successive portions of the pipe were cut off, until C E was no longer than 20 lines, and the external diameter 18 lines. In this case the stream filled the pipe, and the expenditure was also made in 28".

When the length C E was 20 lines, its external diameter E F was increased to 20 lines. In this case the stream was detached from the sides of the tube, and the expence of four feet took place in 42 seconds, as in the VIth experiment.

These experiments teach us, that by varying the divergence of the sides of tubes, the lateral communication of motion has a minimum and a maximum of effect. The minimum is seen in the last experiment. It appears that the lateral communication ceases to produce its effect when the angle made by the sides of the tube with each other exceeds 16 degrees. The XIIIth experiment nearly determines the maximum of the effect when the same angle is about 3 degrees. These limits may also, perhaps, in a small degree depend upon some function of the velocity.

[To be continued.]

## IX.

### *An Account of the Means employed to obtain an overflowing Well.*

*By Mr. BENJAMIN VULLIAMY.\**

SIR,  
**P**ERMIT me, in compliance with your request, to give you a short account of the well at Norland-house, belonging to Mr. L. Vulliamy; a work of great labour and expence, executed entirely under my direction, and finished in November 1794.

Before I began the work, I considered that it would be of infinite advantage should a spring be found strong enough to rise over the surface of the well; and though I thought it very improbable, yet I resolved to take from the beginning the same precautions in doing the work as if I had been assured that such a spring would be found. But although this very laborious undertaking has succeeded beyond my expectation, yet, from the knowledge I have acquired in the progress of the work, I am of opinion, that it will very seldom happen that the water will rise so high; nor will people, I believe, in general, be so indefatigable as I have been in overcoming the various difficulties that did and ever will occur in bringing such a work to perfection.

In beginning to sink this well, which has a diameter of four feet, the land springs were stopped out in the usual manner, and the well was sunk and steined to the bottom. When the workmen had got to the depth of 236 feet, the water was judged not to be very far off,

\* In a letter to the Right Honourable Sir Joseph Banks, Bart. K. B. P. R. S. Phil. Trans. 1797.



and it was not thought safe to sink any deeper. A double thickness of steining was made about six feet from the bottom upwards, and a borer of  $5\frac{1}{4}$  inches diameter was made use of. A copper pipe of the same diameter with the borer was driven down the bore hole to the depth of 24, at which depth the borer pierced through the rock into the water; and by the manner of its going through it must probably have broken into a stratum containing water and sand. At the time the borer burst through, the top of the copper pipe was about three feet above the bottom of the well: a mixture of sand and water instantly rushed in through the aperture of the pipe. This happened about two o'clock in the afternoon, and by twenty minutes past three o'clock the water of the well stood within 17 feet of the surface. The water rose the first 124 feet in eleven minutes, and the remaining 119 feet in one hour and nine minutes. The next day several buckets of water were drawn out, so as to lower the water four or five feet; and in a short time the water again rose within 17 feet of the surface. A sound line was then let down into the well in order to try its depth. To our great surprise, the well was not found by 96 feet so deep as it had been measured before the water was in it; and the lead brought up a sufficient quantity of sand to explain the reason of this difference, by shewing that the water had brought along with it 96 feet of sand into the well. Whether the copper pipe remained full of sand or not, is not easy to be determined; but I should rather be inclined to think it did not.

After the well had continued in the same state several days, the water was drawn out so as to lower it eight or ten feet; and it did not rise again by about a foot so high as it had risen before. At some days interval, water was again drawn out, so as to lower the water as before; which at each time of drawing rose less and less, until after some considerable time it would rise no more; and the water being then all drawn out, the sand remained perfectly dry and hard. I now began to think the water lost; and consequently that all the labour and expence of sinking this well, which by this time were pretty considerable, had been in vain. There remained no alternative but to endeavour to recover it by getting out the sand, or all that had been done would be useless; and although it became a more difficult task than sinking a new well might have been, yet I determined to undertake it, because I knew another well might also be liable to be filled with sand in the same manner that this was. The operation of digging was again necessarily resorted to, and the sand was drawn up in buckets until about 60 feet of it were drawn out; consequently there remained only 36 feet of sand in the well: that being too light to keep the water down, in an instant it forced again into the well with the same violence it had done before; and the man who was at the bottom getting out the sand was drawn up almost suffocated, having been covered all over by a mixture of sand and water. In a short time the water rose again within 17 feet of the surface, and then ceased to rise as before. When the water had ceased rising the sounding line was again let down, and the well was found to contain full as much sand as it did the first time of the water's coming into it.

Any further attempt towards recovering the water appeared now in vain; and most people would, I believe, have abandoned the undertaking. I again considered, that the labour and the expence would be all lost by so doing; and I determined without delay to set about drawing the sand out through the water by means of an iron box<sup>n</sup> made for that purpose, without giving it time to harden as before. The labour attending on this operation was very great, as it was necessary continually to draw out the sand, and thereby to prevent the  
sand

sand from hardening. What rendered this operation the more discouraging was, that frequently after having drawn out 6 or 7 feet of sand in the course of the day, upon founding the next morning the sand was found lowered only one foot in the well, so that more sand must have come in again. This, however, did not prevent me from proceeding in the same manner during several days, though with little or no appearance of any advantage arising from the great exertions we were making. After persevering however for some considerable time, we perceived that the water rose a little nearer the surface, and I began to entertain some hopes, that it might perhaps rise high enough to come above the level of the ground; but when the water had risen a few feet higher in the well some difficulties occurred, occasioned by accidental circumstances, which very much delayed the progress of the work; and it remained for a considerable time very uncertain whether the water would run over the top of the well or not.

These difficulties being at length surmounted, we continued during several days the process before mentioned of drawing out the sand and water alternately; and I had the satisfaction of seeing the water rise higher and higher, until at last it ran over the top of the well into a temporary channel that conveyed it into the road. I then flattered myself, that every difficulty was overcome; but a few days afterwards, I discovered, that the upper part of the well had not been properly constructed, and it became necessary to take down about ten feet of brick work. The water, which was now a continued stream, rendered this extremely difficult to execute. I began by constructing a wooden cylinder 12 feet long, which was let down into the well, and suspended to a strong wooden stage above, upon which I had fixed two very large pumps of sufficient power to take off all the water that the spring could furnish at 11 feet below the surface. The stage and cylinder were so contrived as to prevent the possibility of any thing falling into the well; and I contrived a gage by which the men upon the stage could always ascertain to the greatest exactness the height of the water within the cylinder. This precaution was essentially necessary, in order to keep the water a foot below the work which was doing on the outside of the cylinder to prevent the new work from being wetted too soon. After every thing was prepared, we were employed eight days in taking down 10 feet of the wall of the well, remedying the defects, and building it up again; during which time ten men were employed, five relieving the other five, and the two pumps were kept constantly at work during one hundred and ninety-two hours. By the assistance of the gage, the water was never suffered to rise upon the new work until it was made fit to receive it. When the cylinder was taken out, the water again ran over into the temporary channel that conveyed it into the road.

The top of the well was afterwards raised 18 inches, and constructed in such a manner as to be able to convey the water five different ways at pleasure, with the power of being able to set any of these pipes dry at will, in order to repair them whenever occasion should require. The water being now entirely at command, I again resolved upon taking out more sand, in order to try what additional quantity of water could be obtained thereby. I cannot exactly ascertain the quantity of sand taken out, but the increase of water obtained was very great; as instead of the well discharging thirty gallons of water in a minute, the water was now increased to forty-six gallons in the same time.

If you think, Sir, that the above account of an overflowing well, the joint production of nature and art, is deserving your attention, I feel myself much gratified in the pleasure I have  
in

in giving you this description of it; and have the honour of being with the greatest regard, &c. &c.

B. VULLIAMY.

Explanation of Plate XII.

Fig. I.

*a* Top of the well, with the water running over.

*bb* Ground line.

*c* Sand lying in the well.

*d* Copper pipe.

*ffffff* Steining of the well.

*gg* Double steining six feet from the bottom upwards.

*h* Stratum which the end of the copper pipe was driven into.

Fig. II and III.

Iron box for drawing sand out of the well, weighing about 60lbs. one foot square, and two feet nine inches long.

*a* Handle of the box.

*b* A flap or door which opens inwards by a joint at *c*. There is another door like this on the other side.

*c* The joint.

*d* The centre or pin of the joint.

SCIENTIFIC NEWS, AND ACCOUNTS OF BOOKS.

**I**N a Letter from Sig. Fabbroni to Sig. D. Luigi Targioni of Naples, inserted in the 85th No. of the Giornale Letterario di Napoli, I find an account of a very effectual composition for extinguishing fire, invented by M. Von Aken. The composition is,

Burnt alum	-	-	-	pounds	30
Green vitriol powdered	-	-	-		40
Cinabrese or red ochre in powder	-	-	-		20
Potters' clay, or other clay, also powdered	-	-	-		200
Water	-	-	-		630

With 40 measures of this mixture an artificial fire was extinguished under the direction of the inventor by three persons, which would have required the labour of 20 men and 1500 measures of common water. Sig. Fabbroni was commissioned to examine the value of this invention, and found in his comparative trials with engines of equal power, worked by the same number of men, that the mixture extinguished the materials in combustion in one sixth part less time, and three eighths less of fluid than when common water was used. He observed, as might indeed have been imagined from the nature of the material, that the flame disappeared wherever the mixture fell, and that the saline, metallic and earthy matters formed an impenetrable lute round the hot combustible matter, which prevented the access of the air, and consequently the renewal of the destructive process.

Sig. Fabbroni estimates the price of this composition at about one foldo (or halfpenny) per pound, but remarks, that it requires fewer hands, and affords the incalculable advantage of a speedier extinction of the fire. Whence he concludes, that it might be advisable to keep the ingredients ready powdered to mix with water.

I have given this abridged account, because it is evident that such inventions are worthy the attention of philosophers and economists, even though in the first applications they may prove less advantageous than their inventors may be disposed to think. It is scarcely probable that this practice in the large way, with an engine throwing upwards of 200 gallons (value about 3l. 10s.) each minute, would be thought of or adopted, or that a sufficient store of the materials would be kept in readiness; since at this rate the expenditure for an hour would demand a provision to the amount of 210l. sterling. But in country places the process, or some variation of it, might be applied with sufficient profit in the result; more especially if it be considered that common salt or alum, or such saline matter as can be had and mixed with the water, together with clay, chalk, or lime, ochreous earth or common mud, or even these left without any salt, may answer the purpose of the lute with more or less effect, and extinguish an accidental fire with much greater speed and certainty than clear water would do.

Mr. Park has circulated Proposals for publishing by Subscription (under the Patronage of the African Association) his "*Travels in the interior Parts of Africa, by Way of the River Gambia, performed in the Years 1795, 1796, and 1797, by the Direction and at the Expence of that Association.*" The Work will form One handsome Volume Quarto, and is expected to be ready for delivery early in the ensuing Season. One Guinea is to be paid at the time of subscribing, which it is expected will be the price of the Book; but as the charges are not at present ascertained, it is understood that a further payment of Half-a-Guinea will be expected, provided the Committee of the African Association shall certify that such demand is reasonable. Subscriptions are taken by G. Nicol, Bookseller, Pall-Mall.

Proceedings of the Association for promoting the Discovery of the interior Parts of Africa, containing an Abstract of Mr. Park's Account of his Travels and Discoveries, abridged from his own Minutes by Bryan Edwards, Esq. Also Geographical Illustrations of Mr. Park's Journey, and of North Africa at large. By Major Rennell. London: Printed for the Association. Quarto, 162 pages, with the following Maps by Major Rennell. 1. The Route of Mr. Mungo Park upon a large Scale. 2. The Lines of Magnetic Variation in the Seas round Africa; and 3. A Map showing the Progress of Discovery and Improvement in the Geography of North Africa. The Scale of this interesting Map, which comprehends the whole of Africa, from the Mediterranean Sea to the Equator, affords five Equatorial Degrees in two Inches. The Work has no Bookseller's name, and is not vendible.

The title-page renders it needless to repeat, that this book consists of two distinct works. Of the latter, which adds no small portion to the well-acquired fame of its author, I have not yet been able to satisfy myself that any abridgment can be offered to the Public without mutilations, which such a subject cannot endure. The reader has already been presented with the concluding chapter in our present number. The whole will be re-printed, together with the Maps, in Mr. Park's own work. Mr. Edwards's abstract of Mr. Park's Travels contains the following particulars:

From the house of Dr. Laidley at Pisania, on the banks of the river Gambia, but three degrees

degrees more westerly than the mouth of that river, Mr. Park departed to the eastward for the kingdom of Woolli with two Negro servants, himself on horseback and his servants each on an ass. He carried a small assortment of beads, amber, and tobacco, a few changes of linen and apparel, a pocket sextant, a magnetic compass, and a thermometer, together with two fowling-pieces, two pair of pistols, and some other small articles. At Medina, the capital of Woolli, he was hospitably received, and proceeded to the kingdom of Bondou, where the sovereign compelled him to surrender his coat, but nevertheless gave him five drams of gold dust and plenty of provisions. From the capital of Bondou he travelled through Kajaaga, which is bounded on the North by the Senegal river, where the French formerly had a small factory. The king commanded that he should be brought before him; but Mr. Park, who had been cautioned to avoid him, declined the interview, and escaped with the loss of about half his goods and apparel. Hence he was conducted to Kasson, under the protection of the nephew of the king of that district, where he was treated with great kindness and hospitality, but detained some weeks an account of the extreme curiosity of the natives to behold an European. Hence he proceeded still further eastward to Kemmoo, a large and populous town, since destroyed, but at that time the metropolis of an extensive kingdom called Kaarta. The king of this place, who received our traveller with great kindness, was at that time at war with the neighbouring nation of Bambarra, to the eastward, through which the Joliba or Niger river flows. Unfortunately for Mr. Park, it was the opinion of the sovereign of Kaarta, that he could not with safety pass into Bambarra immediately from his dominions; in consequence of which he advised him to shape his course to the northward into the territory of the Moors, called Ludamar, on the border of the Great Desert; through which territory he might continue his route easterly, and enter Bambarra on the northern side. By complying with these instructions, Mr. Park entered the frontier town of the Moors, called Jarra, about a degree to the northward of Kemmoo, near which he passed through the village of Simbing, whence the last dispatch of Major Houghton written with pencil was received.

Thus far our traveller had continued his journey to the eastward declining to the north, through six degrees of longitude with about a degree and a half of northing, the town of Jarra being placed in the map in about  $15^{\circ} 5'$  north latitude. The territory through which he passed was very generally clothed with native woods, and presented to the eye an appearance of great uniformity. In his progress eastward the country rose into hills, and the soil became various, but was every where fertile in such places as had been cleared. Bondou in particular is a land abounding with black cattle, sheep, goats, and poultry, with an excellent breed of horses, though the usual beast of burthen in all the Negro territories is the ass. Animal labour is no where applied to agricultural purposes. The land is cultivated by slaves, and affords plenty of rice and Indian corn. The Pagans make an intoxicating liquor from honey. The woods furnish a small species of antelope, of which the venison is highly esteemed. Among wild animals in these countries, the most common are the hyena, the panther, and the elephant. The latter is often destroyed for the sake of its teeth, but they have not yet tamed it for the service of man.

Besides the grains proper to tropical climates, the inhabitants cultivate in considerable quantities, ground nuts, yams, and pompions. They likewise raise cotton and indigo, and

have sufficient skill to convert these materials into tolerably fine cloth of a rich blue colour, and they make good soap from a mixture of ground nuts and a ley of wood ashes.

Their trade with the Whites is composed of slaves, gold-dust, ivory, and bees-wax. Their inland traffic consists chiefly of salt procured from the Moors, and warlike stores obtained from the European traders on the Gambia river. These articles are sold again to itinerant merchants called Slatees, who come down annually from distant countries, some of which are unknown even by name to the natives of the coast, with slaves and a commodity called shea-toulou, or tree-butter. This butter, in Mr. Park's opinion, besides the advantage of its keeping without salt the whole year, is whiter, firmer, and of a richer flavour than the best butter he ever tasted made from cow's milk. The tree which affords it very much resembles the American oak; and the nut, from the kernel of which the butter is prepared by boiling it in water, has somewhat the appearance of a Spanish olive, and is enveloped in a sweet pulp under a thin green rind. The growth and preparation of this commodity are among the first objects of African industry in the Eastern States, to which Mr. Park had access. The natives of the Gambia countries are also supplied, in considerable quantities, with sweet-smelling gums and frankincense from Bondou.

The government in all these petty States, though monarchical, is no where absolute. The chiefs form an aristocracy, which greatly restrains the powers of the Sovereign, and prevent him from declaring war or concluding peace without their consent. Every considerable town is governed by a magistrate, whose office is hereditary, and who collects the duties and customs from traders, which are paid in kind. The lower orders or bulk of the people are in a state of slavery or vassalage to individual proprietors; but the power of the master, as well with regard to treatment, as the disposal of the slave to a stranger, is limited with regard to natives. These indulgencies are not however extended to captives taken in war, or obtained in traffic.

To return to Mr. Park, whom we left at Jarra in the power of the Moors, a set of the worst fanatics, who consider it as a meritorious act to destroy a Christian. After a fortnight's waiting, permission arrived from Ali, the Moorish chieftain or king of the country, for him to proceed in his journey to the eastward. With much difficulty, danger and insult, he succeeded in passing through a district of near two degrees in length, and was within two days journey of the frontier town of Bambarra, when he was carried back to the Moorish camp by order of the chief. On his arrival he was thrown into confinement, in which he remained for eight or ten weeks exposed to daily insult, robbed of all his effects, in danger of perishing from the frequent want of food and every other necessary of life, with no other probable consequence to expect than ultimately to perish by the caprice or fanaticism of the barbarians around him. Here it was that he learned some particulars of the death of Major Houghton, who was seduced into the Desert by the Moors, robbed of all his property, and died either for want of sustenance, or by the violence of those who refused to supply that want. For the particulars of Mr. Park's adventures we must wait till his work appears. He succeeded in July 1796 in escaping from his oppressors. He was fortunate enough to procure his own horse, saddle and bridle, a few articles of his apparel, and his pocket compass, which he had concealed in the sand. The joy he experienced at his escape soon subsided into more anxious emotions. Alone in the woods of Africa, exposed to the ferocity of wild beasts, and the dread of meeting again with men more ferocious

cious than those animals ; sinking under the rage of hunger, and the still more intolerable torture of thirst, it was in vain that he chewed the bitter leaves of the trees, or climbed to look around him for a watering-place. A seasonable shower however saved him from perishing during the first night ; and after a weary course without food or water for the greatest part of the day following, he had the good fortune to meet with relief among a few huts of Negro shepherds. In this manner, and with no better dependance for support than the kindness of the most wretched of human beings, he proceeded on the object of his mission for fifteen days ; when, on the morning of the 16th, having been joined by some Negroes who were travelling to the town of Sego, he had the inexpressible satisfaction of beholding the object of his wishes, the long sought Niger glittering to the morning sun as broad as the Thames at Westminster, and flowing slowly from west to east through the middle of a very extensive town, which his fellow travellers told him was Sego, the capital of the great kingdom of Bambarra, which Major Rennell places in  $14^{\circ} 10'$  North latitude, and  $2^{\circ} 26'$  West longitude from Greenwich.

*[The remainder of this Abstract in our next.]*

A TABLE for reducing the Unities of the English Inch, Gallon, and Grain into  
Mètres, Litres, and Grammes.

Eng. Measures	Inches in Metres.	Cubic Inches in Litres.	Ale Gal- lons of 282 Inches in Litres.	Wine Gal- lons of 231 Inches in Litres.	Grains in Grammes.	Eng. Measures	Inches in Metres.	Cubic Inches in Litres.	Ale Gal- lons of 282 Inches in Litres.	Wine Gal- lons of 231 Inches in Litres.	Grains in Grammes.
1	0.0254	0.0161	4.6168	3.7821	0.0435	51	1.2950	0.8349	235.46	192.89	2.2207
2	0.0508	0.0327	9.2336	7.5643	0.0871	52	1.3204	0.8513	240.07	196.67	2.2642
3	0.0762	0.0491	13.850	11.346	0.1306	53	1.3458	0.8677	244.69	200.45	2.3078
4	0.1016	0.0655	18.467	15.128	0.1742	54	1.3711	0.8840	249.31	204.24	2.3513
5	0.1270	0.0819	23.084	18.911	0.2177	55	1.3965	0.9004	253.92	208.02	2.3948
6	0.1523	0.0982	27.701	22.693	0.2613	56	1.4219	0.9168	258.54	211.80	2.4384
7	0.1777	0.1146	32.318	26.475	0.3048	57	1.4473	0.9332	263.16	215.58	2.4819
8	0.2031	0.1310	36.931	30.257	0.3483	58	1.4727	0.9495	267.77	219.36	2.5255
9	0.2285	0.1473	41.551	34.039	0.3919	59	1.4981	0.9659	272.39	223.15	2.5690
10	0.2539	0.1637	46.168	37.821	0.4354	60	1.5235	0.9823	277.01	226.93	2.6126
11	0.2793	0.1801	50.785	41.604	0.4790	61	1.5489	0.9986	281.63	230.71	2.6561
12	0.3047	0.1965	55.402	45.386	0.5225	62	1.5743	1.0150	286.24	234.49	2.6996
13	0.3301	0.2128	60.018	49.168	0.5661	63	1.5997	1.0314	290.86	238.28	2.7432
14	0.3555	0.2292	64.635	52.950	0.6096	64	1.6251	1.0478	295.48	242.06	2.7867
15	0.3809	0.2456	69.252	56.732	0.6531	65	1.6505	1.0641	300.09	245.84	2.8303
16	0.4063	0.2619	73.869	60.514	0.6967	66	1.6758	1.0805	304.71	249.62	2.8738
17	0.4317	0.2783	78.486	64.297	0.7402	67	1.7012	1.0969	309.33	253.40	2.9174
18	0.4570	0.2947	83.102	68.079	0.7838	68	1.7266	1.1132	313.94	257.19	2.9609
19	0.4824	0.3111	87.719	71.861	0.8273	69	1.7520	1.1296	318.56	260.97	3.0044
20	0.5078	0.3274	92.336	75.643	0.8709	70	1.7774	1.1460	323.18	264.75	3.0480
21	0.5332	0.3438	96.953	79.425	0.9144	71	1.8028	1.1623	327.79	268.53	3.0915
22	0.5586	0.3602	101.57	83.207	0.9579	72	1.8282	1.1787	332.41	272.31	3.1351
23	0.5840	0.3765	106.19	86.989	1.0015	73	1.8536	1.1951	337.03	276.10	3.1786
24	0.6094	0.3929	110.80	90.772	1.0450	74	1.8790	1.2115	341.64	279.88	3.2222
25	0.6348	0.4093	115.42	94.554	1.0886	75	1.9044	1.2278	346.26	283.66	3.2657
26	0.6602	0.4256	120.04	98.336	1.1321	76	1.9298	1.2442	350.88	287.44	3.3092
27	0.6856	0.4420	124.65	102.12	1.1757	77	1.9552	1.2606	355.49	291.23	3.3528
28	0.7110	0.4584	129.27	105.90	1.2192	78	1.9805	1.2769	360.11	295.01	3.3963
29	0.7364	0.4748	133.89	109.68	1.2627	79	2.0059	1.2933	364.73	298.79	3.4399
30	0.7617	0.4911	138.50	113.46	1.3063	80	2.0313	1.3097	369.34	302.57	3.4834
31	0.7871	0.5075	143.12	117.25	1.3498	81	2.0567	1.3261	373.96	306.35	3.5270
32	0.8125	0.5239	147.74	121.03	1.3934	82	2.0821	1.3424	378.58	310.14	3.5705
33	0.8379	0.5402	152.35	124.81	1.4369	83	2.1075	1.3588	383.19	313.92	3.6140
34	0.8633	0.5566	156.97	128.59	1.4804	84	2.1329	1.3752	387.81	317.70	3.6576
35	0.8887	0.5730	161.59	132.38	1.5240	85	2.1583	1.3915	392.43	321.48	3.7011
36	0.9141	0.5894	166.20	136.16	1.5675	86	2.1837	1.4079	397.05	325.26	3.7447
37	0.9395	0.6057	170.82	139.94	1.6111	87	2.2091	1.4243	401.66	329.05	3.7882
38	0.9649	0.6221	175.44	143.72	1.6546	88	2.2345	1.4407	406.28	332.83	3.8317
39	0.9903	0.6385	180.06	147.50	1.6982	89	2.2599	1.4570	410.90	336.61	3.8753
40	1.0157	0.6548	184.67	151.28	1.7417	90	2.2852	1.4734	415.51	340.39	3.9188
41	1.0411	0.6712	189.29	155.07	1.7852	91	2.3106	1.4898	420.13	344.18	3.9624
42	1.0664	0.6876	193.91	158.85	1.8288	92	2.3360	1.5061	424.75	347.96	4.0059
43	1.0918	0.7040	198.52	162.63	1.8723	93	2.3614	1.5225	429.36	351.74	4.0495
44	1.1172	0.7203	203.14	166.41	1.9159	94	2.3868	1.5389	433.98	355.52	4.0930
45	1.1426	0.7367	207.76	170.20	1.9594	95	2.4122	1.5553	438.60	359.30	4.1365
46	1.1680	0.7531	212.37	173.98	2.0030	96	2.4376	1.5716	443.21	363.09	4.1801
47	1.1934	0.7694	216.99	177.76	2.0465	97	2.4630	1.5880	447.83	366.87	4.2236
48	1.2188	0.7858	221.61	181.54	2.0900	98	2.4884	1.6044	452.45	370.65	4.2672
49	1.2442	0.8022	226.22	185.33	2.1336	99	2.5138	1.6207	457.06	374.43	4.3107
50	1.2696	0.8186	230.84	189.11	2.1771	100	2.5392	1.6371	461.68	378.21	4.3543



Also in Decimals, at different Periods  
Money inferred therefrom. To which is  
added during the present Century, at shorter  
Periods.

Standard and Value.)						
Appreciation of Money, according to the Price of					Mean Appre- ciation by Interpolation.	
Year of our Lord.	Cent.	Twelve Miscella- neous Articles.	Meat.	Day Labour.	Mean of all.	A. D.
						1050 26
1050	10	42			26	1100 34
						1150 43
1150						1200 51
						1250 60
1250						1300 68
						1350 77
1350	00	56		75	77	1400 83
						1450 88
1450						1500 94
						1550 100
1550	00	100	100	100	100	1600 144
						1625 188
1600						1650 210
						1675 238
1625						1700 257
						1720 287
1650						1740 314
						1750 342
1675	216	239	166	188	210	1760 384
						1770 427
1700						1780 496
						1790 531
1720						1800 } 562
						nearly }
1740	97	434	266	250	287	
1760	203	492	400	275	342	
1780						
1795	226	752	511	436	531	

viz. 100.

Before *Præfatum*, 1st and 2d edit. *Liber Garderobæ*, in  
1299. Royal Household, in divers Reigns, from Edw. III. to  
King V. England, from the Year 1000 to 1765, by Mr. Com-  
brune, copied this Article from the Philosophical Transactions,  
M.D.C.



Year	Month	Day	Time	Place	Remarks
1892	Jan	1	10.0	10.0	10.0
1892	Jan	2	10.0	10.0	10.0
1892	Jan	3	10.0	10.0	10.0
1892	Jan	4	10.0	10.0	10.0
1892	Jan	5	10.0	10.0	10.0
1892	Jan	6	10.0	10.0	10.0
1892	Jan	7	10.0	10.0	10.0
1892	Jan	8	10.0	10.0	10.0
1892	Jan	9	10.0	10.0	10.0
1892	Jan	10	10.0	10.0	10.0
1892	Jan	11	10.0	10.0	10.0
1892	Jan	12	10.0	10.0	10.0
1892	Jan	13	10.0	10.0	10.0
1892	Jan	14	10.0	10.0	10.0
1892	Jan	15	10.0	10.0	10.0
1892	Jan	16	10.0	10.0	10.0
1892	Jan	17	10.0	10.0	10.0
1892	Jan	18	10.0	10.0	10.0
1892	Jan	19	10.0	10.0	10.0
1892	Jan	20	10.0	10.0	10.0
1892	Jan	21	10.0	10.0	10.0
1892	Jan	22	10.0	10.0	10.0
1892	Jan	23	10.0	10.0	10.0
1892	Jan	24	10.0	10.0	10.0
1892	Jan	25	10.0	10.0	10.0
1892	Jan	26	10.0	10.0	10.0
1892	Jan	27	10.0	10.0	10.0
1892	Jan	28	10.0	10.0	10.0
1892	Jan	29	10.0	10.0	10.0
1892	Jan	30	10.0	10.0	10.0
1892	Jan	31	10.0	10.0	10.0

1892	Jan	1	10.0	10.0	10.0
1892	Jan	2	10.0	10.0	10.0
1892	Jan	3	10.0	10.0	10.0
1892	Jan	4	10.0	10.0	10.0
1892	Jan	5	10.0	10.0	10.0
1892	Jan	6	10.0	10.0	10.0
1892	Jan	7	10.0	10.0	10.0
1892	Jan	8	10.0	10.0	10.0
1892	Jan	9	10.0	10.0	10.0
1892	Jan	10	10.0	10.0	10.0
1892	Jan	11	10.0	10.0	10.0
1892	Jan	12	10.0	10.0	10.0
1892	Jan	13	10.0	10.0	10.0
1892	Jan	14	10.0	10.0	10.0
1892	Jan	15	10.0	10.0	10.0
1892	Jan	16	10.0	10.0	10.0
1892	Jan	17	10.0	10.0	10.0
1892	Jan	18	10.0	10.0	10.0
1892	Jan	19	10.0	10.0	10.0
1892	Jan	20	10.0	10.0	10.0
1892	Jan	21	10.0	10.0	10.0
1892	Jan	22	10.0	10.0	10.0
1892	Jan	23	10.0	10.0	10.0
1892	Jan	24	10.0	10.0	10.0
1892	Jan	25	10.0	10.0	10.0
1892	Jan	26	10.0	10.0	10.0
1892	Jan	27	10.0	10.0	10.0
1892	Jan	28	10.0	10.0	10.0
1892	Jan	29	10.0	10.0	10.0
1892	Jan	30	10.0	10.0	10.0
1892	Jan	31	10.0	10.0	10.0

1892

# *Apparatus for bleaching.*

Fig. 1.

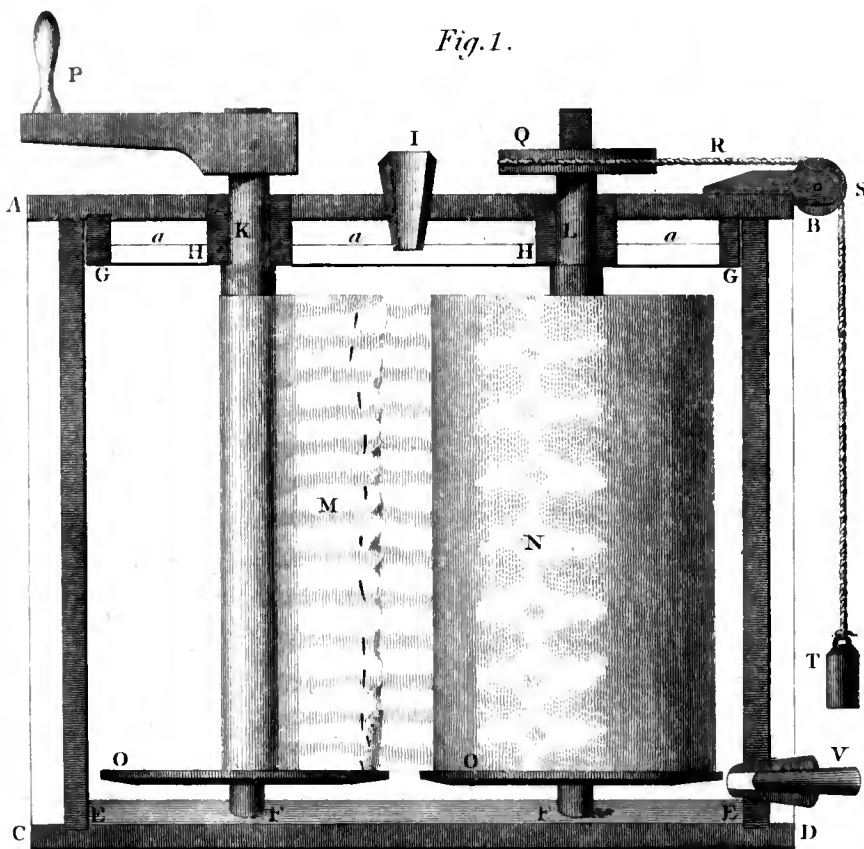
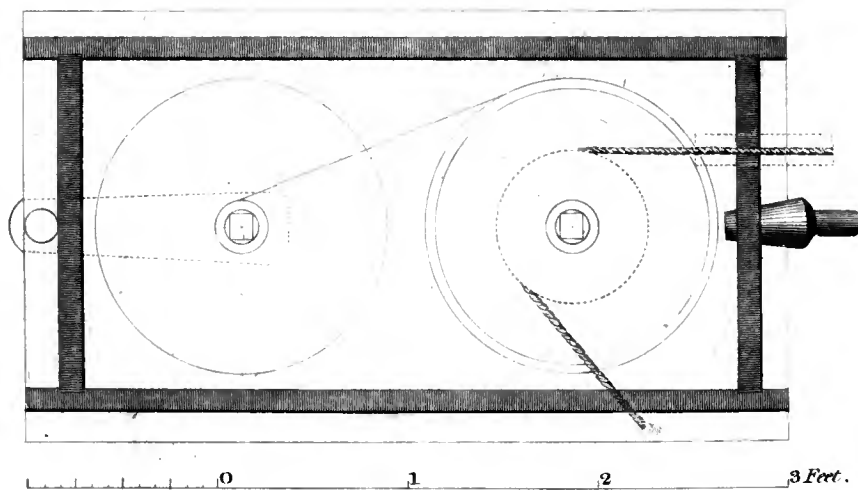


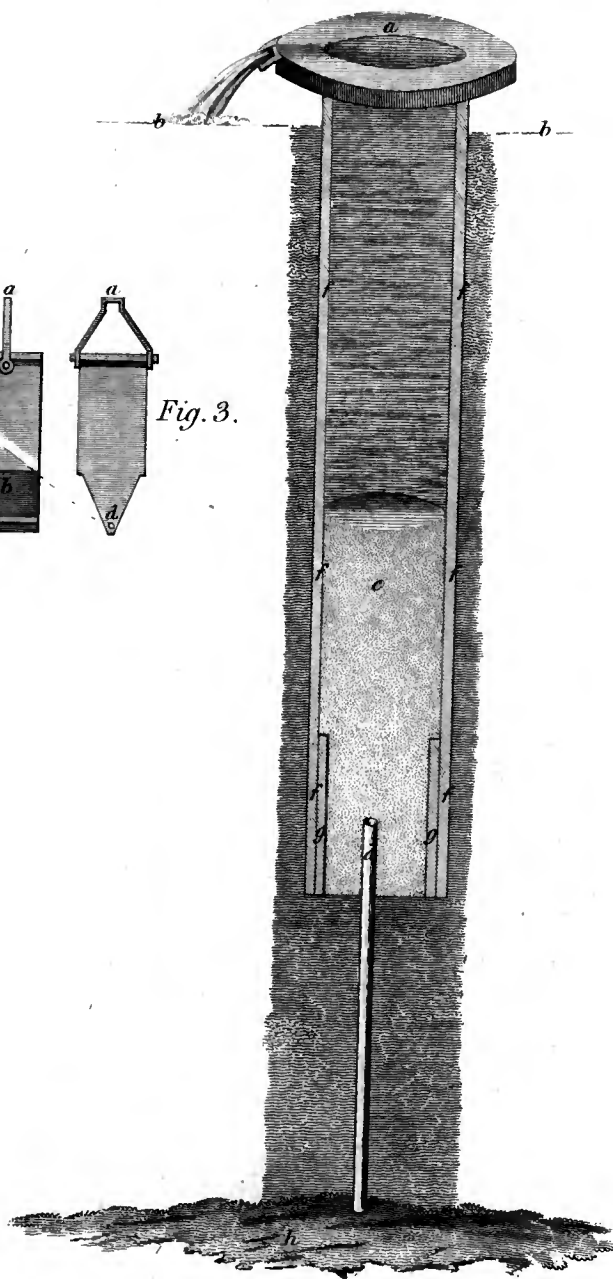
Fig. 2.





# *An Artificial overflowing Well*

*Fig. 1.*



*Fig. 2.*



*Fig. 3.*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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OCTOBER 1798.

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ARTICLE I.

*Curious Circumstances upon which the Vitreous or the Stony Character of Whinstone and Lava respectively depend; with other Facts. In an Account of Experiments made by Sir JAMES HALL, Bart. F.R. and A.S.S. Edin.\**

ON the 5th March, and 18th of June, Sir James Hall read to the Royal Society of Edinburgh an account of a series of experiments on whinstone and lava. These experiments, which relate to the theory of the earth published by the late Dr. Hutton, had been projected by Sir James Hall in 1790; at which time he read a paper to the society on the formation of granite and other unstratified substances. He had then begun a few experiments, but had been obliged by some circumstances to discontinue them. Having obtained, however, very promising appearances of success, they were again resumed, and carried into effect in the course of last winter †. In the execution of the experimental part, Sir James Hall acknowledges himself greatly indebted to the assistance of Dr. Kennedy.

The subject of the communication was suggested by some obvious and plausible objections which have lately been urged against Dr. Hutton's system. Whinstone or basaltic being considered in that system as having attained its present position in a state of igneous fusion, it has been alledged, as a sufficient refutation of this hypothesis, that whinstone, when made to undergo fusion in one of our furnaces, yields glass, a substance very different from the original stone; the formation of which cannot therefore be ascribed to the action of fire.

\* Abstract, by favour of the Author, from his Paper which will appear in the Edin. Transf. vol. v.

† Sir James Hall showed the result of his first successful experiment at a meeting of the Society on the 5th of February, 1798.

This difficulty had formerly occurred to Sir James Hall; who had endeavoured to obviate it by stating, that the mass during slow refrigeration in the bowels of the earth had undergone a change similar to that of glass into Reaumur's porcelain; and that by crystallization it had lost the vitreous, and assumed the stony character. The truth of this explanation has now been amply confirmed by these experiments, which comprehend seven different species of whinstone. Each of the original substances was reduced, by fusion and subsequent rapid cooling, to a state of perfect glass. This glass, being again placed in the furnace, was subjected to a second fusion. The heat, being then reduced to a temperature generally about  $28^{\circ}$  of Wedgwood, was maintained stationary for some hours; when the crucible was either immediately removed, or allowed to cool with the furnace. The consequence was, that in every case the substance had lost the character of glass, and by crystallization had assumed in all respects that of an original whinstone.

It must be owned, that in most cases the new production did not exactly resemble the particular original from which it was formed, but some other original of the same class; owing to accidental varieties in the mode of refrigeration, and to chemical changes which unavoidably took place during the process. In the case, however, of the rock of Edinburgh Castle, and of that of the basaltic columns of Staffa, the artificial substances bear a complete resemblance to their originals, both in colour and texture.

Sir James Hall conceives, that the objection to Dr. Hutton's system, mentioned above, is thus completely obviated; since the stony character of whinstone is shown to be the natural consequence of the slow refrigeration, which, according to that system, must have taken place in the bowels of the earth, as the substance of whinstone passed from a liquid to a solid state.

Experiments were made with equal success on lava. This class of bodies is known to possess the crystalline and stony character, in common with whinstone, and to resemble it so much, that in many cases the two substances cannot be distinguished by their external appearance\*. In others, however, certain circumstances occur by which they are characterized. But in consequence of fusion these distinguishing marks disappear; and the glass of a lava seems in no respect to differ from that of a whin. Sir James Hall therefore infers that the two substances are intrinsically the same, and ascribes their differences to the circumstances under which each class passed from a liquid to a solid state; the lavas having cooled in the open air, and the whins, according to Dr. Hutton, in the bowels of the earth. Six lavas, four of which † made part of undoubted currents of *Ætna* and *Vesuvius*, and broken from those currents by Sir James himself, were submitted to experiment. The glass produced by fusion and rapid cooling, yielded, when treated like the glass of whin, the same kind of crystallized, stony, or earthy masses, completely resembling an original whin or lava.

In the course of these experiments, the fusibilities of the various original substances, and of those artificially produced, were ascertained with much care by Wedgwood's pyrometer,

\* These observations are applicable to lavas in general, comprehending all those of *Ætna* and *Vesuvius*; but do not strictly apply to some of the lavas of the *Lipari Isles*, and of *Ischia*, which possess some curious and interesting properties to be mentioned in a future communication. H.

† No. 3, 9, 11 and 12 of the table.



and are stated in the annexed table, which may be depended upon, in every example, to within two, or at most three degrees. The artificially crystallized substances have been denoted, at Dr. Hope's suggestion, by the name of *crystallite*.

The various fusibilities stated in the table afford some conclusions of consequence. The whins submitted to trial are more refractory than the lavas; though their excess in this respect is not great, since the most fusible of the former class equal the most refractory of the latter. The glasses are all incomparably more fusible than the original stones. This last circumstance has long been known as a fact; but Sir James proposes to suggest the theory of it, and of all the peculiarities which occurred in those experiments, in a second communication which he means soon to lay before the Society.

It is observable, that the lava No. 12 is fusible at 18; that is, it resembles in this property the most fusible glasses. And it is in fact a glass; for, being lifted in a soft state from a flowing lava of Vesuvius by Sir James Hall, it quickly cooled, and has of consequence assumed the vitreous character in every respect: for, besides its easy fusibility, it possesses the shining surface and fracture of glass. This substance, being treated like the artificial glasses of whin and lava, crystallized like these, and assumed the character of a stony lava, both in texture and in difficult fusibility, since it softened only at 35°. Here then is a proof beyond dispute, that the stony character of a lava is occasioned solely by slow cooling.

Although the internal structure of lava was thus accounted for, yet Sir James was embarrassed with the state of its external surface; which, though cooled in contact with the open air, is seldom or ever vitreous, holding an intermediate station between glass and stone;—but this difficulty was removed by a circumstance which took place in the course of these experiments. It was found that a small piece of glass of any of the lavas, or of several of the whins, being introduced into a muffle, the temperature of which was at any point between the 20th and the 22d degree of Wedgwood's scale, the glass became quite soft in the space of one minute; but, being allowed to remain till the end of a second minute, it was found to have become hard throughout in consequence of a rapid crystallization, to have lost its character of glass, and to have become by 12 or 14 degrees more infusible, being unaffected by any heat under 30, though the glass had been fusible at 18° or at 16. This accounted for the scoria on the surface of lavas; for the substance even at the surface, being in contact with the flowing stream and surrounded with heated air, could not cool with excessive rapidity; and the experiment shows, that should any part of the mass, in descending heat, employ more than one or two minutes in cooling from 22 to 20, it would infallibly lose its vitreous character.

The internal phenomena of volcanos being thus explained by the properties of common fire, and the resemblance, or identity, of many lavas and of whinstone being thus established, Sir James Hall conceives, that the powerful arguments advanced by Dr. Hutton, to prove the igneous origin of whinstone and other mineral bodies, are very strongly corroborated; since these experiments show that these substances may have been formed by a simple extension of the same causes which continue at this day to agitate various parts of our globe.

Independently however of any allusion to system or to general theory, Sir James Hall flatters himself that these experiments may be of some importance by simplifying the history of volcanos; and above all by superseding some very extraordinary, and, he conceives,

unphilosophical opinions advanced with regard to volcanic heat, which has been stated as possessing very little intensity, and as acting by some occult and inconceivable influence, or with the help of some invisible agent, so as to produce liquidity without fusion. These suppositions, which have been maintained seriously by some of the most celebrated naturalists in Europe, have originated from the difficulty of accounting for the stony character of lavas, when compared with that of glass, which they assume in consequence of fusion in our furnaces. But now he hopes we may be relieved from the necessity of such violent efforts of imagination, since the phenomena have been fully accounted for by the simple though unnoticed principle of refrigeration, and have been repeated again and again with ease and certainty in a small chamber furnace.

## T A B L E.

The fusibilities, according to Wedgwood's pyrometer, stated in the following table, were ascertained by heating the substances in a muffle, in which they could be distinctly observed, while exposed to the action of the heat. A small piece of the substance to be examined being put into the muffle, a pyrometer-piece was placed close beside it, and the heat raised gradually. The substance was touched at intervals with a slender iron rod; and when found so soft as to yield easily to slight pressure, the pyrometer was withdrawn, and measured.

[ All the whins except No. 7 were taken from their original rocks in the neighbourhood of Edinburgh. ]

		Original substances softened.	Glass softened.	Crystallite softened.
No. 1.	Whin from a quarry on the Water of Leith,	40;45	17;15	33
No. 2.	Whin of the rock of Edinburgh Castle,	45	24;22	35
No. 3.	Whin of basaltic columns—Hill of Arthur's Seat,	55	18	35
No. 4.	Whin from the south side of Arthur's Seat, near Duddystone,	43	24	38
No. 5.	Stone of the nature of whin, found in large blocks in the bed of Water of Leith,	55	16	37
No. 6.	Whin of Salisbury Rock,	55	24	38;40
No. 7.	Whin of basaltic columns, Staffa,	38	—	—
No. 8.	Lava of Ætna, which destroyed part of Catania in 1669,	34	20	38
No. 9.	Lava near Piedmonte—Ætna,	32	18	36
No. 10.	Lava of La Motta di Catania—Ætna,	38;42	18	36
No. 11.	Lava of Torre del Greco—Vesuvius,	40	18	27;28
No. 12.	Lava lifted red hot from a flowing stream on Vesuvius, by Sir James Hall,	18	18	35
No. 13.	Lava from Iceland,	35	15	43

## II.

*On the Analysis of Pumice, which is found to contain Potash; and of Basaltes and Lava containing Soda. By Dr. KENNEDY\*.*

ON the 5th of February 1798, an analysis of pumice was communicated to the Royal Society of Edinburgh by Dr. Kennedy. By this analysis he shewed that the pumice contained potash as one of its component parts. The specimen analysed was of the common kind, having a fibrous texture and a silky lustre. By a heat of 60° of Wedgwood, it was converted into a kind of glassy enamel; and in a muffle, even at a heat of 35 or 40, was so far altered, that its fibrous texture could no longer be distinguished. Besides potash, it contained silice, argill, and a small quantity of iron; but no lime or magnesia.

Several other varieties of the common kinds of pumice used in the arts, were found as fusible as the specimen above mentioned, and gave the same kind of glassy enamel.

The result of the analysis, with respect to the earths and iron, corresponds very nearly with the result of Mr. Klaproth's, published in the 2d vol. of his *Beiträge*; only he did not find potash in the specimen he analysed; in which, however, there was probably some saline substance; for the same kind of pumice melted in the porcelain furnace of Berlin; in which a compound consisting only of silice, argill, and a minute portion of iron, would certainly not have melted. The heat of this furnace was 136 of Wedgwood.—See Klap. *Beytr.* vol. 2d, p. 88.

On the 6th of August Dr. Kennedy announced to the Society, that he had discovered mineral alkali in several varieties of basaltes or whinstone. He found by chemical analysis, that the alkali existed in these substances in a state of very intimate combination with their earthy bases; and that it was with difficulty separated, even by the strongest acids.

Dr. Kennedy also analysed a specimen of lava from *Ætna*, at the request of Sir James Hall, who thought it extremely probable that alkali would be found in lava, as well as in basaltes, on account of the great resemblance which these two substances have to each other, both in external appearance and in many chemical properties. The specimen was broken, by Sir James Hall and Dr. Hume, from the celebrated current of lava which in 1669 destroyed part of the town of Catania. It was found, like the basaltes, to contain soda.

One of the fixed alkalis, potash, has already been discovered in stony substances by the celebrated Mr. Klaproth, to whom the world is indebted for so many analyses performed with the greatest skill. The experiments above-mentioned show, that the other fixed alkali, soda, likewise exists in stony substances.

\* Abstract, by favour of the Author. This paper will appear in the 5th vol. of the *Edinburgh Transactions*.

## III.

*Experiments and Observations on the Preparation, and some remarkable Properties of the Oxygenated Muriat of Potash. By Mr. THOMAS HOYLE, jun. \**

HAVING an opportunity in preparing the oxygenated muriatic acid for the purpose of bleaching, by a small extension of the apparatus, to prepare likewise the oxygenated muriat of potash, and to make experiments on that substance, I have been induced to digest the most material facts and observations which occurred, and to lay them before the Society: especially as I do not find much on the subject in the writings of others, and as many have probably been deterred from the investigation by the exorbitant price of the article, and by some apprehensions of danger attending it.

A few experiments, which are not new, have been introduced, in order to bring under one point of view the principal chemical facts which relate to this salt. I have given in most cases an exact account of the quantities of the different ingredients composing the mixtures; and as persons not much accustomed to such experiments may be inclined to repeat some of them, I would caution them *not to use greater quantities than are here specified*, particularly where the terms violent detonation, explosion, &c. are employed.

I would not by any means wish it to be understood that I have exhausted the subject: many more experiments and much labour and assiduity are required, before the nature and uses of so active a substance can be fully ascertained.

I find it has been introduced into medicine with success; and I hope its good effects in that respect will not be frustrated by the high price of the article, as it may be procured at a much cheaper rate than it is commonly charged.

I. *On the Preparation of the Salt, and its Solution in Water and the Acids.*

Finding that a quantity of gas escaped occasionally from our apparatus for making the new bleaching liquor, more especially when the fire was not properly managed, or when by any other means a greater quantity of gas was produced than the liquor could absorb; I thought it would be useful to adapt to the large apparatus a smaller one, in which this superfluous gas might be condensed; as the escape of it was sometimes disagreeable to the workmen. This I did by filling an earthen-ware bottle with a strong solution of potash in water (consisting of about three pounds of alkali to the gallon), which I found entirely relieved us from the disagreeable smell we frequently experienced before, and at the same time yielded a considerable quantity of the oxygenated muriat. Though the production of the oxygenated muriat in this way be somewhat precarious, depending upon the management of the person who conducts the process (it being the bleacher's interest to condense the whole of the gas in the liquor he wants for his business); yet I think, if the portion which commonly escapes were thus disposed of, a considerable quantity of this salt might be made by bleachers with little additional expence, except what is incurred by the purchase of the alkali, and some more labour and attention. At some of my first trials, about two years ago, I found the gas which escaped from the materials of one distillation sufficient to saturate two gallons of the alkaline solution, from which I procured about six ounces, and

\* Manchester Memoirs, vol. v. part 1.

sometimes

Sometimes more of the salt, after being purified by several crystallizations. But having made some alteration in the apparatus, I now find that the same quantity of alkali may remain for three or four distillations before sufficient gas be furnished to form the salt; except the person employed be remarkably inattentive to his duty. I consider this as a valuable improvement, the making of the salt being only a secondary object. The salt was chiefly formed during the distillation. The alkali became warm toward the latter end of the process, especially if the absorption of gas was very rapid, a quantity of caloric being disengaged. In this case, a considerable part of the salt soon crystallized, on the lixivium being set in a cool place, and a great deal of gas appeared to escape; which on one occasion I collected, and found that it precipitated lime from its solution in water, and extinguished flame: and therefore, though it had a slight smell of the oxygenated muriatic acid gas, I believe that it consisted chiefly of carbonic acid, as the former occasions no precipitation of lime water, which the latter uniformly does. A glass jar containing 32 ounce measures of this gas, being left over water one night, was reduced to about one fourth its bulk. The gas that remained seemed to contain more oxygen than the air of the room; two measures of it, with one of nitrous gas, gave 1.53, whilst an equal quantity of common air gave 1.9.

Before any of the salt appeared to be formed in the alkaline solution, I have constantly observed a quantity of earthy matter to be precipitated. This was carefully separated from the salt, and, after being washed repeatedly in boiling water, was suffered to dry; but not having examined it with sufficient minuteness to say what it is, I shall content myself at present with stating some of its properties. It did not detonate with sulphur, and was totally or nearly insoluble in water. The sulphuric acid dissolved it, and gave evident signs of muriatic acid, which appeared to be slightly oxygenated. After being exposed to a red heat for half an hour, the above properties still appeared the same, except with the sulphuric acid. I thought the gas that was disengaged had more of the smell of simple muriatic acid gas, though along with it a little of the oxygenated gas might be perceived. The muriatic acid did not appear to dissolve any of this substance, either before or after its calcination. With the nitrous acid a strong smell of the oxygenated muriatic gas was produced. From a dram of this substance in an earthen retort exposed to a strong heat, about six ounce-measures of gas were produced, consisting of a mixture of carbonic and azotic gas, the latter of which was in the greatest quantity, forming by estimate about three fourths of the whole.

The form of the crystals that first appeared in the solution of alkali were quadrangular plates: what were afterwards formed, when the lixivium became cool, were needle-like, as were those that were produced by spontaneously evaporating the remainder of the ley: they appear to have the same property of detonating as the first. These different forms of crystals appeared on dissolving the salt in hot water, and, when cold, separating the salt, and suffering the water to evaporate spontaneously.

I frequently observed, that unless the alkali began to part with a considerable portion of gas without the admission of any from the apparatus, little or none of the oxygenated muriat was procured; and that as this gas (which I have before observed to be chiefly the carbonic acid) escaped, the crystallization took place, and increased or diminished according to the evolution of that gas. This I found uniformly the case, whether mild or caustic  
alkali

alkali was employed. A given quantity of the strong solution of potash appeared to produce more of this salt than the same quantity of a solution of pearlsh of the same specific gravity.

The remaining lixivium, on evaporation, did not yield this salt, though a muriat of potash was formed, that appeared to be considerably oxygenated: since, with the addition of the sulphuric or muriatic acid, it became a very powerful destroyer of vegetable colours; it would not detonate with sulphur, or inflame combustible substances, with acids; it was very soluble in water, much more so than the muriat first formed from the same alkali.

I may here remark, that I think the French chemists were right in calling the first salt the hyperoxygenated muriat, as the salt last mentioned is certainly oxygenated in some degree: however, in the following experiments I shall use the term oxygenated muriat, when speaking of the salt formed during the distillation, and on cooling the lixivium after being saturated with the gas.

*Experiment I.* One part of the oxygenated muriat of potash required about seventeen parts of water at the temperature of  $60^{\circ}$ , to dissolve it; whilst five parts of boiling water dissolved two of the salt. Repeated solutions did not appear to injure, but rather to increase, its detonating property. The crystals became much whiter; and a quantity of the earthy matter before mentioned was separated at every fresh crystallization.

*Experiment II.* A quantity of this salt was put into a bottle, and placed in a situation much exposed to the light: after being kept there more than twelve months, it did not appear to have lost any part of its detonating property. This fact is contrary to Chaptal's assertion, that the mere impression of light is sufficient to decompose it\*.

*Experiment III.* Water saturated with this salt was exposed to the light for several months, without appearing to be at all changed. It was put into a bottle with a ground stopper and tube, to which an apparatus was adapted to receive any gas that might come over; but no gas whatever was disengaged.

*Experiment IV.* Sixty grains of salt were fused by the heat of a lamp in a bottle with a ground stopper and tube. After having been kept in a fluid state for about half an hour, I found that it had lost two grains in weight, and that a small quantity of air was given out, which proved to be oxygenous by the test of nitrous gas. The salt which had been melted would still detonate with sulphur, &c. The loss of weight was, I am inclined to think, chiefly owing to the escape of the water of crystallization; for the salt when cool had lost its transparency.

*Experiment V.* From forty grains of the salt in an earthen retort, I procured by the application of heat about thirty-six cubic inches of oxygenous gas; the evolution of which was very rapid, and commenced as soon as the retort became slightly red. Forty grains exposed in a crucible to a strong red heat appeared, from the mean of two experiments, to have lost about seventeen grains in weight. The remaining muriat, being afterwards thrown into the sulphuric acid, produced a very strong smell of oxygenated muriatic acid; from which I inferred that the whole of the oxygen had not been expelled by the heat; whence the oxygenated muriat of potash may, I think, be stated to contain about half its weight of oxygen in a concrete state.

\* Elements of Chemistry, i. 250.

*Experiment VI.* Strong nitrous acid disengaged the oxygenated muriatic acid from this salt. During the solution of two or three grains of the oxygenated muriat in this acid, a grain or two of phosphorus was dropped into the glass containing the mixture; when a number of vivid flashes appeared in the liquor, darting forth at intervals for a considerable time. This is one of the most striking experiments I ever saw; but a little caution is necessary in performing it, the phosphorus being sometimes thrown out of the mixture\*.

*Experiment VII.* The muriatic acid dissolved this salt, a great deal of the oxygenated acid being given out. A few grains of the salt added to an ounce of the acid rendered it a very powerful destroyer of vegetable colours. This mixture may probably be used with advantage in taking stains of ink, &c. out of linen or cotton.

Phosphorus added to this acid along with the salt did not produce the same effect as with the nitrous acid; no light appearing, as in the last experiment.

*Experiment VIII.* On putting a little of the salt into the sulphuric acid, a violent crackling, or a great number of small explosions, took place, and a very strong smell of nitrous gas was produced; the mixture at the same time assuming an orange colour, which disappeared after it had stood a short time. A very small piece of phosphorus having been dropped on about two grains of the salt (previously thrown into the acid), an explosion immediately took place, which blew out a great part of the mixture upon my hand; an accident that might have proved serious if I had not had water near me.

*Experiment IX.* Finding a great quantity of gas to be disengaged from this salt by the sulphuric acid, which had a very strong smell of nitrous gas, I put forty grains of the salt into a glass retort, and poured upon it nearly an equal weight of sulphuric acid diluted with water. With the heat of a lamp the gas began to come over very rapidly, and was received in a glass jar placed in a basin of water. A considerable portion of it appeared to be absorbed by the water, which acquired a yellowish colour. This colour disappeared on standing a few days, and a brown matter was deposited, which being carefully collected and dried weighed one grain, and appeared to be manganese; for a little of it, being put into the muriatic acid, so far oxygenated it that it would destroy the blue colour of a diluted solution of indigo in the sulphuric acid. The precipitate before mentioned, that was first produced in the alkali employed, did not appear to have this effect. The quantity of this sediment that I had an opportunity of collecting was so small, that I could not try many other experiments with it: indeed I did not always succeed in procuring it; for I found that, unless the disengagement of the gas was very rapid, but little of it could be obtained.

*Experiment X.* On two drams of the salt in a glass retort, I poured an equal weight of sulphuric acid diluted with a little water, and adapted the retort to Woulfe's apparatus. The heat of a lamp was applied; and presently the gas began to escape, and was absorbed by the water in a considerable quantity; to which it communicated a yellowish colour, and a liquid began to trickle down the neck of the retort into the receiver. This had continued but a short time before a violent explosion took place, which broke the retort and two of the receivers to pieces, together with several other glasses which were on the table. This was several times repeated, but with more caution than before; and I always found,

\* This curious experiment was first noticed by J. Collier, and was communicated by him to the Society some time ago.

that when the mixture acquired a certain degree of heat an explosion certainly took place, except the retort had a pretty wide neck, and the neck was simply introduced into a receiver with a considerable opening in it without any lute; or put into water, as in the last experiment: and even in this case I would not advise so much of the salt to be used at one time as is here mentioned. The small quantity of acid I was able to collect in this way, by adapting a loose receiver, appeared to be a weak muriatic acid slightly oxygenated; it was of a dilute purple colour, which disappeared on its being exposed a short time to the light: a small piece of iron dropped into it caused it to become transparent immediately.

It was a matter of much surprise to me, to find so strong a smell of nitrous gas produced on decomposing this salt with sulphuric acid. Now, as nitrous gas consists of azot and oxygen, supposing this to be nitrous gas (for I do not assert it to be so, though I should think the smell in this instance an almost sufficient criterion), whence comes the azot? At first I thought it might come from a decomposition of the alkaline base of the salt; as some chemists have imagined the vegetable alkali to be composed of lime and azot: in that case, I expected the residuum would have been the sulphat of lime; but I found it to be chiefly sulphat of potash, with a little of the oxygenated muriat that remained undecomposed along with it. At present I shall not hazard any opinion respecting the origin of this nitrous smell; but hope some experiments I am at present engaged with will, if I can find time to prosecute them, throw some light upon this subject.

II. *On the Detonation and Inflammation of combustible Substances with the Salt produced by Friction and the Acids.*

THE detonating properties of this salt were tried with various substances in the following experiments: the different mixtures were intimately combined by gently rubbing them in a stone-ware mortar: after this was done, one smart stroke across the mixtures would cause the whole of some of them to explode at once, and others successively by repeating the friction. The sulphuric acid inflamed most of these mixtures of the salt with combustible substances: the nitrous acid also had the same effect with some of them.

*Experiment I. with Phosphorus.* Half a grain of this substance rubbed with the same weight of the salt produced violent explosion with flame. I apprehended it would be dangerous to use much greater quantities, as the phosphorus is frequently thrown out with violence before it is consumed. The sulphuric acid inflamed this mixture, as I have before stated.

*Experiment II. with Charcoal.* Two grains of salt with one of charcoal intimately mixed, and perfectly dry, produced by a smart stroke a strong flame without much report. The sulphuric and nitrous acids inflamed this mixture, the latter with most rapidity.

*Experiment III. with Pit-coal.* A grain of dry pit-coal rubbed with the same quantity of the salt produced sparks and some small reports. With half the quantity of coal the reports were much louder.

The sulphuric acid added to about twenty grains of the salt with ten of the coal, produced a bright red flame rising up to a considerable height.

*Experiment IV. with Sulphur.* A grain of the salt rubbed with half a grain of sulphur produced a very loud report, attended with flame and a strong smell of sulphureous acid. When the sulphur was reduced to a quarter of a grain, the explosion was not made at once.



as before, but successively. When the proportion of sulphur was increased to three fourths of a grain, it produced a very loud report, much the same as the first; and the whole appeared to explode at once. Equal parts of sulphur and the salt did not cause so strong reports as when a less quantity of sulphur was employed: this mixture exploded successively. The sulphuric and nitrous acids inflamed it.

*Experiment V. with Sulphuret of Potash.* One grain of the salt rubbed with the same weight of this substance produced a very loud explosion with flame. With half a grain of the sulphuret I thought the report fully as violent. A little of these mixtures melted over the fire had not the effect of the fulminating powder made with nitre. It only emitted a flash without any report, nor was I able to produce a fulminating mixture by varying the proportions of the salt, alkali and sulphur. The sulphuric or nitrous acids dropped on this mixture gave a very strong bright flame.

*Experiment VI. with Sulphuret of Mercury. (Cinnabar.)* Equal parts of this substance and of the salt detonated successively by friction, a grain of each being used. A change of proportion appeared to weaken the detonating property of the mixture. The sulphuric acid inflamed this mixture, but not so rapidly as in the last experiment. The nitrous acid did not inflame it.

*Experiment VII. with Sulphuret of Arsenic. (Orpiment.)* A grain or two of the salt rubbed with an equal weight of this substance produced little more than a flash; but a grain of the salt with half a grain of the sulphuret gave a strong report, though very little friction was used. Reducing the quantity of sulphuret to a quarter of a grain, the explosions were weak and successive. A larger quantity of this mixture than is mentioned above makes a report which is very unpleasant, with considerable flame. I was greatly surprised, the first time I made the experiment with two or three grains of the salt and a portion of the sulphuret, by their exploding in a most violent manner, though a very slight friction had been used. The sulphuric or nitrous acids gave a very strong flame the moment they were dropped upon this mixture.

*Experiment VIII. with Cotton-wool.* A small quantity of very dry cotton-wool was rubbed with a little of the salt; no detonation took place. The wool was afterwards dropped into the sulphuric acid, and took fire immediately; but the nitrous acid would not inflame it.

*Experiment IX. with Loaf-sugar.* One grain of this substance rubbed with two of the salt gave a number of successive reports. The sulphuric or nitrous acids dropped on this mixture instantly produced a strong flame ascending to a considerable height.

*Experiment X. with fixed and essential Oils.* A few drops of spermaceti oil rubbed with a grain or two of the salt produced a number of loud reports. The sulphuric acid inflamed this mixture; the nitrous acid did not.

Olive oil, the essential oils of rosemary, juniper, cloves, carraway, aniseed, cinnamon, nutmeg, amber, mint, and essence of lemon, were rubbed with the salt: all of them detonated successively, and such of the mixtures as were tried took fire with the sulphuric acid.

*Experiment XI. with Spirit of Turpentine.* A few drops of spirit of turpentine rubbed with a little of the salt detonated in much the same manner as the substances used in the last experiment. The sulphuric acid dropped on this mixture produced a strong flame, with a cloud of very black smoke.

*Experiment XII. with Camphor.* A little of this substance on being rubbed with a grain

of the salt produced a number of successive detonations. The sulphuric acid produced flame with some explosions.

*Experiment XIII. with Rosin.* One part of this substance with two parts of the salt detonated successively when well rubbed together. The sulphuric acid inflamed this mixture, but the nitrous acid did not.

*Experiment XIV. with Gum-Arabic.* The detonations were very slight. It was mixed with twice its weight of the salt. The sulphuric acid set fire to the mixture, but the nitrous acid would not.

*Experiment XV. with Prussian Blue.* No detonations whatever were produced by friction, nor did the acids inflame a mixture of this substance with the salt\*.

*Experiment XVI. with Indigo.* Half a grain of fine Spanish indigo rubbed with a grain of the salt detonated successively, like the mixture with rosin or gum. The sulphuric acid inflamed this mixture, but the nitrous acid did not.

*Experiment XVII. with Ether.* A few drops of ether on about two grains of the salt rubbed to a very fine powder produced no detonation by friction. The sulphuric or nitrous acids poured suddenly upon it produced flame.

*Experiment XVIII. with Iron-filings.* These alone rubbed with the salt produced no detonation by simple friction; but two grains of the salt, one grain of iron-filings, and half a grain of sulphur, being well rubbed together, about a quarter of a grain of this mixture exploded violently with friction. The sulphuric acid added caused a few sparks to appear; but the nitrous acid did not produce any. Varying the above proportions did not appear to improve the detonating property of the mixture.

*Experiment XIX. with Aurum Musivum.* Equal parts of this substance and of the salt detonated strongly with flame, on being rubbed together in an iron mortar: a very slight friction was necessary. The sulphuric acid gave a small flame, but with the nitrous I could not procure any.

From the foregoing experiments I think we may venture to conclude, that the oxygenated muriat of potash is equally harmless as common nitre; except it be brought into an intimate union with something that has a greater affinity with one of its constituent parts, than exists between those parts when combined in the salt, and that some combustible substance be present: but its oxygen being so easily disengaged renders a little caution necessary; and as the sulphuric or nitrous acids seem so readily to inflame many of the mixtures, I would not advise any person to make more of them than is necessary for immediate experiment. This precaution may prevent any unpleasant circumstance from accidental mixture with the acids, which appear to disengage a great part of the oxygen almost instantaneously.

I shall not say much about the theory of these detonations, none of the foregoing experiments having been so carefully conducted as to determine accurately what changes took place; yet I think we may attempt to explain some of them in the following manner: With phosphorus the oxygen seems to combine, and form phosphoreous acid gas, or phosphoric acid; with sulphur, the sulphureous acid gas, or sulphuric acid, according to the rapidity of

\* Chaptal (Elements of Chemistry, vol. ii. page 377,) says, "Prussian blue takes fire more easily than sulphur, and detonates strongly with the oxygenated muriat of potash. (Quære, Did he not make use of indigo here?)

the combustion ; with charcoal and other vegetable substances, the carbonic acid ; with sulphuret of arsenic there may be sulphureous acid gas and arsenic acid produced.

The sudden production of gas striking the surrounding air, is most probably the cause of the loud reports produced by friction, &c. agreeably to the conclusions of Berthollet ; and the muriatic acid may remain combined with the potash, and a portion of the combustible substance employed : but, when the sulphuric or nitrous acids are used, the muriatic acid is certainly disengaged.

Since the above experiments were made, I have found that a paper has been read before the National Institute of Paris, On detonation by concussion, by Citizens Fourcroy and Vauquelin \*. They there mention some of the mixtures I have described, and their inflammation with the sulphuric acid. They likewise notice, that very loud reports and sparks were produced, on a very small quantity of different mixtures being struck with a hammer on an anvil. This on trial I found to be the case ; and a little cotton-wool well impregnated with the salt being struck in that way immediately took fire. But, to get this to succeed, the salt and cotton should be perfectly dry : this is a necessary precaution in all experiments on the detonating property of this salt by friction, &c. In the paper above alluded to it is stated, that sugar, the gums, fixed and volatile oils, alcohol and ether, do not detonate or take fire by simple trituration : but the experiments I made seem not to agree with this assertion ; for all the above substances that I tried, except ether, detonated either more or less, on rubbing them briskly in a stone-ware mortar ; some of them required to be intimately mixed, as sugar and gum ; but others produced very loud reports, as when fixed and essential oils were used.

#### IV.

*Observations on the Natural History of Guiana. In a Letter from WILLIAM LOCHEAD, Esq. F.R.S. Edin. to the Rev. Dr. WALKER, F.R.S. Edin. Regius Professor of Natural History in the University of Edinburgh †.*

DEAR SIR,

ALLOW me at present to trouble you with a few general observations on natural history, which I had an opportunity of making while on a botanical excursion, with my friend Mr. Anderson, to the Dutch colony of Demerary. Guiana is a country but little known in Europe, though its animals and vegetables have added considerably to the catalogue of natural productions. It is not, however, the organic kingdom which I mean at present to touch upon ; all I aim at is, to give you some idea of the face of the country, as leading to the knowledge of its formation and present state. It is not a field for the mineralogist, as its interior is unexplored. But to the geologist, who wishes to trace revolutions of the latest date, it is not uninteresting to contemplate such a recent and singular country as Guiana.

I need not inform you, that under Guiana is comprehended all the coast of South Ame-

\* *Annales de Chimie*, tom. xxi. p. 235. *Nicholson's Chemical Journal*, i. p. 169.

† *Edinburgh Transactions*, iv.

rica, from the Amazons to the Oroonoko; that it trends nearly N. W. and S. E.; that it is in general a very low and flat country, especially the Dutch or westernmost part of it; and that it is watered by several rivers and creeks, which rise in a chain of mountains running nearly E. and W. and dividing Guiana from the inland parts of South America, which form the banks of the Amazons and its numerous branches.

*Coast.*—No coast can be more easy to make than that of Guiana. The changed colour of the water indicates soundings long before you make the land, and you may run on in seven fathoms before you can discover it from the deck. The bottom is at that distance a soft mud. All along the coast near Demerary, you have only about two fathoms at a good league from the shore; to leeward of Essequibo, it deepens still more gradually. In standing off or on five or six miles, you will hardly deepen or shallow the water as many feet. When a high sea sets in upon such a coast, it is easy to conceive that at a very considerable distance from the land it must be affected by the bottom. The interval betwixt wave and wave becomes more distinct. As they roll on in succession, the lower part is retarded, the upper surface accelerated: each billow of course becomes steeper and more abrupt, till at last it gradually ends in a breaker, when it has come to the depth of only a few feet. These *rollers* as they are called are the dread of seamen, especially betwixt Essequibo and Pomeroon, where the water is shallow, and the bearing of the coast very much north and south, and exposes it fully to the action of the trade winds. In small craft, those acquainted with navigation do not hesitate to run along the coast, even among the rollers themselves; but vessels drawing from eight to twelve feet water, especially if the swell be heavy and it falls calm, can hardly get off. If anchor and cables fail, they drift on till they are fast in the mud, and there they will continue sometimes for weeks together before they go to pieces. The sea water becomes exceedingly thick and muddy within a few leagues from the coast of Demerary, as much or more so than the Thames is at London. A stranger would naturally take this for the discharge of large flooded rivers after a rainy season. By and by I shall explain the true cause of it.

On approaching the continent of South America, a change on the face of the sky will strike the attentive observer. The clouds become less distinct from each other, and the intervals between them less clear. They are blended into one another as it were, and suffused more generally over the atmosphere. They appear to be surcharged with vapour, or to have a stronger disposition to deposit it.

There is a particular prevailing appearance of the heavens within the tropics, when you are at a distance from continents or very high islands, which has so often struck me that I wonder it has not been taken notice of. I call it a *tropical sky*, and thus describe it. The clouds in fine weather are in a single series or stratum, sailing away regularly with the trade-winds. They are small, and distinctly separated from each other. The intervals or sky above them of a clear azure. The lower surface of the clouds is perfectly horizontal. As the temperature is commonly very equal over the sea, the condensation takes place every where at an equal height from the surface of the water. In the clouds that are over head you cannot indeed perceive this; but it becomes more and more visible as the eye recedes from the zenith. The lower limb of each distant cloud appears perfectly level and well defined, brighter than the superincumbent part. At a distance nothing is to be seen but these limbs closer and closer in gradation, one behind the other; and the whole horizon round

round resembles the roof of a stage with an infinity of half dropt curtains as far as the eye can reach. In two voyages from Europe, I have met with this tropical sky as far north as Cape Finisterre. It came with a fair wind, which continued with us like a regular trade wind, accompanied with the same appearance of the clouds till we made the West Indies. In running down the trade winds, every one has an opportunity of verifying this description, and must be struck with the beauties which this sky presents at the setting of the sun. The inhabitants of the lower islands may also be well acquainted with it. In the higher ones, the attraction of the mountains ever forms sets of clouds of other appearances, as being produced by other causes. With our present knowledge of meteorology, hardly any other cause can be assigned for the phenomena above mentioned, than the constant equal temperature that every where prevails on the intertropical seas. One analogous fact however may be mentioned; the exceeding small range of the barometer in the torrid zone. Does the same cause regulate so exactly the height of the clouds, and maintain the uniform suspension of the mercury? We might almost suspect it did, were it not well known that the barometer varies as little upon continents, and in the vicinity of mountains in these regions as elsewhere, though the condensation of vapours is in such cases much more irregular. Upon the continent you will frequently observe this tropical sky also, especially in fine settled weather; but much more commonly you will find the sky there, and even before you make the coast, covered with heavy large dark clouds in some places, and in others at a greater height, the serene dappled sky so often seen in Europe.

*Winds.*—The *trade wind* generally prevails all the day long, and on the sea coast seldom fails even at night; but in less than fifty miles up the river it is a dead calm at night, and the breeze is not able to penetrate so far till towards noon. Still farther up we had whole days of a stark calm, and the heat very intense.

*Dews, fogs and temperature.*—The dews, following the law which they generally observe, are very heavy when and wherever there is but little wind, and the hotter the day and evening they fall the more copiously; they were of course more abundant up the river than near the sea-coast. The exhalations in the day-time from a hot and misty country covered with vegetables being very great, the condensation occasioned by the absence of the sun and the cold accompanying that condensation are in proportion. Near the coast, the diurnal difference of temperature is but trifling, the constant trade-wind preserving in the air nearly the same medium of heat as in the body of the ocean; but far up the river the range of the thermometer was very great. The heat of the day was intolerable. In the shade it was frequently above 90°. This, when there is no breeze, forces you into the woods for shelter. Towards evening it cools; during the night the cold increases, and is greatest about five in the morning. The thermometer would then be from 72° to 74°. The body of the river being large enough to retain its heat, the evaporation goes on from its surface through the night, and is condensed into thick fog, which hangs over it, and is seldom dispersed before eight or ten next day. While the air was as above in the morning at 72°, the water along side gave 80° to 83°, and seldom rose two degrees higher at noon. We had an opportunity of verifying an observation made by the few inhabitants who live far up the river Demerary, that, when it feels very cool in the morning before day-break, they are sure of fine weather; when, on the contrary, it feels warm, they expect rain. They sleep,

sleep in hammocks, and the houses they have are pervious enough to the air ; so they are sensibly affected by any change in its temperature.

*Seasons.*—As to the seasons, it is not an easy matter, from the accounts given by the colonists, to ascertain them exactly. All seem to agree, that since cultivation has been somewhat extended they are not so regular as before ; that the dry season encroaches on the rainy, and that during the latter they have often several dry days in succession. The account given by Dr. Bancroft was the one generally allowed ; that it is dry about the equinox, and rainy about the solstices ; that of consequence they have two wet and two dry seasons every year. We thought it difficult to reconcile this with the account given of the seasons of other countries in similar climates, and with what actually takes place in the Carib islands. I will give you my ideas on the subject. It is within the tropics a very general rule, that the vicinity of the sun brings the rainy season. To the northward of the line, therefore, this must be our summer months. It is another invariable law, that as in lunar influences, so in the change of seasons produced by the sun, some time is necessary after the maximum of the cause to produce the full effect. The highest tides are not till two or three days after the full and change. The greatest heat of the day is two hours afternoon, and the hottest months in Europe are July and August, not June, when the sun is highest. Among the West India islands the full effect of the sun's vicinity is still later. I have found August, and more especially September, to be the hottest months in the year, and they are accordingly the height of the rainy season. It begins thus : No sooner has the sun come to the northward, and begun to be vertical among the islands in April and May, than his force is felt, the sky is more disturbed, the wind is more frequently from the southward and in squalls, and now and then there are heavy showers. In June the same effects continue, and increase in July, when the proper rainy season may be said to begin, and continues in force more or less till the middle of October. August and September, with part of July and October, when these effects are at their greatest height, are styled the hurricane months, and by the French *Phivernage*. During them the full force of the great luminary which distributes light and life, however necessary, seems sometimes too much for nature. She is oppressed and sickens ; her respiration is disordered by intense heat ; sometimes calms, sometimes heavy squalls ; the agitated elements vent themselves in lightning, with thunder and torrents of rain, or are sometimes thrown out into those horrid convulsions, hurricanes, which seem to threaten instant dissolution. Guiana is happily free from these scourges of the Antilles. Their force has lately been partially felt at Tobago, which was thought beyond their reach. In Trinidad, the greatest storms they have hitherto experienced do not deserve the name of hurricanes ; and to the southward on the main of America they are utterly unknown. The difference then between Guiana and the islands is this : In the former, the rainy season sets in earlier, as indeed the sun is sooner vertical. Their principal rains are in the end of April, in May, June and July. They are also sooner over ; for August, September and October, and I believe part of July, are commonly fair weather. But again November, in part December, January and February, reckoned dry months among the islands, are in Guiana a second rainy season. The cause of this I take to be as follows : North-easterly winds pretty stiff, cold and bleak comparatively in these climates, are frequently among the islands during the winter months.

months. They are well known by the name of Norths. They are often accompanied with rain; but it is not very heavy, nor thought of consequence enough to give the denomination of a rainy season. These winds we know to reach as far as the coast of Guiana; and there I have reason to believe they are productive of more rain than in the islands. The face of a large continent, and its effects upon the atmosphere, may very probably make them give up more of their humidity than they do among the Antilles, though at the same time their force and bleakness may not be so much felt. If this conjecture hits the truth, the following ought to be the corollaries, and are left to future observation:—In this rainy season, when the sun is near the southern solstice, their rains will be with pretty steady northerly breezes on the coast. They may be of longer continuance at a time, but they will not be so heavy as those of summer, and they will be chiefly on the sea-coast, and probably will not extend a great way up the country. It remains even a query with me, whether the rain that accompanies the norths among the islands, especially those most remote from the line, be not generally in a greater proportion than is commonly supposed.

*Country.*—I will now endeavour to give you some idea of the face of the country. Though, as is well known, Guiana is flat and swampy, yet it affords to the attentive eye an interesting variety. The sea-coast is little if at all raised above the level of high water, and is continued at this level for many miles inland. It is properly an immense woody swamp, never dry in the driest season, covered with several feet of water in the wet. Next the shore, as far as the brackish water extends, it is covered with mangroves, which grow to a considerable height and form a thick shade. They are elevated on their branchy intermingled roots from the bare wet clay or mud, on which there is scarcely one herb or plant, but which seems to be all in motion from the prodigious number of crabs which make their holes in it. Further on, when the under water is fresh, you meet with a new set of vegetables, principally small trees, which from their situation are obliged to adopt the habits of mangroves, having the bottom of their trunks supported three or four feet above ground by their ramified roots. Several climbing plants are mixed with them. Arunis in great variety and profusion emerge from the water, or embrace the stems of the trees; and several broad-leaved plants of the hexandria and triandria classes assist the arunis in forming an herbage. In all this low part of Demerary there is not one tree of a large size, nor among them all above two or three species which can be applied to use as timber. Proceeding still up the river, its banks are found generally to raise themselves above the level of the water; and when you have gone up one tide (betwixt twenty and thirty miles), they are so high that there is no farther occasion for dams to keep the plantations from being overflowed at high water, as below: canals or ditches are sufficient to drain the land, which is still perfectly flat. The trees are here different in species and larger in size than below, and the woods are much more practicable. As they are drier, the ground has acquired a regular sort of surface, and there is neither that plexus of roots nor the same number of vines (the common name in the West Indies for all climbing plants) to entangle those who choose to traverse them. The soil here is generally a stiff, cold, reddish clay, mixed a-top with a portion of vegetable mould.

The sand-hills present to the admiring eye a scene very different from what it had been accustomed to below. The first you meet with upon the Demerary is upwards of thirty miles from the mouth of the river, and on the right hand ascending, or on its western shore.

There are of them further down in the country, but not close to the river side. This one is the extremity of a ridge which extends to the westward several miles. As you ascend the river you meet with many more of the same kind on both sides, whose direction seems likewise to be east and west, or nearly at right angles with the average course of the stream. They vary from 50 to 100, 150 or 200 feet of perpendicular height above the level of the river and the intervening flat country. Their breadth and extent vary sometimes only a few hundred paces, sometimes many miles. Their length is great; with some interruptions, I have reason to believe they are generally continued from one side of the colony to the other, only intersected in different places by the rivers and their branches. They consist of a pure siliceous sand, so white that it dazzles the eyes, commonly fine-grained and loose, but not unfrequently mixed with little strata of coarser pebbles, mostly quartz, and sometimes concreted into a proper sand-stone. In the last case, a black or reddish tinge is in many cases communicated to it from clay, decayed vegetables, or other extraneous matter. There is no regular stratification to be found in it, more than what is common to all sands the produce of depositions of different dates; and, as they are of different materials, thicker in one place, thinner in another, sometimes horizontal but oftener inclined, and convex or concave according to circumstances. We could meet with no appearance of shells or other marine productions, but in a few places pieces of broken vegetables buried in the sand where it was concreted. They were black, as all the fossil vegetables that I have ever seen in sand-stone. Upon and by the sides of the sand-hills grows the most valuable timber of these colonies. The trees there are of a good size, and very clear of obstructing underwoods or vines. The wallabba (*parivoa grandiflora* of Aublet); the spiri or green-heart (a new species of laurel); the coumarou or tonquæbean tree, *coumarouna odorata* of Aublet; the mora, valuable for boat timbers; and many others whose wood is equally hard and beautiful.

Continuing to ascend the river, the sand-hills become rather more frequent; but the intervals still remain a perfect flat, though now several feet above the level of the stream, and the soil is still a stiff clay. Hitherto the river is deep all over, generally from two to five fathoms; the bottom is mud or clay, and the shores on either side at low water covered with ooze. About 130 miles up, however, or just before it begins to shallow, the bottom is covered with banks of a hard white or brown sand. It was a problem for some time, whence all this sand originated in such a country. It was soon solved. Leaving here the vessel that had hitherto carried us, we proceeded in a canoe; and at about 160 or 170 miles distance from the mouth of the river we met with the first proper hills of solid materials. The nearest to us was a rock of granite projecting into the stream, whose direction it gave a change to at this place; and it served for a landing place to the highest piece of cleared land upon the river next to the post-holders. It was part of a low ridge of the same stone which crossed the country probably to Berbia or beyond it, and was succeeded by many other series of hills more inland, and, as far as we could examine them, of the same materials. The granite was both of the red and the gray kinds, but chiefly of the latter. A number of seams or dikes crossed it here and there in all directions, not distinctly separate, but firmly united to the rest, making as it were but one body with it, and consisting of the same materials differently modified. Their component parts were generally smaller; they were more compact and closer in the texture than what surrounded them; and where they had



had been equally exposed to the action of the weather, they appeared to have borne it much better than the surrounding granite. The origin of the sand was now accounted for. This stone, in some cases exceedingly firm and durable, is in others very liable to decay; and the wash of these enormous chains of hills was able to furnish abundance of such sand as we had met with below. The granite afforded many varieties; indeed, every shade from large and distinct grained to that whose component parts of felspar, schist, or quartz, were so small as to resemble pretty compact compound lavas, or some of our mixed whin-stones in Scotland. All these varieties would be found at no great distance from each other. I brought some specimens from Tiger's Berg, a hill about 500 feet perpendicular height, which have every appearance of having undergone the action of fire. They resemble half vitrified scoriæ, and would be taken for them, but that they were actually broken off from the granite, and discover all its parts in the fracture. The summit of this hill is irregular, with several pits and holes among the rocks. A little higher than it, and I suppose nearly about 200 miles from the sea, you meet what are called *the falls*. They are only five or six rapids within the space of a mile or two, formed by ledges of very close-grained gray granite that run across the river. There are breaks in each of them, through which the dextrous Indians are able in their light canoes to pass up at any season, even the driest; and when the river is swelled by the rains they become totally obliterated. Two days journey or two and a half above this is the great fall, where the stream comes over the face of a rock, as we are informed, twenty feet high.

*Savannahs.*—Savannahs, ever since the discovery of America, have been known to occupy large spaces in the southern parts of that continent. They are to be met with abundantly in Guiana, and are of two kinds very distinct from each other, the *wet* and the *dry*. Of the former many are extensive as the eye can reach—immense verdant plains occupying the whole face of a country, with or without a few straggling insulated patches of wood. In the dry season they appear meadows of long grass or reeds, and are seldom practicable for any distance, for the bottom is very rarely dry. In the wet season they are all one entire plain of water, over the surface of which the grass still rises, but which may be every where navigated in the courials or canoes. Towards the end of the drought the Indians set fire to them. The young growth which succeeds attracts the deer; and the native, on the return of the half-decay days, pursues them in his little bark across their former plains. The soil upon these savannahs can neither be very deep nor very good; yet water may be always commanded, and labour and industry might convert these deserts into rice fields. It is a question whether the days of slavery will ever see that event. The culture of this useful vegetable, which in the east has for ages been the standing food for millions, brings too moderate a return, at least in an infant colony, for the rapacious agricultural system of the West Indies.

The dry savannahs are neither so frequent nor so extensive, yet we have passed through some of them several leagues in circumference. They are formed along the flats on the top of the sand ridges, and covered by a very thin coat of verdure. They resemble exactly enough some of the bare moors in Scotland. Many beautiful plants of the class gynandria are their chief ornaments, as is also the orchis, which grows in similar situations with you. Some melastomas and more rhexias supply the place, and bear somewhat of the habit of the ericæ; for your sedums and saxifrages is the little *sauvagesia*; and, in hollows of the

same savannahs where moisture prevails, what I never could have expected to see within five degrees of the line, and not more than 50 or 100 feet above the level of the sea, the drosera lifts its humble head from a bed of the sphagnum palustre.

[To be continued.]

## V.

On the Sugar Maple. By Citizen TESSIER\*.

SUGAR is one of the most common products of the vegetable kingdom, and is found ready formed in a great number of species. It is obtained from the wild cherry-tree, (merisier), the poplar, the birch, the nut-tree, the pods of gleditzia, maize, the asclepias syriaca, &c.; but the tree which next to the sugar-cane affords this product in the greatest abundance and best quality is the sugar maple (*acer saccharinum*).

Many species of maple grow naturally in France, Germany, Switzerland, and England. The *acer opalus* is found particularly in Italy, and that which Linneus distinguishes by the name of *acer tataricum* in Asia. But America is the country of most of the sugar maples. It was from Quebec that Mr. Sarasin forwarded this tree to the *Jardin des Plantes*. Father Charlevoix, at the distance of a league and a half from Quebec, was regaled, to use his own expression, with the saccharine juice of maple. Kalm, at the post of Three Rivers, between Montreal and Quebec, saw the process of making sugar with the sap of the maple. The *acer saccharinum* is there so common that it is used for fuel.

A climate in which the winters are long and severe is best adapted to the sugar maple. I do not know whether it is found in a more northerly latitude than that of Canada. In that country, though situated in the latitude of about 44°, the cold lasts longer, and is more intense than in France, on account of the enormous masses of water, the woods and the mountains. Towards the south, the maple becomes very rare, so that few are seen beyond the lower part of Louisiana. Kalm has remarked, that they grow to a less height in the southern parts of the United States than in Canada, and that they do not grow in New Jersey and Pennsylvania, except on the sides of the Blue Mountains, and the steep banks of rivers exposed to the north. And even in this exposition they do not obtain more than one third or a quarter of the height they acquire in Canada. The intelligent Dupratz, author of the History of Louisiana, confirms what Kalm has advanced. The species of maple which we possess in France do not thrive excepting in places where the cold is of long duration. Kalm assures us, that being in the neighbourhood of Chester, a small town on the Delaware in Pennsylvania, he saw red maples on a marshy soil accompanied by the alder—an observation which points out the soil in which this tree ought to be planted. The sugar maple is also found in the State of Vermont in Kentucky, in the country beyond the Ohio, opposite to Pennsylvania and Virginia. According to the author of the American Geography, sugar is made from it in these countries; but it does not seem to be an object of great interest, as the author of that geography simply mentions the fact.

\* Inserted in the *Annales d'Agriculture*, and thence copied into *La Decade Philosophique*, &c. No. 97, an vi. from which last work the present translation is made.

Citizen Michau, nurseryman at Charles Town, has a considerable quantity near the banks of the Ohio in the state of Virginia, beyond the Apalachian mountains. They were planted in a good soil. In this country the maples have such a growth, that, if the author of the American Geography may be credited, sycamores are to be met with forty-four feet in circumference\*.

The Canadians have long been accustomed to use the juice of the maple as a refreshing beverage. When it issues out of the tree it is clear, whitish, and of a cool saccharine taste. By exposure to the sun in summer, it is converted into good vinegar. It is obtained by boring the trunk of the tree, taking care to direct the instrument upwards. The blade of a knife, or a piece of thin wood in the form of a ruler, is inserted to conduct the fluid to a vessel placed beneath for its reception: without this precaution, it would flow down the bark of the tree and be lost. Mr. Gaultier observes, that the perforation must be made into the proper ligneous circles, and that the saccharine juice is not to be obtained by making incisions in the middle bark or the liber, or at least that the quantity obtained will be very small. At the commencement of the thaws the sap flows abundantly for about three weeks; after which it thickens and entirely stops. The maples afford more sap the greater the quantity of snow has been, and the more rigorous the winter. The most favourable period is when the snow begins to melt, and the cold weather still continues. The flow is considerable in the spring, when the thaws are great and decided. The colder the nights, the greater the quantity of sap which flows on the following day. It seldom flows during the night, unless the weather be mild. If these observations had been made in all parts where there are sugar maples, they would afford reason to conclude, that the regular alternation of great cold during the night and very perceptible heat during the day-time, which takes place in the northern parts of America, contributes to elaborate the sap of the maple and render it sweet.

The juice of the maple is collected earlier or later according to the country. In the vicinity of Quebec, Three Rivers, and Montreal, it lasts from the middle of March to the middle of May, when the sun begins to have power; but near Lake Champlain it is collected from the middle of February to the early part of March. In this country the thaws commence about the end of January.

If we give credit to Mr. Gaultier, the French taught the savages to extract sugar from the sap of the maple; but if we depend on Kalm, the savages knew this art before the Europeans had discovered America, and the latter people have only followed the practice of the savages. Whatever may be the value of these two unsupported assertions, it is certain, that in order to extract this sugar the liquor is boiled over the fire, taking care to stir and skim it until it has obtained a very thick consistence. If it be kept too long over the fire, it acquires a taste of honey like melasses. This sugar becomes spontaneously purified. It is sometimes clarified with whites of eggs before it is sufficiently boiled, and after the clarification the ebullition is continued: when the boiling is sufficient, the sugar is poured into a vessel which gives it its form. In Canada, wherever the maples abound,

\* This appears to me to be greatly exaggerated; the largest American sycamores, according to the report of persons who have measured them, being no more than from twenty to twenty-four feet round. *Note of the Author.*

they make this sugar, as, in France, cherry brandy is made in such places as abound with cherries.

The first juice which is extracted from the maples in the spring is sweeter than that which flows at the end of the season, when it has the taste which in Canada is called the taste of the sap. This last, from which the sugar is more difficultly extracted, is kept in the state of syrup. It contains more sugar in proportion as the weather is colder. Trees of the same age do not afford the same quantity either of sap or of sugar, neither do the quantities of sugar in like quantities of sap agree. That of the old maples, which is less abundant, is more saccharine. The sap of maples which grow in a mountainous and stony soil is more saccharine than that of such trees as grow in low humid soils. This observation holds good with regard to all vegetable productions.

A tree of three or four feet in circumference may afford from thirty to sixty pints of liquor, and sometimes more; and a pound of sugar is about the quantity obtained from sixteen pints, that is to say, nearly three pounds of sugar per tree. Care must be taken to make the openings on the same side every year, in order that the tree may not die. The south or south-west is the proper side. Mr. Gaultier obtained in a quarter of an hour a pint of this saccharine fluid from a perforation three inches deep, made on this side of a tree four feet in circumference; at the same time that a perforation in the same tree on the north and north-east afforded him only a chopine or half pint in the same time. If the perforations be multiplied the tree becomes exhausted, and its old age is accelerated. It seems to me that it would be advisable to leave the tree untouched every second year, in the same manner as good economists suffer their mulberry trees to repose at like periods.

Father Charlevoix caused a refiner at Orleans to make a trial to refine the maple sugar. He found some difficulties in the attempt; but with attention these difficulties have been surmounted, for it is at present refined in America. This sugar, in the state we usually receive it from that country, may be kept a long time without alteration, as I have seen in a piece in the collection of Citizen Jussieu.

Gaultier and Kalm affirm, that the maple sugar of the savages of Canada is mixed with flour, whether to render it more nutritious or to augment the quantity; but this kind of sophistication, if the maple sugar should become an object of commercial interest, would be easily discovered. Such sugar is whiter than other samples not so adulterated.

The sugar of the maple is employed by the inhabitants of the remote parts of Canada, for the same uses as the sugar of the cane with us, because they are poor; and this sugar, though less pleasing to the sight and taste, costs them nothing but the trouble of tapping the trees and evaporating the fluid. This sugar is more difficultly soluble in water; and its sweetening quality, if I may use this expression after Kalm, is to that of the sugar-cane as one to two. The richer inhabitants of towns which by commerce have intercourse with the colonies where the sugar-cane is cultivated, prefer for their ordinary consumption the sugar of this last plant, and use that of the maple as an agreeable medicament. It is particularly recommended for coughs and colds, and its use is even prescribed for disorders of the lungs.

It is estimated that between twelve and fifteen thousand pounds weight of maple sugar is annually made in Canada. It is the product of four or five thousand trees. From this fact, supposing an arpent of land of a hundred perches, at eighteen feet the perch, planted

with maples in full growth, each affording sixty pints of liquor, the product would be six hundred and sixty-six pounds of unrefined sugar. For the arpent may support two hundred and twenty-two trees, placing the stems at twelve feet distance from each other.

The maples begin to afford a certain quantity of sugar at the age of 18 years, and cease to afford any at 60 or 70 years. In America, those trees are reckoned large which rise above 60 feet. There are some which are two feet in diameter. The wood of certain species is hard, and of a close texture. If they be tapped too soon, there is reason to fear that it may injure the growth, and render the product of sugar inconsiderable. It is therefore of advantage to determine the period of life at which the tapping ought to be commenced. And on this head it has been ascertained, that the best and most abundant produce is obtained from trees of the middle size, and that scarcely any is afforded by maples which are large and old.

The sugar maple has not hitherto been cultivated in France, but as an object of curiosity. Some enlightened cultivators, who endeavour to direct all their pursuits to objects of utility, have entertained the hope that some advantage might be derived from this tree. There are a sufficient number of these trees at present in France, to afford the means of determining whether they produce sugar in our climate, and in what quantity; and this decision is necessary to be had before the cultivation in the large way ought to be proposed. For it is possible that they may afford a very saccharine liquid in America, and much less in Europe, as happens with the liquid amber; which, according to Kalm, affords much balsam in New Mexico and South Carolina; but produce less in Virginia, and none at all in Pennsylvania and New York.

The difference which exists between the cold of the nights in the American countries and those of Europe under the same degrees of latitude, the heat of the sun being equal, must necessarily influence the progress of vegetation.

In the mean time, until we shall acquire the necessary information, if it should be thought fit to increase the number of these trees, the following is the method of proceeding:—The surest and most useful is to set the seeds. They may be found in the plantations of M. Duhamel, M. LesMalesherbes, and several other curious gardeners. It is observed, that the acer saccharinum brought from America is not raised but with difficulty, either because it is not transplanted at the proper time, or because it has failed of receiving proper care during the passage. It would perhaps be necessary to send expressly to America an intelligent gardener, or to give instructions in the country to an attentive person to send the best seed to France, and even the plant, in order to accelerate our progress. The acer saccharinum never grows from slips; but it grafts very well on the sycamore. Its resemblance to the plane maple of Europe, which is such that the two trees are confounded, seems to shew that it would be most convenient to graft upon this tree; but a gardener has several times attempted this in vain. An opaque matter which lies between the wood and the bark opposes the union. The maple negundo, the only one which is multiplied by slips, is likewise propagated by seed and by layers. The jasper maple is grafted with the greatest success on the sycamore. It rises with a better stem when grafted in this manner; but care must be taken to graft very low. Ripe seeds of the red maple have not yet been obtained; but it may be raised from American seed, and in this manner

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the two individuals may be obtained. The *acer tomentosum*, lastly, is one of those which is most easily multiplied in our climates.

Though it is said that some species of maple may grow in light stoney soils, they prosper better in good ground. The soil of the mountains cannot always be considered as bad. The red maple delights in the plains and valleys, and all the species are injured by a strong sun. They succeed well if sheltered to the south by mountains or woods, as I conclude from the state of the sugar maples of *Maleherbes* and *Rambouillet*.

The advantages which would result from cultivating the sugar maples in Europe, and particularly in France, are relative to the quantity of sugar they may afford. When it shall be proved that a great quantity, or at least several pounds per tree, can be obtained, lands which are even susceptible of cultivation will be devoted to this object. Some species will require to be placed on the slopes of mountains, where the snow and cold weather prevail for a long time. Others will be planted in low situations on the borders of meadow lands, lakes, rivers, and grounds approaching to marsh lands, such as those where the alder delights to grow. Such plantations, which may contribute to increase the production of sugar without any other care than that of tapping the trees and concentrating the liquor, will be of great utility in supplying our habitual want of this article. Besides which, the sugar maple presents to turners, musical instrument makers, inlayers, the workers in staining wood, and gunsmiths, a valuable material, which in some of the species is veined and marbled. It is proper, therefore, to encourage the growth of this tree.

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In addition to the facts and observations contained in the foregoing memoir, which, if conclusive with respect to France, would probably be much more so with regard to the northern districts of our island; it may be added, that Dr. Benjamin Rush of Philadelphia communicated an excellent paper on the same subject to the American Philosophical Society, which was published in their *Transactions*, vol. iii. in the year 1793. The abstract of his paper is as follows:

The *acer saccharinum* of Linné, or sugar maple tree, grows in great quantities in the western countries of all the middle states of the American Union. It is as tall as the oak, and from two to three feet in diameter; puts forth a white blossom in the spring before any appearance of leaves: its small branches afford sustenance for cattle, and its ashes afford a large quantity of excellent pot-ash. Twenty years are required for it to attain its full growth. Tapping does not injure it; but, on the contrary, it affords more syrup, and of a better quality, the oftener it is tapped. A single tree has not only survived, but flourished after tapping for forty years. Five or six pounds of sugar are usually afforded by the sap of one tree—though there are instances of the quantity exceeding twenty pounds. The sugar is separated from the sap either by freezing, by spontaneous evaporation, or by boiling. The latter method is the most used. Dr. Rush describes the process, which is simple, and practised without any difficulty by the farmers.

From frequent trials of this sugar, it does not appear to be in any respect inferior to that of the West Indies. It is prepared at a time of the year when neither insect nor the pollen of plants exists to vitiate it, as is the case with common sugar. From calculations

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tions grounded on existing facts, it is ascertained that America is now capable of producing a surplus of one eighth more than its own consumption; that is, on the whole, about 135,000,000 pounds, which in the country may be valued at 15 pounds weight for one dollar. Dr. Rush mentions many other benefits his country may derive from this invaluable tree, and concludes his paper with an account of some of the advantages of sugar to mankind; not merely, as commonly considered to be a luxury, but as an excellent wholesome and nourishing article of food. Annexed also is an extract from the report of the committee of the British privy council on the subject of the African slave trade, containing Mr. Botham's statement of the mode of cultivating a sugar plantation at Batavia\*.

VI.

*The Progress of Mechanical Discovery, exemplified in an Account of a Machine for cutting Files. (W. N.†)*

THE folly and consequent distress of pursuing experiments in chemistry, for the sole purpose of commercial advantage, has been repeatedly observed both by public writers and in private life. The obscurity which attends the processes of this art, the imperfections of theory, and the seductions of hope, have united to lead men in pursuit of medicines of uncommon powers, and agents which should convert the cheaper metals into gold and silver. It is a subject of no wonder, to those who have not suffered their mental habits to be vitiated by these seductive analogies, that difficulties and disappointment should attend the life of a man thus employed. But mechanics have, in general, been more favourably regarded. A number of simple and admirably useful effects are produced by the operation of machines. We daily see improvements produced by means easily understood. The mechanic who endeavours to strike into a new path, finds he can reason from what has been done before him, and usually begins his work with a conviction that the results he is desirous of obtaining will infallibly happen. Hence it is that a prodigious number of new schemes find their way into books; on which both the author and the reader set a high value, and of which the futility is discerned only by a few practical men. Some of my readers have supposed this source of information to be much more productive than it really is. A very slight enquiry concerning new machines and inventions, whether they have been carried into effect, and whether they have superseded the old methods of operation, will immediately strike out of the list of valuable articles not less than nine tenths of the objects to which the public attention is solicited. And if it be asserted that the description of such abortive projects might be of use to afford hints to speculators, I must take the liberty to observe, that it is a most serious thing to engage in a new invention, and a no less serious duty in the editor of a public work to

\* To avoid the imputation of plagiarism, I must take notice that the two last paragraphs appeared in an anonymous publication in the year 1794, but were written by myself. N.

† As the observation on the wrapper, that all papers without name or signature are written by the Editor, has been overlooked by some readers, who have made enquiry respecting the authors of such papers, he has thought it expedient in future to add his initials.

be well assured of the value of what he recommends or suffers to recommend itself to his readers. From views of this kind, it has appeared to me, that I should do some service to an active set of men, some of whom have effectually served this country, if I were concisely to point out the course of mechanical invention, in order that those individuals only may be induced to engage in it, who possess the acquisitions and means to do it with some effect.

We will therefore suppose a very acute theorist, who is not himself a workman, nor in the habit of superintending the practical execution of machinery, to have conceived the notion of some new combination of the mechanical powers to produce a determinate effect; and for the sake of perspicuity, let us take the example of a machine to cut files\*. His first conception will be very simple or abstracted. He knows that the notches in a file are cut with a chisel driven by the blow of a hammer, by a man whose hands are employed in applying these instruments, while his foot is exerted in holding the file on an anvil by means of a strap. Hence he concludes, that it must be a very easy operation to fix the chisel in a machine, and cause it to rise and fall by a lever, while a tilting hammer of the proper size and figure gives the blow. But, as his attention becomes fixed, other demands arise, and the subject expands before him. The file must be supported upon a bed or mass of iron, of wood, of lead, or other material:—it must be fixed either by screws or wedges, or weights, or some other effectual and ready contrivance:—and the file itself, or else the chisel, with its apparatus for striking, must be moved through equal determinate spaces during the interval between stroke and stroke, which may be done either by a ratchet wheel or other escapement, or by a screw. He must examine all these objects, and his stock of means in detail; fix upon such methods as he conceives to be most deserving of preference; combine, organize, and arrange the whole in his mind; for which purpose solitude, darkness, and no small degree of mental effort, will be required:—and when this process is considerably advanced, he must have recourse to his drawing board. Measured plans and sections will then shew many things which his imagination before disregarded. New arrangements to be made, and unforeseen difficulties to be overcome, will infallibly present themselves. The first conception, or what the world calls the invention, required an infinitely small portion of the ability he must now exert. We will suppose, however, that he has completed his drawings. Still he possesses the form of a machine only; but whether it shall answer his purpose, depends on his knowledge of his materials. Stone, wood, brass, lead, iron forged or cast, and steel in all its various modifications, are before him; the general processes of the workshop by which firmness, truth, and accuracy, are alone to be obtained; and those methods of treatment, chemical as well as mechanical, which the several articles demand:—these and numberless other practical objects call for that skill and attention, which may either lead to success, or, by their deficiency, expose him to the ignorance or obstinacy of his workmen. If he should find his powers deficient under a prospect so arduous—if he

\* Transactions of the American Philos. Society, vol. ii. or Repertory, v. 124. The file is fixed on a bed of lead, and a chisel fixed at the end of a lever, is struck down with a hammer. This lever rises again of itself by means of a spring, and during its rise it moves a ratchet wheel, connected with the support of the bed; which consequently it shifts together with the file after every stroke.



cannot submit to the severe discipline of seeing his plans reversed, and his hopes repeatedly deferred—if unsuccessful experiment should produce anguish without affording instruction, what will then remain for him to do?—Will he embitter his life by directing his incessant efforts, his powers and resources, to a fascinating object, in which his difficulties daily increase; or, will he make that strong exertion of candour and fortitude, which will lead him to abandon it at once?

These are the inevitable stages of operation, through which every inventor in mechanics must pass. To the mere habit of viewing objects in new lights, the habit which leads to the outline of invention, he must add the power of disposing his notions in the form of an individual engine or instrument; and he must himself become a workman, capable of discerning the means by which his ideas may become realized in the proper materials. It may perhaps seem as if I had selected an instance of difficulty, and indulged my imagination in a sketch of obstacles seldom likely to be met with. This, however, is far from being the case. Nothing seems more simple and easy at first sight, than to make an engine to cut notches in a piece of steel; and a very ingenious person, in the work above referred to, has accordingly given an accurate design of an engine for that purpose, which no doubt he thinks must succeed. But manufacturers well know the value of such an engine, and have long ago attempted to make it by that and various other methods without success. That engine in particular, promising as it appears, is utterly incapable of working, for several reasons, scarcely to be discovered but by practical men, but which cannot with sufficient brevity be here detailed. And with regard to general obstacles in the detail of inventions, I am so far from magnifying them, that I am warranted by much experience, as well on my own behalf, as that of others whose plans and operations have come before me, to affirm, that no mechanical invention really new was ever brought to its complete or perfect state, at so small a charge as three times the cost of the finished engine, exclusive of the incalculable labour of the contriver.

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## VII.

*The Dutch Process for making the Blue distinguished by the Name of Turnfol\*.*

**L**ICHEN, Archil, or in case this last cannot be obtained, the greater moss of the oak, is dried, cleaned, and pulverized in a mill, resembling the oil mill, and then sifted through a brass wire sieve, the interstices of which do not exceed one millimetre in width (1-250th of an inch). The sifted powder is then thrown into a trough, and mixed with an alkali called vedas, which is nothing else but the cendres gravelées in powder. The proportion is one part by weight of the alkali, to two parts of the pulverized vegetable. This mixture is moistened with a small quantity of human urine; the urine of other animals does not contain a sufficient quantity of ammoniac. The mixture ferments, and is kept moist by successive additions of urine. As soon as the materials have become red, they are transferred into another trough, where they are again moistened with urine, and stirred to renew the fermentation. Some days afterwards the paste acquires a blue colour, in which state it is carefully mixed with one third of excellent pot-ash well powdered; and

\* From the Journal du Commerce, copied in the Decade Philos. &c. No. 57.

with this new mixture certain trays are filled, which are one metre ( $39\frac{1}{2}$  inches) deep, and eight decimetres ( $31\frac{1}{2}$  inches) wide. When the fermentation which takes place for the third time has given the paste a considerably deep blue colour, chalk or powdered marble is added, and the whole is well and perfectly mixed. This last addition is made, not to improve the quality of the blue, but to add weight. It is merely an affair of profit. The blue thus prepared is put into iron moulds 32 centimetres long and 22 square at the end ( $1\frac{1}{4}$  inch by  $\frac{8}{16}$  of an inch). The moulded pieces are then placed upon deal planks, in well-aired lofts, to dry; after which they are packed in casks for sale.

The Hollanders made a secret of this process: and in order to mislead, they have published, that the blue was made with rags coloured by the plant turnsole; whence it has obtained its denomination\*. We may derive much profit by carrying this discovery into practice.

## VIII.

*Experiments and Remarks on certain Ranges of Colours hitherto unobserved, which are produced by the relative Position of plain Glasses with regard to each other. (W. N.)*

THOSE ranges of colours which are afforded by the reflection and transmission of light through thin transparent plates, have been an object of much attention to philosophers, ever since the experiments made by Robert Hooke and Sir Isaac Newton. The latter of these philosophers endeavoured to generalise the facts by a statement, which, because in part hypothetical, has been treated with contempt by some eminent men, though it has been referred to in most disquisitions on these phenomena. When a convex lens is applied to a plain glass, it is well known that coloured circles surround the place of contact; and as these circles are found to vary in their dimensions, the nearer the glasses lie with respect to each other, whether by means of the curvature or of pressure, it has been concluded that the effects are caused or governed by the distances of the surfaces. From various considerations Sir Isaac was led to conclude, that the rays of light are themselves possessed of a property, by which, in certain equidistant points of their length, they are disposed to enter transparent bodies, and in certain other points, intermediate between these, they are disposed to be reflected. From this assumption he deduced, that if a ray of light passed through the first surface of a medium, it would be either transmitted or reflected at the second surface, accordingly as the distance might happen to coincide with a point of transmission or reflection. According to this doctrine, it must follow that the interval will govern the effect, not in consequence of its own magnitude, but of the precise number of measures, or, as he calls them, fits of transmission or reflection it may contain.

Thus, for example, if we suppose a ray of indefinite length to be divided into equal parts, and imagine certain marks of division to subsist; if the ray pass, through the first surface of a medium exactly at one of these marks, it will be transmitted through any surface, however distant, which shall be remote from the first, either nearly or accurately some precise number of parts; because the fits of transmission are respectively at those points or marks: and

\* English writers have used this denomination. But the dry-salters, or dealers in drugs, distinguish these particles by the name of litmus.

on the other hand, it will be reflected from any surface whose distance is measured by some number of whole parts, together with half a part; because the fits of reflection lie half-way between the divisions. In this statement I have merely enunciated what are supposed to be the facts, and have not attended to his supposition of an ethereal elastic fluid, pervading all space and thrown into undulations more rapid than the velocity of light itself. Neither can I at present enter into any discussion concerning its probable value. My present object is to relate a few experiments, which, at the same time that they shew the existence of these fits at very considerable intervals, appear to prove that the reflection and transmission of light in the same medium is governed by other circumstances as well as by the distance between the confines or surfaces. These experiments lead to many speculations and extended paths of enquiry. It has long been my intention to multiply and repeat them, in order to discover the laws upon which they depend: but other avocations have already delayed the accomplishment of this purpose for more than seven years; for which reason, I hope that my desire to see the object pursued by others may operate as an apology for the imperfect state of the research, of which I here present the commencement.

The experiments of the Abbé Mazeas on colours produced by applying flat plates of glass to each other, are related at full length in Priestley's History of Light and Colours, p. 499. This philosopher found, that rubbing the glasses together caused the colours to appear at the same time that adhesion took place between them, and Muschenbroeck found that lenses of long focus do not afford colours after having been laid by for a length of time unless they be washed and wiped. One of the most remarkable circumstances in the experiments of Mazeas was, that the colours of his flat plates are moveable by a very slight increase of temperature; which produces an effect similar to what would have arisen from removing the glasses further asunder, or diminishing the pressure which might have been applied to them. Beccaria was, I believe, the first who observed that these colours may be produced by superinducing an electric charge on the external surfaces of two plates, which are by this means made to adhere. But in all the observations I have met with, whether made by Priestley in his History, or by any other writer, these powers have been supposed to operate by increasing or diminishing the distance between the surfaces. The following facts will shew that this conclusion requires to be modified.

In the year 1791 I cut a plate of very clear glass into portions, which were intended to be used to defend a vessel of quicksilver from the agitation of the air. Its colour, when viewed edgewise, was a very light green, and it had been carefully ground by an optician, with its sides truly parallel to each other. The thickness was twelve hundredths of an inch. A piece 3,2 inches long and 2,4 wide was laid upon another larger piece, both having been previously wiped. The result was, that faint colours appeared in rows about six or seven in number. Pressure appeared to alter these very little in position; but it produced other more vivid colours, which were much more moveable, and crossed the former without affecting them. The scarcely moveable ranges were very little disturbed by the heat of the finger applied against the undermost glass; though this application was sufficient to produce a very great alteration in the figure of the other ranges. When the upper glass was lightly placed upon the lower, the faint and scarcely moveable ranges appeared alone, and the glasses had very little adhesion; so that the uppermost slid about on the larger plate beneath, without seeming to carry the ranges with it, but arrived at other ranges, which

which from their immobility seemed as if attached to the lower glass. These scarcely moveable colours, however, were quickly moved, and brought nearer to each other by raising one edge of the glass with the hand; and they continued visible when the glass was lifted as high as one-fortieth or thirtieth of an inch, by slipping a knife under its edge. When a plate of metal was placed beneath the lower glass, and electricity (which from the disposition of the machine happened at that time to be negative) was communicated to the upper plate, the vivid colours appeared, and the adhesion of the glasses was increased; but the scarcely moveable ranges remained little if at all affected. When the glasses were taken from the machine towards the window, the vivid colours slowly receded as the electricity was dispersed, and in this state they were very advantageously and evidently seen crossing the other less moveable ranges.

These facts appeared at that time to lead to no other conclusion than that the moveable ranges had been disturbed by some other circumstance besides that of the mere distance of the plates: for, if the distance had indeed been varied, it seems reasonable to suppose that both sets of ranges would have been affected.

Soon afterwards, upon making some observations with the artificial horizon, and a sextant constructed by Troughton, I observed a series of colours in the horizon-glass, when the position of zero was to be ascertained. They appeared both in the silvered and the clear part. The position of the glasses, when the colours were seen, was very nearly parallel as to the vertical situation of the planes; but it admitted of the index being moved through nearly forty minutes before they disappeared. These glasses, namely the index-glass, and the horizon-glass, were  $3\frac{1}{2}$  inches asunder.

This last fact appears to justify the inference of Newton, who considered the colours of thick and thin plates as depending on the same cause; which doctrine was applied by Dr. Pemberton, to account for the numerous ranges of colours sometimes observed beneath the common rainbow\*.

As one of the greatest difficulties in Newton's doctrine seems to have been that the fits of reflection and transmission are supposed to extend to vast distances, it seemed desirable to repeat this experiment with as great an interval between the glasses as could conveniently be had. With this view I placed one plate of glass on the surface of a vessel of mercury, and held another in my hand parallel to it, in such a position that the reflected light from the first passed through the second glass to my eye. The white clouded sky and the chimney of an opposite house were seen by reflection in both glasses, and it was easy to move the upper glass till both images of the chimney coincided. In this position the glasses would have been parallel if the object had been indefinitely distant; but in the present case, the parallelism could not be obtained but by a slight elevation of the moveable image. By this disposition the colours were made to appear when the plates were four feet asunder, and I have no doubt but that the same would have happened at much greater distances if the imperfect method of adjusting the parallelism could have been applied.

The same effect, as might naturally be expected, took place when the eye was so placed as to receive the reflected light from the lower glass, after it had been transmitted through the upper.

\* *Philos. Trans.* abridged, vol. vii. or *Priestley's Optics*, p. 596.

In addition to these observations, it may be remarked, that I have not observed these colours in such plates of common looking-glasses as I have tried; that they do not appear in a small pocket sextant which I possess; that the disposition to exhibit these colours seems to be increased by wiping or friction, and also, as I think, by a continuance of the action which is necessary to produce them, or cause them to run along the surface of the glasses.

IX.

*Some Account of the Country and Climate of the North-western Lakes of America. By Major C. SWAN, Paymaster to the Western Army\*.*

D'Etroit, Oct. 10, 1797.

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"This country is yet new, and almost in a state of nature, like its inhabitants. It is true the soil is extremely rich and fertile; and it is to a superabundant burden of vegetation, and a flat surface for hundreds of miles together producing much stagnant water, that we may attribute the unwholesomeness of the climate, which is almost certain to affect the inhabitants with bilious complaints every fall †.

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
"Gen. Wilkinson arrived here in June this year, and, after making some prompt arrangements for the garrison, proposed a voyage to Michilimackinac, and invited me to accompany him; and on the 4th day of August we embarked in a sloop of about 70 tons burthen. We had a safe and pleasant trip, not only to Michilimackinac, but even into Lake Superior; and returned to this place on the 4th of last month, highly gratified indeed.

"We first left this place, and traversed Lake Sinclair, a handsome circular lake about twenty-five miles across. We then proceeded up the river of that name, which is broad and very handsome, for about forty miles, to a rapid at the entrance of Lake Huron; traversed this immense beautiful lake, three hundred miles long; and arrived on the 15th of August at the streight which unites it to Lake Michigan. This streight is broad; and the Isle de Bois Blanc, or White Wood Island, Round Island, and Michilimackinac Island form a cluster in the middle of the streight, and afford a romantic and majestic landscape from the sea. The Isle de Bois Blanc is eleven miles and a half long, and from two to three wide, lying parallel to the two coasts of the streight, but nearest to the south-side. Round Island

\* From the Medical Repository, printed at New York, vol. i. p. 526. It is introduced by the following Note of the American Editor:

Note. The following Article consists of Extracts from a Letter of Major Swan to Capt. Frye, commandant of the garrison at Governor's Island, who was so obliging as to communicate them to one of the Editors; and they are now made public with the consent of the Author. These Extracts are briefly made from a Journal kept by Major Swan at the time; and are inserted for the threefold purpose of illustrating the table of thermometrical observations which follow, of communicating a short but authentic view of a part of the north-western territory, and of inviting further information.

† These remarks have particular reference to the neighbourhood of D'Etroit.

is about three miles in circumference, and lies at the upper or south-west end of De Bois Blanc. The island of Michilimackinac is circular, and lies between the upper end of De Bois Blanc and the north-western coast of the streight; having a chanhel of about one mile and a half between it and De Bois Blanc, and a channel of nine miles between it and the north-western coast of the streight. It measures seven miles and three quarters in circumference, and is nearly circular. On the south-side of this island there is a small bafon of a segment of a circle, serving as an excellent harbour for vessels of any burthen, and for canoes. Around this bafon the village is built, having two streets of nearly a quarter of a mile in length, a Roman chapel, and containing eighty-nine houses and stores; some of them spacious and handsome, with white lime plastering in front, which shews to great advantage from the sea. At one end, and in the rear of the town, is an elegant government-house of immense size, and finished in great taste. It is in the form of ; one story high, the rooms fifteen feet and a half in the clear. It has a spacious garden in front, laid out with taste, and extending from the house on a gentle declivity to the water's edge. There are two natural limpid springs in the rear of the house, and a very lively grove of sugar-trees called the Park. Suitable out-houses, stables and offices are added; and it is enriched on three sides with beautiful distant prospects. Twenty rods from the rear there is a sudden and almost perpendicular ascent of about a hundred feet of rock, upon the top of which stands the fort, built of stone and lime, with towers, bastions, &c. occupied by our troops, and commanded by Major Burbeck. About half a mile from the fort, in the rear, there is an eminence which I estimate to be about two hundred and fifty feet from the surface of the water. This spot commands an extensive and sublime view of the adjacent country. The fort, the village, the neighbouring islands and channels seem prostrated at your feet; while to the south-west you look into the immensity of Lake Michigan, which loses itself in the southern hemisphere; and to the north-west the great Lake Huron lies expanded to the bounds of the horizon. It was a beautiful morning when I had this view.

"This celebrated streight is the only key to the immense lucrative skin trade now solely carried on by British subjects from Montreal, with the nations of Indians, called the Sauteurs or Chipewas, Sioux, Reynards, &c. who inhabit the water-courses that fall into the Mississippi between the Illinois and the falls of St. Anthony. Canoes are loaded and fitted out by these traders every year from Michilimackinac. They commonly set out in July, and return in June, July or August the year following, to Michilimackinac, from whence they started. Here they are again met by the Montreal canoes with fresh goods, exchange loading, and each return from whence they came. The Montreal canoes penetrate to Michilimackinac by way of Grand River; which, with the exception of a small portage, conveys them to the northern point of Lake Huron; and return by the same route. Those from Michilimackinac penetrate the interior or Indian country by way of Green Bay, an arm of Lake Michigan, thence through Fox River into the Mississippi and its tributary streams; and return also to Michilimackinac by the same route.

"On the 22d of August we left Michilimackinac, and on the 23d anchored in the streight of St. Joseph, which leads to Lake Superior. At this place nature has displayed very handsomely again. The mouth of the streight is about thirty miles wide, but so strewed over with innumerable small circular islands that it is difficult to obtain a view in any direction of more than six or eight miles. Indians have sometimes been lost among

these

these islands for weeks together. They extend into Lake Huron, and continue along the north-west coast of the lake for an hundred and eighty miles, and are called by the savages the Meneto or Devil's islands. From the entrance of the streight at a place called the Detour, it is nine miles to the new British garrison built on the point of the island of St. Joseph, commonly called the Carraboo island. This is the largest in the streight; being about twenty-five miles long, and from ten to three broad.

"On the 23d of August we left the vessel, embarked in three canoes, ascended the streight in what is called the Canoe channel, and encamped at Muskito Point.

"The 24th, at one o'clock P. M. we arrived at the falls of St. Marie, called *le Saut de St. Marie*. These falls are about three-quarters of a mile long and half a mile wide; the rapid not violent; and the perpendicular of the whole fall about thirty feet. There is a small kind of village on the United States side containing sundry large warehouses, and a few decent dwelling-houses occupied by the Agents of the Canada North-west Trading Company. There is not a clear white woman in the place.

\* \* \* \* \* "The 25th it rained \* \* \* \* \*

\* \* \* "On the 26th we set off in two bark canoes from the upper end of the portage for Lake Superior. \* \* \*. At one o'clock P. M. we entered Lake Superior; looked fairly into it; drank of its waters; ate our dinner; and put about with a fine fair wind. We reached the falls again at four o'clock in the afternoon; placed experienced guides with strong paddles in the bow and stern of each canoe; hoisted the fifteen stripes; and launched into the bosom of the cataract. In a moment we were safe in the basin at the bottom of the falls!

"We embarked early on the 27th. Having a strong current and fair wind we descended in the ship channel, and reached the vessel at Carraboo island, at nine o'clock in the evening.

"The 28th we put to sea again; and on the 4th of September at sun-down reached this place.

"I inclose to you herewith degrees of heat which were ascertained by regular observation with Fahrenheit's thermometer every day; by which you will perceive that the temperature of the Lakes differs widely from that of the Atlantic country."

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On comparing the Table of Observations which is annexed with observations made on the same days in this city, the difference will appear so remarkable that the reader may suspect some error in the instrument made use of by Major Swan: and such were my suspicions. But crossing the East river to Governor's Island in company with that gentleman on the 4th of March 1798, he observed, that the wind which we then felt, and which was very brisk, resembled, in point of temperature, that which he experienced on Lake Huron on the 14th and 15th of August 1797; and added, that on the same evening a frost affected the gardens at Michilimackinac so severely as to destroy the greater part of the vegetables.

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## T A B L E

OF THE

Degrees of Heat observed on Fahrenheit's Thermometer, from August 4,  
to September 4, 1797.

1797.	6 o'Clock A. M.	12 o'Clock M.	7 o'Clock P. M.	Average.	Remarks, where.
August 4	66	63 $\frac{1}{2}$	59	62 $\frac{1}{4}$	Lake Sinclair.
5	59	66	63 $\frac{1}{2}$	62 $\frac{1}{4}$	Ditto.
6	55 $\frac{1}{2}$	65	66	62	River Sinclair.
7	60	67	67	64	Ditto.
8	59	61	61	60	Ditto.
9	57	62	52	57	Ditto.
10	58	62	61	60	Ditto.
11	63	59 $\frac{1}{2}$	60	60	Lake Huron.
12	57	60	61	59	Ditto.
13	55	55 $\frac{1}{2}$	55	55	Ditto.
14	50	49	47	48	Ditto.
15	45	66	54	55	Michilimackinac.
16	52	70	53	58	Ditto.
17	54	69	60	61	Ditto.
18	52	67	62	60	Ditto.
19	54	64	56	58	Ditto.
20	53	64	58	58	Ditto.
21	62	63	63	62	Ditto.
22	54	61	59	58	Streights of St. Joseph.
23	57	65	60 $\frac{1}{2}$	62	Ditto.
24	50	63	60 $\frac{1}{2}$	57	Ditto.
25	46	53	49	49	Falls of St. Marie.
26	46	49	50	48	Lake Superior.
27	50	56	49	51	Streights of St. Joseph.
28	40	50	46	45	Lake Huron.
29	51	54	51	54	Ditto.
30	49	57	53	53	Ditto.
31	50	56	49	51	Ditto.
Sept. 1	48	57	47	50	Ditto.
2	51	59	50	53	Ditto.
3	49	58	50	52	River Sinclair.
4	48	56	49	51	Lake Sinclair.



## X.

*An Essay on the Art of conveying Secret and Swift Intelligence.* By RICHARD LOVELL EDGEWORTH, Esq. F. R. S. and M. R. I. A. \*.

**M**ODELS of the French Telegraph have been so often exhibited, and the machine itself is so well known, that it is not necessary to describe it minutely in this place. It is sufficient to say, that it consists of a tall pole, with three moveable arms, which may be seen at a considerable distance through telescopes; these arms may be set in as many different positions as are requisite, to express all the letters of the alphabet:—By a successive combination of letters shewn in this manner, words and sentences are formed, and intelligence communicated. No doubt can be made of the utility of this machine, as it has been applied to the most important purposes. It is obviously liable to mistakes, from the number of changes requisite for each word, and from the velocity with which it must be moved to convey intelligence with any tolerable expedition.

The name, however, which is well chosen, has become so familiar, that I shall with a slight alteration adopt it for the apparatus which I am going to describe. *Telegraph* is a proper name for a machine, which describes at a distance. *Telegraph*, or contractedly *Tellograph*, is a proper name for a machine that describes *words* at a distance.

Dr. Hooke, to whom every mechanic philosopher must recur, has written an essay upon the subject of conveying swift intelligence, in which he proposes to use large wooden letters in succession. The siege of Vienna turned his attention to the business. This method is more cumbrous than the French Telegraph, but far less liable to error.

I tried it before I had seen Hooke's work in the year 1767, in London; and I could distinctly read letters illuminated with lamps in Hampstead church-yard, from the house of Mr. Elers in Great Russel Street, Bloomsbury, to whom I refer for the date and circumstance.—To him and to Mr. E. Delaval, F.R.S. to Mr. Perrot of Harehatch, and to Mr. Woulfe the chemist, I refer for the precedency which I claim in this invention. In that year I invented the idea of my present tellograph, proposing to make use of windmill sails, instead of the hands or pointers which I now employ. Mr. Perrot was so good as to accompany me more than once to a hill near his house, to observe with a telescope a windmill at Nettlebed; which places are, I think, sixteen miles asunder. My intention at that time was, to contrive not only a swift but an unsuspected mode of intelligence: By

\* Transactions of the R. Irish Academy, vi. 125. The first part of this paper consists of near thirty pages of very interesting historical detail, respecting the art of conveying intelligence by sounds and signals. It was practised by Theseus, in the Argonautic expedition; by Agamemnon, at the siege of Troy; and by Mardonius, in the time of Xerxes. It is frequently mentioned in Thucydides; it was used by Tamerlane; by the Moors in Spain; and by the Welch in Britain; by the Irish; and by the Chinese, on their famous wall of fifteen hundred miles, by which they separated themselves from Tartary. I have omitted this part on account of its length.

means of common windmills this might have been effected, before an account of the French Telegraph was made public\*.

My machinery consists of four triangular pointers or hands, each of which points like the hands of a clock to different situations, in the circles which they describe.

It is easy to distinguish whether a hand moving vertically, points perpendicularly downwards or upwards, horizontally to the right or left, or to any of the four intermediate situations.

The eye can easily perceive the eight different positions in which one of the pointers is represented, plate xiii. fig. 1. by turning the eye to the circle A.

A similar circle may be imagined round each of the pointers, by which the numbers which they are intended to express, may be discovered with much facility.

Of these eight positions, seven only are employed to denote figures; the upright position of the hand or pointer being reserved to represent O or Zero. The figures thus denoted, refer to a vocabulary in which all the words are numbered. Of the four pointers, plate 1. that which appears to the left hand of the observer, represents thousands, the others hundreds, tens and units in succession, as in common numeration.

† In the annexed plate the four large pointers stand at 2774, which in the common arrangement of my vocabulary, denotes the Royal Irish Academy. For permanent stations, which may be seen clearly with tolerable glasses at twenty miles distance, stone or wooden pillars sixteen or twenty feet high must be solidly erected: on the top of these a moveable circle, or platform, turns horizontally upon a centre: on this platform an axis moves vertically, and carries the arm or pointer along with it. Eight handles turn the pointers,

\* Since this paper was written I received a letter from Mr. Perrot, which has been seen by the President, containing the following passage.

"I perfectly recollect having several conversations with you in 1767, on the subject of a speedy and secret conveyance of intelligence: I recollect our going up the hills, to see how far and how distinctly the arms (and the position of them) of Nettlebed windmill were to be discovered with ease. As to the experiments from Highgate to London, by means of lamps, I was not present at the time; but I remember your mentioning the circumstance to me, I believe in the same year. All these particulars were brought very strongly to my memory, when the French a few years ago conveyed intelligence by signals; and I then thought and declared, that the merit of the invention undoubtedly belonged to you.---I am very glad that I have it in my power to send you this confirmation, because I imagine there is no other person now living who can witness your observations in Berkshire."

† I insert, Plate xiii. fig. 2. a line described by telegraphs as an example.—It is the first line of the following verses, written on the prospect of corresponding between England and Ireland by the Telegraph:

Hark from Basaltic rocks and giant walls,  
To Britain's shores the glad Hibernia calls;  
Her voice no longer waits retarding tides,  
The meeting coasts no more the sea divides.  
Quick, at the voice of fortune, or of fame,  
Kindles from shore to shore the patriot flame;  
Hov'ring in air, each kindred genius smiles,  
And binds with closer bands the sister isles.

The numbers are, 2645, 2331, 573, 1113, 244, 2411, 6336.

which

which are fixed in their different positions by a catch or alidad. By means of the platform, the pointer may be turned to any part of the compass; and as one side of it is painted black and the other white, either side may be employed, as the colour of the clouds, or the situation of the place, may require.

Besides these permanent machines, of which dimensions and a description are subjoined in plate xiii. I make use of portable machines, (which may be detached like tentacula from the main body in hazy weather,) consisting of pointers ten or twelve feet high, and of a light triangular stand, which can be easily fastened with tent pegs to the ground: these may be lodged in any house near the place where they are used, or in times of danger may be carried back to the permanent stations every night.

In managing a correspondence by these machines, it is necessary to have certain signals established: nor are these signals merely arbitrary; it is absolutely necessary that they should be made by the two external or by the two internal pointers, else they could not be repeated by the intermediate stations without confusion; because, in the middle stations, that pointer which represents thousands when conveying a message eastward for instance, must, when answer is returned in an opposite direction, represent units; the same change will take place between the pointers that denote hundreds and tens.

Certain hours of the day must be appointed for ordinary communication. Suppose ten o'clock in the morning, and five in the afternoon in summer. Every communication begins from the capital. If no intelligence is required to be conveyed from thence, the word *BEGIN* is sent to the county station, which may then proceed or dismiss the meeting.

When any communication is to be commenced, the pointers that denote thousands and units are whirled round, till the same is done at the corresponding station. When this signal has been answered, the person who gave it proceeds to send his intelligence. As soon as he begins, the pointer of hundreds at the opposite station is turned to two, and kept in that position till the word is made out from the vocabulary; it is then turned up to *O* or *ZERO*. The person who is speaking, when he perceives by this signal that he is understood, turns all the machines to *NOUGHT*, which is always to be done at the conclusion of every word.

When all his machines are in this position, his correspondent again turns his pointer belonging to the place of hundreds to two, where it is to remain till he receives another word, and so on till all that is meant to be said is finished. To denote that his communication is finished \*—*THOUSANDS* and *UNITS* are to be vibrated backwards and forwards with the point downwards like a pendulum, till the same is done at the opposite station.

If any interruption takes place on either side, from a cloud, a shower, or any accident, it is pointed out by vibrating *THOUSANDS* and *UNITS* with their points upwards; which signal must be repeated from the opposite station. Whoever has made the signal of interruption, must make a signal of recommencement, when he is ready to proceed, by vibrating *HUNDREDS* and *TENS*, with their points upwards; when this is answered (but not before,)

\* I use the words *Thousands* and *Units* here, and in the rest of this description, for the pointers or machines that stand in the numerical place of thousands and units.

the business may proceed. It should be observed in general, that every signal should be answered.

It requires some steadiness to abide by these signals; but if they are patiently adhered to, the success that they ensure will soon convince the operator of their utility. Without them every thing would be in confusion; by their interposition, perspicuity and order are perfectly ensured.

In my first experiments the impatience of friends who were present, was sometimes so great as to make it very difficult to adhere to previous arrangements; but a very little practice (I mean the practice of five or six days) reduced the routine of communication to as much facility as could be desired, so that a word (or a sentence, if contained in the vocabulary) could be sent in twenty seconds.

Any person who has the slightest taste for science or literature must be struck when he sees instantaneous interpretation of signals which are made at the distance of fifteen or twenty miles, and when he perceives the power which is obtained of transmitting thought with such astonishing rapidity.

I shall not enter into a detail of the signals which are necessary for intermediate stations; it would take up some time to explain them, and they will readily occur from what has been said already.

What I have hitherto described relates to a large and permanent establishment\*; for the management of which one man is required at each pointer, one at the telescope, and another at the vocabulary; but for ordinary purposes, a single pointer, with one man to work it, and another at the telescope, with a smaller vocabulary, are sufficient. With this reduced apparatus we can with ease speak at the rate of one word per minute to a great distance, as the time lost by intermediate stations is but small.

The vocabulary corresponding with the numbers denoted by this machinery, is composed of a large book with mahogany covers framed, to prevent them from warping. Its size is forty-seven inches by twenty-one; it consists of forty-nine double pages, that is to say, each sheet is folded in the middle, where it opens from one page.

The book is divided into seven parts, consisting each of seven pages, by thin slips of mahogany, which serve to open it easily at each of these divisions. Every one of these seven divisions contains seven pages, and each page contains forty-nine words.

No more than forty-nine words are contained in a page, because the numbers of 8 and 9, and Zero, are omitted. This omission arises from the structure of the machinery, which points only to seven numbers, reserving O for a point of rest, at which point the hands indicate nothing. In every hundred, therefore, only forty-nine numbers are used; and in every thousand only seven hundred is counted. Each division of the book separated by the mahogany rulers, contains all the efficient numbers in seven hundred. Each of these rulers projects (Plate xiii. fig. 3.) beyond the sides of the pages, and each is numbered in succession from one to seven; and they are so placed below one another, as to permit the numbers on all sides of them to be seen at once, as in plate xiii.

When any number of thousands is pointed out, it can by means of these rulers be im-

\* The house belonging to this establishment might be made tenable against a mob, or musquetry, at a small expence, by port flankers of elm or ash adapted occasionally to the windows. See plate xiii. fig. 4.

mediately

mediately selected; the series of seven pages, which one of these rulers opens, is cut, like the alphabet of a ledger, at the edge in seven divisions. By these means the page containing the hundred which is wanted, is instantly found. In the page thus found the tens, from ten to seventy inclusive, are divided from each other so as to be instantly distinguishable, and the units under each division are in like manner easily selected.

Plate III. is a specimen of the first page of the vocabulary; and though it is but one-fifth of the real size, it is sufficiently distinct. It is divided into eight classes; all the classes are numbered downwards seriatim from 1 to 77, omitting cyphers or zero, and eights and nines. When once the class required is ascertained, any number on the page can be found immediately:—As for instance, the reader will easily select class iv. number 36, or class vii. number 77, and so of the rest.

Nothing remains to be explained but the manner in which the class in each page is pointed out by the machinery. For this purpose, before the pointers are turned to any set of figures, the pointer that represents thousands is turned to the class that is wanted; as soon as the correspondent answers this signal, THOUSANDS is returned to O, and instantly all the pointers are moved to the places which denote the figures required for any word or sentence.

When the class is thus ascertained, an index, which slides on the mahogany cover of the book, is set to the column belonging to this class; the number of thousands is then opened by the ruler, as soon as it is read off by the telescope; the number of hundreds is opened by the pages when they are cut away, and the number of tens and units is seen on the page. As the pointers are moved in succession from thousands to units, the different divisions of the book can be opened as fast as the pointers are moved. The order of this book might be reversed with apparent advantage, by dividing the book into classes by the mahogany rulers, &c. But I prefer, for reasons which it would be tedious to insist upon, the arrangement which I have followed.

As secrecy is an object of the greatest consequence, I shall endeavour to point out in a few words the superiority of this mode of communication over any alphabetical arrangement, not only in point of expedition, but of concealment.

Although the common alphabet may be varied at pleasure, and any arbitrary signs may be employed to convey the powers of each letter, yet, by certain rules, any of these arrangements may be deciphered. Whoever sees the movements of the French telegraph. (I mean of that which is commonly known as such) may unfold the intelligence which it conveys, by merely marking down the changes which he sees, and putting them into the hands of a decipherer. The rules for deciphering depend upon the usual arrangements of letters. In our language a single letter must be A or I. The proportions which exist between words of one, two, three, and any greater number of letters, are classed in catalogues; and from these the monosyllables of any cipher are easily obtained, and from the letters of these monosyllables the letters of longer words are discovered. By similar rules, some of which are very ingenious, and which depend upon the general philosophy of language, any alphabetical cipher may be easily unfolded. But these rules, except a very few of them, are useless, when we employ ciphers which denote entire words. Here the most obvious means of discovery may be avoided, by omitting those common words which occur so frequently in every language, the, and, that, to, &c. But, supposing that from its frequent recurrence any particular word should be discovered, no progress can be made from these

these data. The cipher of each word is an isolated fact, which leads to nothing farther. Suppose the knowledge of any particular vocabulary should fall into hands for which it was not intended, a slight change in the numeration, without any actual change of the figures, would entirely prevent discovery:--For instance, if the Lord Lieutenant wished to send orders to the Commander in Chief; if he made use of the numbers written in the vocabulary in one day, he might, after previous communication, employ a different numeration by ordering that 1 (for instance) should be added to every figure. If class ii. number 3664, stood in the vocabulary for gunpowder by the addition which I have proposed, the number would stand class iii. number 4775, which might mean a crocodile, or Tippoo Saib, or any thing foreign to the real word. By similar provisions any number of separate correspondents might carry on a mutual intercourse, without interfering with one another.

In the course of twelve months I tried a great number of experiments, and carried on a great number of conversations with the tellograph; of all these a regular journal has been kept, containing what was unsuccessful, as well as what succeeded. If such journals were kept in the prosecution of philosophical pursuits, they would pay for the trouble of keeping them by the accuracy of the experience which they ensure.

I shall not at present enter into any detail of my Nocturnal Tellograph. Its velocity far exceeds what can be done by day, as in clear weather stations at fifty miles distance may be plainly distinguished.

When this paper was first presented to the Academy, I had determined to try an experiment across the Channel from Donaghadee to Port-Patrick. I was ambitious of being the first person who should connect the islands more closely, by facilitating their mutual intercourse. Public business prevented me from going to the sea-side at the time I had intended, and from carrying on a series of conversations by day and night between the two kingdoms; but Mr. Lovell Edgeworth, my son, had the satisfaction of sending four messages across the Channel at four o'clock P. M. on the 24th of August 1795, and of receiving immediate answers before a vast concourse of people. The machines by which this communication was made, were thirty feet high, and fifteen feet at the base. A child of four years old could turn them. Misty weather prevented them from being seen; but when the weather cleared up, a pointer of twelve feet high could have been plainly distinguished across the Channel.

Though I have bestowed much attention and labour upon this subject, I do not pretend to say that the means of tellographic communication, which I have invented, are the best that can be devised. Imitations without end may be attempted; pointers of various shapes and materials may be employed; real improvements will also probably be made; and, perhaps, new principles may be adopted. The varieties of art are infinite, and none but persons of narrow understanding, who feel a want of resources in their own invention, are jealous of competition and disposed to monopolize discoveries. The thing itself must sooner or later prevail, for utility convinces and governs mankind; and however inattention or timidity may for a time impede its progress, I will venture to predict, that it will at some future period be generally practised not only in these islands, but that it will in time become a means of communication between the most distant parts of the world, wherever arts and sciences have civilized mankind.

TABLE.

SPECIMEN OF THE VOCABULARY  
BELONGING TO  
MR. EDGEWORTH'S TELLOGRAPH.

Common Words, Clafs o.	Words lefs common. Clafs 1.	Technical Terms, c. n. m. Clafs 2.	Perfons. Clafs 3.
1.	1.	1.	1.
11 A	11 Abafe	11 Aback	11 Abbot
12 Ab	12 Abate	12 Abacus	12 Ackland
13 Ac	13 Abbey	13 Abaft	13 Acton
14 Ad	14 Abbefs	14 Abatis	14 Achefon
15 Ae	15 Abbot	15 Abdomen	15 Adams
16 Af	16 Abdicate	16 Abductor	16 Adamfon
17 Ag	17 Abed, Abet	17 Abeal	17 Adair
2.	2.	2.	2.
21 Ah	21 Abide	21 Aberration	21 Adolphus
22 Ai	22 Abjure	22 Abeyance	22 Addington
23 Ak	23 Ablative	23 Ablution	23 Ahmuty
24 Al	24 Able-bodied	24 Abortion	24 Aikin
25 Am	25 Abolifh	25 Abreast	25 Alcock
26 An	26 Abominatiön	26 Abrogation	26 Aldridge
27 Ap	27 Abortive	27 Abfcfs	27 Allot
3.	3.	3.	3.
31 Aq	31 Above-all	31 Abfcis	31 Alley
32 Ar	32 Above-board	32 Abfcenthium	32 Allett
33 As	33 Above-mentioned	33 Abforbent	33 Allen
34 At	34 Abridge	34 Abforption	34 Alder
35 Av	35 Abridgement	35 Abftergent	35 Alexander
36 Au	36 Abruptly	36 Acacia	36 Amyatt
37 Aw	37 Absentee	37 Academic	37 Ambrofe
4.	4.	4.	4.
41 Ay	41 Absolve	41 Acantha	41 Anderson
42 Ax	42 Abfolution	42 Accretion	42 Andre
43 Az	43 Abforb	43 Acefcient	43 Andrews
44 Abandon	44 Abftraft	44 Acetous	44 Angel
45 Abuse	45 Abftrufe	45 Achromatic	45 Anger
46 Alhor	46 Abfurdly	46 Acids	46 Annefly
47 Abjeft	47 Abyfs	47 Acidity	47 Annefdale
5.	5.	5.	5.
51 Ability	51 Academy	51 Acme	51 Antrim
52 Able	52 Accelerate	52 Aconite	52 Anfon
53 Above	53 Accent	53 Acouftics	53 Anfruther
54 Abound	54 Acefs	54 Acroftic	54 Antonie
55 About	55 Aceffary	55 Adamant	55 Anthony
56 Abundance	56 Aceffible	56 Adder	56 Alfred
57 Abroad	57 Acclamation	57 Adder's-tongue	57 Alphonfus
6.	6.	6.	6.
61 Abrupt	61 Accommodate	61 Adductor	61 Amadeus
62 Abfent	62 Accomplice	62 Adelphi	62 Anne
63 Abfence	63 Accoft	63 Ades	63 Anfolm
64 Abfolute	64 Accountant	64 Adit	64 Appleby
65 Abftain	65 Account-book	65 Adjutant	65 Apsley
66 Abfurd	66 Accretion	66 Adnata	66 Archer
67 Abftain	67 Accrue	67 Adonis	67 Aukin
7.	7.	7.	7.
71 Abuse	71 Accumulate	71 Adofculation	71 Archdale
72 Accede	72 Accufative	72 Adracanth	72 Arran, Ld.
73 Accept	73 Ace	73 Adrift	73 Archdall
74 Acceptable	74 Achieve	74 Advance-foffe	74 Afhe
75 Accident	75 Acquifition	75 Advance-guard	75 Atkinfon
76 Accompany	76 Acquittance	76 Advancement	76 Aylward
77 Accomplifh	77 Acre	77 Advertifement	77 Ayre

Officers. Class 4.	Places. Class 5.	Navy and Merchant Ships. Class 6.	Phrases and Sentences. Class 7.
<b>I.</b>	<b>I.</b>	<b>I.</b>	<b>I.</b>
11 Academy of Inscriptions	11 Abbeville	11 Atlas	11 Attend to-day at A. M.
12 Academy of B. L. Paris	12 Aberdeen	12 Ajax	12 ——— at P. M.
13 Academy	13 Abergavenny	13 Albion	13 ——— to-morrow at A. M.
14 Account Office	14 Abington	14 Africa	14 ——— at P. M.
15 Admiralty	15 Abydos	15 Audacious	15 ——— to-night at —
16 Agent to the —	16 Abyssinia	16 Agamemnon	16 ——— to-morrow night at —
17 Admiral	17 Acadia	17 America	17 ——— on Monday at A. M.
<b>2.</b>	<b>2.</b>	<b>2.</b>	<b>2.</b>
21 Adjutant	21 Acaniboo	21 Anson	21 ——— at P. M.
22 Alderman of Bristol	22 Acapulca	22 Alcide	22 ——— on Tuesday at A. M.
23 Alderman of Cork	23 Acam	23 Alexander	23 ——— at P. M.
24 Alderman of —	24 Adia	24 Alfred	24 ——— on Wednesday at A. M.
25 Archdeacon of —	25 Adrianople	25 Arrogant	25 ——— at P. M.
26 — Ardagh —	26 Ætna	26 Asia	26 ——— on Thursday at A. M.
27 — Ardfer —	27 Africa	27 Ardent	27 ——— at P. M.
<b>3.</b>	<b>3.</b>	<b>3.</b>	<b>3.</b>
31 — Armagh	31 Agincourt	31 Achilles	31 ——— on Friday at A. M.
32 — Acowry	32 Aix la Chapelle	32 Adamant	32 ——— at P. M.
33 — Aghadoe	33 Albany	33 Assistance	33 ——— on Saturday at A. M.
34 — Clogher	34 Alcantara	34 Acteon	34 ——— at P. M.
35 — Clonfert	35 Aleppo	35 Argo	35 Alarming Intelligence is re-
36 — Cloyne	36 Alexandria	36 Arto's	ceived from —
37 — Connor	37 Algiers	37 Assurance	37 Acquaint the Commissioner's
<b>4.</b>	<b>4.</b>	<b>4.</b>	<b>4.</b>
41 — Dublin	41 Alicant	41 Arethusa	41 — principal Magistrates at —
42 Archbishop of —	42 Alps	42 Æolus	42 — the High Sheriff at —
43 — Dublin	43 Alsace	43 Active	43 — the Secretary of War
44 — Armagh	44 Antrim	44 Alarm	44 ——— of State
45 — Cashel	45 Aylebury	45 Amazon	45 Agreeable to the Orders of his
46 — Cork	46 Ayrshire	46 Ambuscade	Majesty
47 — Canterbury	47 All Saints	47 Amphion	47 — of the Lord Lieutenant.
<b>5.</b>	<b>5.</b>	<b>5.</b>	<b>5.</b>
51 — Tuam	51 Alnwick	51 Apollo	51 — of Government
52 — York	52 Aloft.	52 Aitrea	52 — of the Commanding Officers
53 Admiral of	53 Alresford	53 Alcmena	at —
54 — the Fleet	54 Aldborough.	54 Andromache	53 — of the Magistrate of —
55 — the White	55 Alençon	55 Albemarle	54 Agreeable to your Orders
56 — ditto 1	56 Andes	56 Aurora	55 All is well
57 — ditto 2	57 Anglesey	57 Amphitrite	56 Alter your Telegraphs to Black.
<b>6.</b>	<b>6.</b>	<b>6.</b>	<b>6.</b>
61 — ditto 3	61 Angola	61 Ariadne	57 ——— to White
62 — ditto 4	62 Anjou	62 Alfred	61 Admit no S. rangers
63 — ditto 5	63 Anhalt	63 Aralanta	62 Admiralty has issued Orders.
64 — ditto 6	64 Antioch	64 Ariel	63 — has received Intelligence.
65 — ditto 7	65 Antwerp	65 Allegiance	64 Arms been found hidden at —
66 — ditto 8	66 Archangel	66 Albany	65 Army approaches in Number
67 — ditto 9	67 Argenton.	67 Alderney	66 An armed Mob at —
<b>7.</b>	<b>7.</b>	<b>7.</b>	67 Art. of Capitulation agreed to.
71 — ditto 10	71 Atherstone	71 Alert	<b>7.</b>
72 — ditto 11	72 Ardee	72 Alligator	71 Assistance is required at —
73 Admiral of	73 Arklow	73 Avenger	72 Appearances are against —
74 — the Blue	74 Armagh	74 Ætna	73 Answer my last to-morrow
75 — ditto 1	75 Athenry	75 Aleto	74 Arrived since my last at —
76 — ditto 2	76 Athlow	76 Aquillone	75 Arrived News from E. Indies
77 — ditto 3	77 Athy	77 Argus	76 Arrived Mails from —
			77 Agreeable Intelligence is re-
			ceived



## S U P P L E M E N T.

SINCE the Royal Irish Academy did me the honour to accept of my essay on the Telegraph, I have made material improvements in its construction, which I think it my duty to communicate.

In September 1796, the Lord Lieutenant ordered me to prepare Telegraphs for an experiment before his Excellency. In consequence I constructed four new Telegraphs.—I had found that the large machines thirty feet high, with which my sons talked, in September 1794, across the channel between Ireland and Scotland, were liable to accidents in stormy weather: my first consideration therefore was to contrive some means of furling their canvas when they were not in use; and, from the rigging of ships, it was obvious that cordage was for this purpose preferable to inflexible braces of wood. I therefore adopted the following construction:

A. (fig. 4. Plate XIV.) a hollow axle-tree made in separate pieces hooped together in the form of a double truncated cone, on the middle of which is fastened a wheel of wood (b, fig. 4) with eight notches cut out (a, fig. 4) to receive eight ribs (r. r. r. r. r. r. r. r. fig. 1 and 2.) These ribs turning on a strong iron ring, shut up like the ribs of an umbrella, and are raised and adjusted by cords passing through eight holes in the flanches or shoulders (F fig. 1. 4.) These flanches and those at (f. fig. 2 and 4) serve to keep the machine in its place upon the stands which support it (fig. 2); the cords are strained and fastened like the cords of a tent (c. c. &c. fig. 2.)

Where permanent buildings are not required, supports for these machines may be constructed in the following manner. Two stands, each of them made of two pieces of wood simply bolted together, as (fig. 3), must be erected, and held steady by means of cords (c. c.) fastened to common tent pegs, as in (fig. 2 P. P. P.) When the machines are large, small piles should be used instead of pegs, and running tackle (t.) should be used both for the cords of the pointers, and the stands. A number of minute circumstances should be attended to in the construction and use of these machines; but I do not think it proper to detail them to this Academy; they should appear in a different place\*.

Besides rendering the Telegraph safe against storms, and more easily manageable, I found by experience that one machine could be made to perform the same effect as four, with but little loss of time; what took up four minutes with four pointers, can be conveyed in five minutes by one. I have also found that, by answering each signal or number shown at every station, all possibility of mistake is avoided.

I believe that, in other establishments of this sort, it has been found that thick and foggy weather has occasioned more interruptions than were expected. With my Telegraphs, I

\* Formerly, in France, every engineer who conducted any public work, was obliged to lodge in a public office exact drawings, with minute descriptions, of every part of and process of his operations. Numberless small improvements in workmanship and tools were preserved by these means, and by degrees were collected into publications of general circulation.

I was required to deliver drawings of all the machinery I employed in the work carried on at Lyons in 1772, for turning the course of the Rhone:

But, in the transactions of a literary Society, such details would be tedious and improper.

have good reason to assert, that there do not commonly occur above eight or ten days in the year when intelligence might not be conveyed by land.

If eight men were posted at each permanent station, at the distance of eighteen or twenty English miles asunder, with machines of twenty-five feet high, in hazy weather they might detach two men with portable Telegraphs, to the distance of about six miles from each station, who, with eight foot Telegraphs, could keep up a regular communication.

The portable Telegraph resembles that which I have described; it differs only in two circumstances: for convenience, as it is small, and does not oppose much surface to the wind, it may be distended with ribs of wood instead of cords.

The portable Telegraph which my son had the honour of showing to his Royal Highness the Duke of York in Kensington gardens, in October last, was furnished with silken cords, on purpose to shew how my larger Telegraphs were constructed; but it was intended merely for reconnoitring near an army, and was only six feet high.

In the essay which the Academy has already received, I said that imitations without end might be made of my Telegraph. Every index or pointer that moves circularly, dividing an imaginary circle into parts, and denoting figures or signs that correspond with a vocabulary, is founded on the same principle as mine. The French have laid aside their former clumsy apparatus, and have constructed a Telegraph on these principles; and the Admiralty in England have, as I am informed, very lately done the same.

The first pointers I employed in 1767 were windmill sails. I then tried indexes of the shape (fig. 5.) Fig. 8. A pointer, like a sword-cutler's sign, was recommended to me by a member of the Academy, as a second or additional hand to move on the same centre as the principal hand. But a triangle, whose base is equal to half its side, is, of all the figures I have tried, the most distinct.

The Night Telegraph remains still to be described; its uses are perhaps more extensive than those of the Telegraphs I have already published; and I propose to make it the subject of another paper upon a future occasion.

The art of conveying swift and secret intelligence is not one of those inventions which attracts attention only by its novelty; on the contrary, I am convinced that it will be thought more valuable, the longer it has been submitted to the test of time and experience.

#### SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

COPENHAGEN, July 12, 1798.

**T**HE Royal Society has proposed the following Prize Questions. The Prize for each is a Gold Medal, value one hundred rixdollars (21. 10s.)

I. *History*. What nations discovered America before the Norwegians, and performed voyages by sea to this part of the globe? How far did the Norwegians extend their discoveries in America, particularly to the southward? What conclusions may be deduced on these points, either from decisive reasons or simple conjectures, from the writings and monuments which still subsist, such as forts, buildings, languages and traditions?

II. *Mathematics*. To find the function of all the quantities, which serve conjointly to determine the magnitude of the calorific effect of every combustible material in common use;

use; such as wood, turf, and pitcoal, whatever may be their peculiar characters in other respects. The required equation must be determined for at least four different cases:

*Case 1.* Where the wood, turf, or coal is burned in a furnace to heat a certain volume of air, as for example, that of a chamber.

*Case 2.* Where the heat is employed in producing the state of ebullition, in a fluid proper to boil certain substances plunged in it.

*Case 3.* Where the heat is employed to harden soft matters, such as clay for tiles.

*Case 4.* And where certain hard or consistent matters, such as metals, are required to be liquified by heat in a furnace or forge. Each of these equations grounded on various experiments must be found and established analytically, so that it may be possible to determine with precision the ratio of the calorific effect, and consequently the degree of utility in economical applications of every kind of wood, turf, or pitcoal.

III. *Natural Philosophy.*—To find by experiment the greatest degree of heat which water in the state of vapour can communicate to other bodies; and to answer the question, whether that part of the water in Papin's digester, which is not converted into steam, can acquire a more elevated temperature than  $212^{\circ}$  of Fahrenheit.

IV. *Philosophy.*—What are the most remarkable degrees, through which practical philosophy has passed from the time in which it was first treated systematically to the present time.

The memoirs in answer to these questions are required to be written in Latin, Danish, or German, and sent before the end of June 1799, to the Secretary of the Society, Professor Abilgaard.

BERLIN,

The Physical Society at Berlin has announced the following Question, for a Prize of 20 Holland Ducats. (9l. 5s.)

ADMITTING that Electricity is a necessary Agent for the formation of Hail, are there any grounds for concluding, that the Electrical Cloud can be rendered incapable of generating Hail, as Lightning is prevented by Conductors? What are the means to be adopted for this purpose, and the facts or observations in general upon the subject which deserve notice?

Proceedings of the Association for promoting the Discovery in the interior Parts of Africa, &c.

Abstract of Mr. Park's Travels. [Continued from page 283.]

INFORMATION of a considerable river flowing through the centre of Africa, between the latitude of  $15^{\circ}$  and  $20^{\circ}$  north, had been received at very early periods from different quarters, which at different times was supposed to be part of the Senegal and of the Gambia. Further enquiries, however, though they confirm the ancient accounts, shewed that this river was not only of greater magnitude than either the Senegal or Gambia, but flowed in a contrary direction, running not to the westward into the Atlantic, but from west to east, to regions unknown. The Moors described it by the name of Nil il Abeed, or the River of Slaves: the Negroes bestowed upon it the appellation of Joliba, or the Great Waters. Something, however, of doubt still remained, particularly with regard to its eastern

eastern direction, which was not received by geographers without difficulty and hesitation. Mr. Park's testimony is decisive, not only that the two names denote the same stream, but also that it flows from west to east, which he ascertained by a perilous ambulation of some hundred miles along its bank. All question respecting the source, the existence, and the direction of this great river is therefore obviated, but its termination still remains unknown.

The city of Sego, at which Mr. Park now arrived, consists of four divisions or quarters, two on each side of the water. The houses are built of clay with flat roofs: some of them have two stories, and many are white-washed. Moorish Mosques are seen in every quarter. These objects, with the numerous boats on the river, a crowded population, and the cultivated state of the surrounding country, formed altogether a prospect of civilization and magnificence, which our traveller little expected to find in the bosom of Africa. From the best enquiries he could make, he had reason to believe that Sego contained thirty thousand inhabitants.

The boats on the Niger are of a singular construction, each being formed of the trunk of two large trees, hollowed out and joined together endways, the junction being exactly across the middle of the boat. They are, therefore, very long and narrow, and have neither decks nor masts. Mr. Park proceeded to the ferry, in order to cross to the King's residence, which was on the other side; but before he could obtain a passage, the King had sent to enquire concerning the object of his journey. Mr. Park gave the best answer he could, adding, that he had been robbed of all he possessed, and implored the King's bounty and protection. The messenger told him to go to a distant village, which he pointed out, and wait for the King's further orders. He found the inhabitants either afraid or unwilling to give him lodging or entertainment, and having turned his horse loose, he sought shelter from a storm of thunder and rain under a tree. At length as night approached, that kindness and humanity inherent in the female sex, to which he had often been indebted on former occasions, came to his relief on the present. A poor Negro woman returning from the labours of the field, observed that he was wet, weary, and dejected; and taking up his saddle and bridle told him to follow her. She led him to her cottage, lighted up a lamp, procured him an excellent supper of fish, and plenty of corn for his horse; after which, she spread a mat upon the floor, and said he might remain there for the night. For this well-timed bounty our traveller presented her with two of the four brass buttons which remained on his waistcoat.

Mr. Park adds other particulars concerning his benefactress, which heighten the picture. He relates, that the good woman having performed the rites of hospitality towards himself, called in the female part of the family, and made them spin cotton for a great part of the night. They lightened their labours by songs: one of which must have been composed extempore, for our traveller was himself the subject of it; and the air was, in his opinion, the sweetest and most plaintive he ever heard. The words, as may be expected, were simple; and may be literally translated as follows: "The winds roared, and the rain fell. The poor white man faint and weary, came and sat under our tree. He has no mother to bring him milk; no wife to grind his corn."—Chorus—"Let us pity the white man, no mother has he, &c. &c." Simple as these words are, they are natural and affecting; and contain a curious allusion to the state of manners in savage life, in which the women perform all the domestic duties.

He continued all the next day in the village without receiving any orders from the king, and found himself the object of universal observation and enquiry. He soon heard enough to convince him that the suspicions of the Moors and slave-traders residing in Sego were very inimical to him; and that many consultations had been held with the king concerning his reception and disposal. On the third day however the messenger arrived with a present of five thousand kowries, to enable him to purchase provisions in his journey from Sego, from the vicinage of which he was commanded to depart immediately. From various circumstances, it seemed probable that the king would have admitted Mr. Park into his presence, if he could have protected him against the malice and fanaticism of the Moors, and if the story of Mr. Park had been more comprehensible by men, who could not conceive how the desire of extending knowledge could have induced him to travel through such an extent of country, without other views of a more objectionable nature. From other enquiries it was also rendered certain, that our traveller would have exposed himself to certain destruction from the Moors, if he had ventured to proceed much further to the westward without the protection of some leading man among them, which he had no means of procuring. Notwithstanding these discouraging circumstances, he determined to endeavour to penetrate further along the banks of the Niger. The first town of note at which he arrived after leaving Sego, was called Kabba. This town is situated in the midst of a beautiful and highly cultivated country, bearing a greater resemblance to the centre of England than Mr. Park could have supposed to have been in the middle of Africa, and the season was that of the shea harvest, or gathering in the fruit which produces the shea-toulou or tree-butter, the great abundance of which in this quarter was astonishing. The growth and preparation of this commodity seem to be among the first objects of African industry, in this and the neighbouring states, and it constitutes a main article of their inland commerce. From Kabba, Mr. Park and his guide proceeded to Sansanding, where notwithstanding his endeavours to avoid the notice of the Moors, he underwent their insults for a considerable time.

Leaving this place early in the morning, he proceeded to a town called Nyara, and thence to Modiboo, a delightful place on the banks of the river, which is here very broad and enlivened with many small and verdant islands, all of them stocked with cattle, and crowded with villages. Here he was again compelled to set off abruptly for fear of the Moors. The limit of his expedition to the westward was Silla, a large town on the southern side of the Niger, in  $1^{\circ} 24'$  West of Greenwich, and latitude  $14^{\circ} 48'$  North, where, from the aggregate of unfavourable circumstances, it became absolutely necessary for him to return.

The town of Silla, whence Mr. Park began his journey homeward, is within two short days journey of Jenné, which is situated on an island in the river. At the distance of two days more, the river empties itself into a considerable lake called Dibbie, (or the dark lake) concerning the extent of which, all the information which could be obtained was, that, in crossing it from west to east, the canoes lose sight of land one whole day. From this lake the water issues in many different streams, which terminate in two large branches, one whereof flows towards the north-east, the other to the east; but they join again at Kabra, one day's journey to the southward of Tombuctoo, and the port, or shipping place, of that city. The tract of land which the two streams encircle, is called Jinbala, and is inhabited by negroes; and the whole distance by land from Jenné to Tombuctoo, is twelve days journey.

From Kabra, at the distance of eleven days journey down the stream, the river passes to  
the

the southward of Houssa, which is two days journey distant from the river; and so far our information seems to be authentic. Of the further progress of this great river and its final exit—whether it be the same which passes by Kaffina\*—whether (as ancient charts seem to indicate) it spreads into one or more inland lakes; or, at an immense distance, intermixes with the waters of the Egyptian Nile—these are questions which future discovery can alone resolve. On each of these points enquiry of the natives was not neglected, but satisfactory and certain information could not be obtained.

Such is the intelligence that was collected by Mr. Park concerning the course of the Niger from its leaving Sego, where he first discovered it. Of the chief towns of Jenné, Tombuctoo, and Houssa, situated on its banks, the last was said to be the most considerable, and the least of them of far greater magnitude than Sego. But there is a place between Jenné and Tombuctoo deserving notice, as containing a very considerable pottery: it is called Downie; and the earthen ware, which Mr. Park frequently met with, appeared to be of extraordinary good consistency, but not glazed. He was told that caravans frequently arrive both at Tombuctoo and Houssa, from the countries on the Mediterranean, travelling across the Desert, by the way of Fezzan, with European goods and other merchandize. By one of these the news was conveyed to the centre of Africa of the capture, by the French, of the Mediterranean convoy, in October 1795. Mr. Park received this information from a Moor who had come from Fezzan.

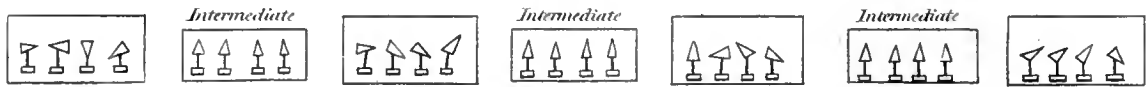
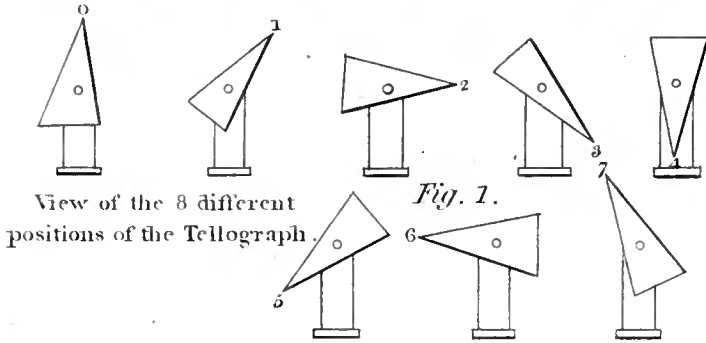
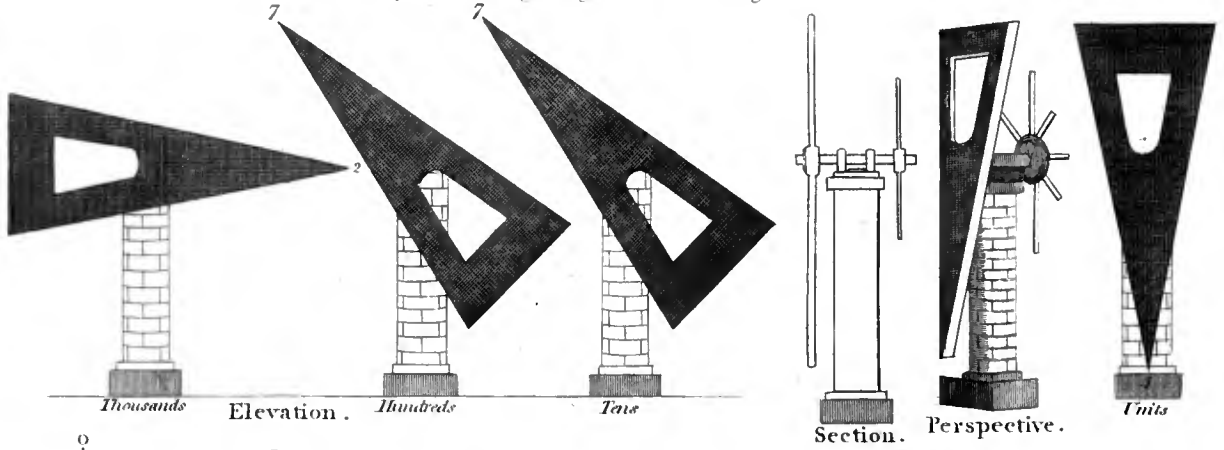
On his return back, Mr. Park learned that the sovereign of the country had given orders to seize him, for which reason he avoided the town of Sego. His course, in ascending the river, was to the south-west; and his subsistence, in travelling through the territories of the benevolent Negroes, was in a great measure afforded him by the Dooty, or chief man of the towns through which he passed, it being, highly to the credit of the African police, part of the duty of this officer to provide food for the necessitous traveller. On many occasions he offered to pay for what he received out of the kowries, which had been presented him by the king, and his offer was sometimes accepted and sometimes refused. On other occasions he rewarded his host in a manner which, from its singularity, deserves to be noticed. Among the various impostures practised by the Moors towards the poor Negroes, they frequently sell them scraps of paper with an Arabic inscription (commonly a passage from the Koran) which are called *saphies* or charms. With one of these about his person, the fond possessor conceives himself invulnerable: neither the lurking serpent nor the prowling tiger is any longer the object of his dread.

In the circumstances to which Mr. Park was reduced, he had the good fortune to discover that the negro natives ascribed to him the power of granting *saphies* of more than Arabic virtue. “If a Moor’s *saphie* is good (says the Dooty of Sanfanding), a white man’s must needs be better;” and Park, at his request, gave him one possessed of all the virtues he could concentrate, for it contained the Lord’s Prayer. The pen with which it was written, was made of reed; a little charcoal and gum-water made very tolerable ink, and a thin board answered the purpose of paper. In his journey westward, this merchandize turned to extraordinary good account; and it is surely needless (says Mr. Edwards) for Mr. Park to frame any apology for having availed himself of such a resource in his situation.

\* Erroneously spelt in the former memoirs, Cakhna.

# Art of conveying Intelligence?

*Philos. Journal, Vol. III. XII. facing p. 334.*



Example of a line of Six words described by the Tellograph.

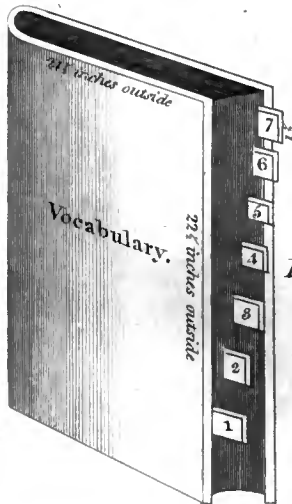
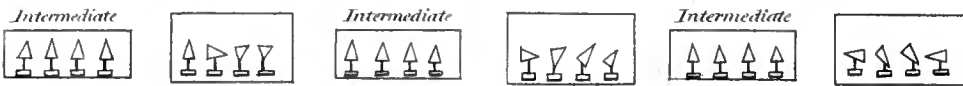


Fig. 3.

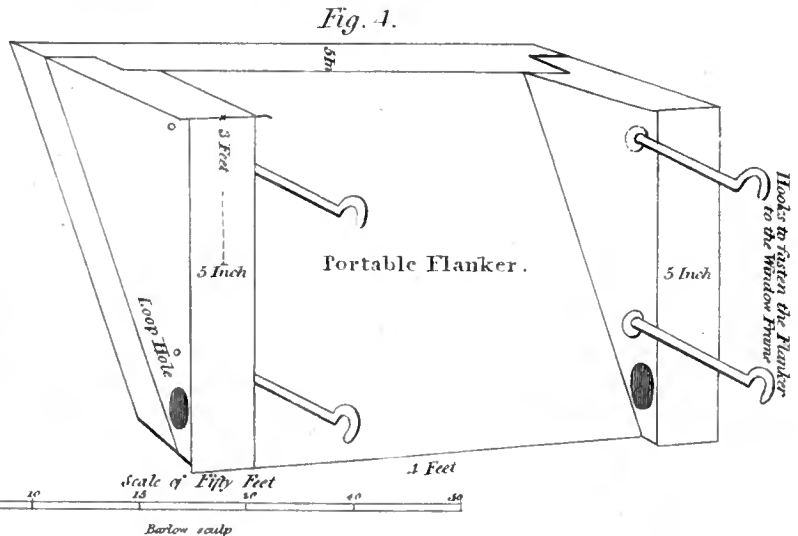
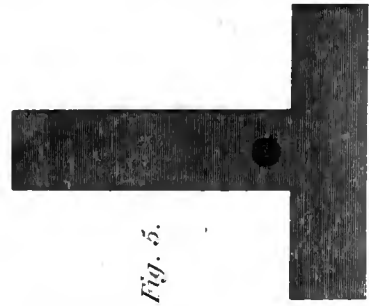
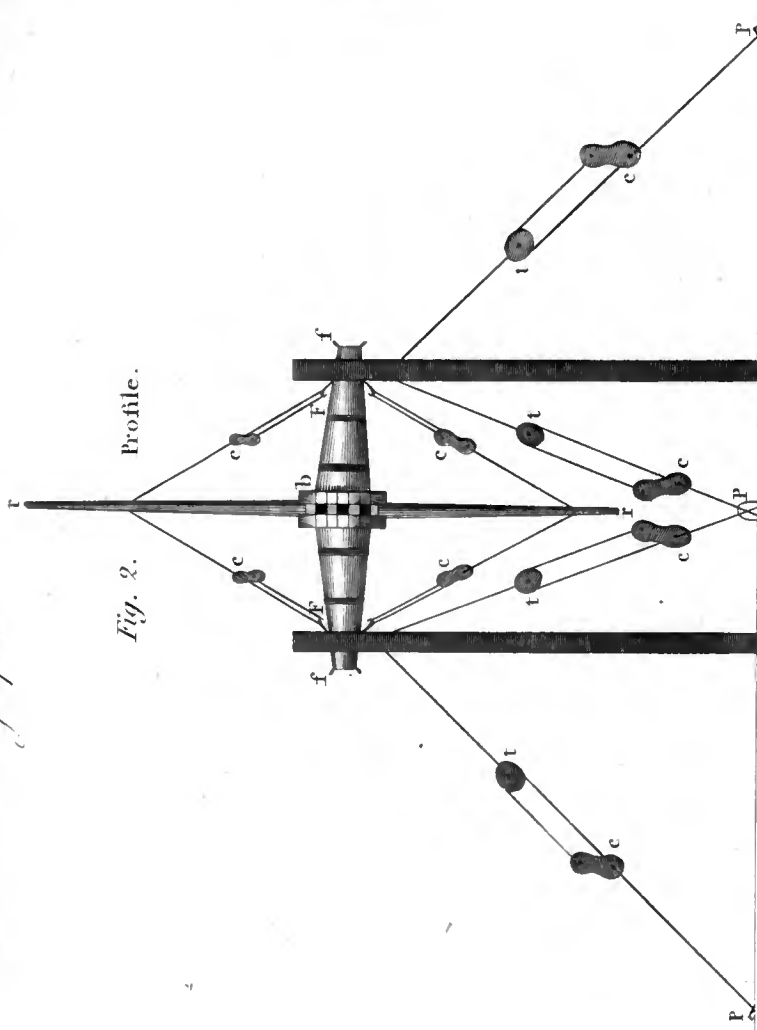
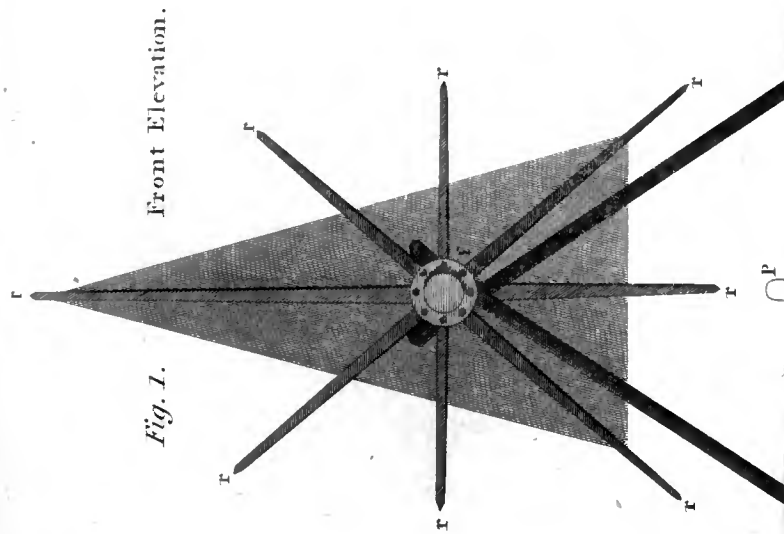


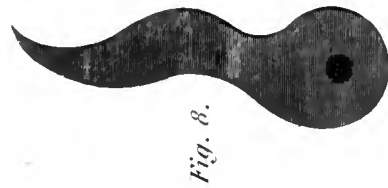
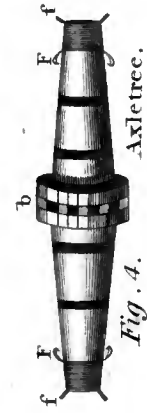
Fig. 4.







*Barlow sculp.*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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NOVEMBER 1798.

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ARTICLE I.

*An Account of three different Kinds of Timber-Trees, which are likely to prove a great Acquisition to this Kingdom, both in point of Profit and as Trees for Ornament and Shade.*  
By CHARLES WHITE, Esq. F.R.S.\*

IN making a collection of such hardy trees and shrubs as would grow and even flourish in the open air, at Sale, in the county of Chester, I soon observed that there were three forest trees, of different genera, which grew much faster than the others in the same soil and situation, viz. the Black American Birch with broad leaves, the Athenian Poplar, and the Iron Oak with prickly cups.

The Broad leaved American Black Birch, *Betula nigra* Linn. Spcc. Plant. 1394, is described by Mr. Aiton in his Hortus Kewensis: *B. foliis rhombo-ovatis, duplicato-ferratis, acutis, subtus pubescentibus, basi integris; strobilorum squamis villosis; laciniis linearibus, equalibus*. It is a native of Virginia and Canada, and was first introduced into England (where it grows in the greatest luxuriancy, and perfects its seeds) by Peter Collinson, Esq. in the year 1736. There is no doubt, therefore, that it will soon become very plentiful and cheap. It is very desirable in pleasure-grounds, as it is the first forest tree in the spring which presents us with its leaves; these are of a light and lively green. Its bark, which is white, makes at all times a beautiful variety when intermixed with other trees. It is said to be the most useful timber tree in North America for building both of houses and boats, and will grow fast in any soil or situation, whether wet or dry.

\* Manchester Memoirs, vol. v. part 1.

Miller, speaking of trees of this description, says "that they may be propagated by seeds, in the same manner as the common birch-tree, and are equally hardy. Some of the trees now begin to produce their catkins in England, so that we may hope to have plenty of their seeds of our own growth, for at present we are supplied with them from America. As these grow more vigorously than the common sort, and thrive on the most barren ground, they may be cultivated to great advantage in England; for their wood is much esteemed in Canada, where the trees grow to a large size: and they are by no means an unsightly tree in parks; for their stems are straight, the bark smooth, and their leaves are much larger than those of the common birch; so may be planted in such places where few other trees will thrive."

Mr. Hanbury says: "The black Virginian birch, being of foreign growth, is propagated for wilderness and ornamental plantations; but as it begins now to become pretty common, it is to be hoped it will soon make a figure among our forest trees, it being equally hardy with our common birch, and will arrive at a much greater magnitude. This species will grow to be upwards of sixty feet in height. The branches are spotted, and more sparingly set on the trees than the common sorts. The leaves are broader, grow on long footstalks, and add a dignity to the appearance of the tree; and as it is naturally of upright and swift growth, and arrives at so great a magnitude in few years, prudence will direct us to let it have a share among our forest trees, to plant them for standards in open places, as well as to let them join with other trees of their own growth, in plantations more immediately designed for relaxation and pleasure." I planted one of these trees nineteen years ago, and it is now forty-five feet six inches in height, and three feet seven inches in the girth.

*The Athenian Poplar Tree, Populus (Græca) foliis cordatis, glabris, basi glandulosis, remote crenatis; petiolis compressis; ramis teretibus.* The Athenian poplar is a native of the islands of the Archipelago, and was first cultivated in England by Hugh duke of Northumberland in the year 1779. Perhaps there is no deciduous tree so beautiful, or so proper for pleasure-grounds intended for ornament and shade, as this poplar; having a fine upright stem; the branches well disposed; the bark smooth, and of a silvery hue, resembling satin-wood. The leaves, which are of a light green, are produced very early in the spring, and are retained on the tree longer than on any deciduous tree in this country, not falling off till late in the autumn: they are never blighted nor infested with insects, nor does it lose a leaf during the whole summer. Though the poplar is generally termed an aquatic, this will grow in any soil or situation; and is of quicker growth in dry upland than any tree we are acquainted with in this climate, though not quite of such quick growth as the Huntingdonshire willow in rich moist meadow land. In such a situation, I have fallen a Huntingdonshire willow\*, from which I made a staircase when it was only of nineteen years growth, from a cutting. The Athenian poplar is propagated with the greatest advantage by suckers and layers; but it is with great difficulty raised from cuttings or truncheons. The common way of raising them, amongst the nurserymen, is, by engrafting them on some other poplar: but the trees thus raised are of little value, being very slow in their growth; and it is owing to this circumstance, perhaps, that their real

\* I cannot find that this species of willow has been described by any botanical writer; but it is well known among the nurserymen by this name.

worth has not before been discovered. About twelve years ago I purchased two plants of this poplar, from two different nurserymen in London, at one guinea each; one of them was grafted upon a different kind of poplar, the other was upon its own roots. I placed them near together, in a dry situation, in a light soil, underneath which was a stratum of gravel. The grafted one made very little progress; I therefore converted it into a stool, and raised several plants from it. The other, which is upon its own roots, has made a rapid progress, being at least fifty-one feet high, and two feet nine inches in the girth. It produces annually a great number of suckers, with which I have supplied many of my friends\*.

The third is *The Iron, Wainscot, or Turkey Oak*, so called by Mr. Luccomb. I have long been in doubt what species of oak this really was; but one of mine having borne some acorns this year, has ascertained it to be a variety of the *quercus cerris*; and it appears to me to be either a *non-descript*, or what Mr. Aiton, in his *Hortus Kewensis*, calls *frondosa: foliis ovato-oblongis, leviter sinuatis, planiusculis*: commonly, Turkey oak tree. It is what Mr. Luccomb generally grafts his Luccomb oaks upon; and the plants certainly grow faster when grafted upon this oak than upon any other. About twenty years ago, in making a collection of oaks, I received several from Mr. Luccomb, both of the iron and the Luccomb oak; but I soon found that the iron oak overgrew all the others, and was equally ornamental as the English oak. From a branch which I have sawed off, the wood appears to be as hard and as ponderous as the English oak.

The following is an account of the size and age of some Iron, Luccomb, and English Oaks, growing in my collection at Sale:

			Height.			Girth.	
			Feet.	Inches.		Feet.	Inches.
An iron oak, 20 years old	—	————	36	0	—	3	3
Another, of the same age	—	————	37	0	—	3	0
A Luccomb oak, of the same age, grafted on an							
English oak	—	————	32	2	—	2	5
An English oak, of the same age		————	28	0	—	2	6
Another, 40 years old	—	————	39	0	—	2	10
Another, 56 years old	—	————	54	0	—	3	4

The following is a Copy of a Letter from Mr. Luccomb to Mr. Babington, dated New-bridge, Exeter, September 17, 1795:

“All I can say of them (the iron oaks) is, that my father had a few of them as a present from William Ball, Esq. of Manhead-house (now Lord Lefburne’s, near Chudleigh, Devonshire), about fifty years since, by the name of the iron or wainscot oak, which Mr. Ball received from Turkey by one of his own ships trading there. They are the same sort which you have noticed at Hillersdon; as my father sold some of them to Mr. Creroy about forty years since. They have, as you observe, a very jagged leaf, and the cup of the acorn is rough like a bur. They are not evergreen.”

\* There is another poplar of very swift growth, which makes a very handsome tree, and will flourish in any situation or soil. It is the *Populus cordifolia canadensis*, or Berry-bearing Poplar, as it is commonly called. This tree will grow freely from cuttings.

The following is a Letter I received from my worthy friend the Rev. Thomas Gisborne, author of several useful publications :

" DEAR SIR,

" Yoxall Lodge, Oct. 20, 1795.

" I have this evening received a letter from my brother-in-law, Mr. Babington, respecting the measurements of the iron oaks at Hillersdon, near Cullompton, where he now is: I subjoin what he says on the subject, and have pleasure in finding the result so honourable to the tree which you recommend.

" I am, Sir, &c.

" THOMAS GISBORNE.

" ——— To-day I have measured some of the oaks about three feet and a half from the ground; and give you the result, which I thought would be fair and satisfactory, in the following way: —

" No. of English oaks.

			Circumference.	
			Feet.	Inches.
8	=====	=====	31	1
9	=====	=====	30	1
9	=====	=====	32	8
<hr/>				
26			26) 93	10
			<hr/>	
	Average circumference		3	7½

" No. of iron oaks.

			Circumference.	
			Feet.	Inches.
6	=====	=====	31	6
6	=====	=====	32	10
6	=====	=====	31	9
<hr/>				
18			18) 96	1
			<hr/>	
	Average circumference		5	4

" As circles are as the squares of their circumferences, pieces of the butts, at this height, a foot long, would be to each other as 1877 : 4096. Now, supposing the iron oaks to carry their butts as much higher than the others as their substance below would lead us to expect (and they seem, in fact, to do this or more), there must be four or five times as much wood in them as there is in the English oaks. An old labourer here informs me, that all were planted at the same time, between forty and fifty years since. They stand in rows, ten feet asunder, and the trees are twenty feet from each other in each row. I measured such trees as first presented themselves, with the exception of one or two which seemed unhealthy. They are on a steep bank and a gravelly soil. The trunks of the iron oaks are covered with a lighter moss, on the whole, than fixes itself on the English oak; but make a fine appearance. I measured many outside trees, and observed that the iron oak seemed to have as great a superiority over the other, in this situation, as it has when surrounded by neighbours. As to the height, the iron oaks very generally out-top the others, and are the master trees; but you know that in a plantation a slender tree will often be nearly as tall as its sturdy neighbour.

" P.S.

" P. S. On looking over the oaks again, I think the iron oaks carry up the thickness of their butts a good deal better, *ceteris paribus*, than the others; and therefore they have five or six times the quantity of wood in them.

" There are but two beeches, and they are both outside trees, and therefore larger than they otherwise would be. The circumference of the two was 12 feet 4 inches; average, 6 feet 2 inches.

" Spanish chefnuts, No.

			Circumference.	
			Fect.	Inches.
6	_____	_____	31	3
8	_____	_____	34	3
4	_____	_____	24	3
18	_____		18) 89	9

Average circumference 4 11 $\frac{3}{4}$  nearly."

N. B. Mr. Babington says, " that there are gates and pales on the premises at Hillersdon, which have been made of the iron oak; and that, as far as he can judge, the wood appears as hard and as tough as that of the common oak."

It has always been considered, that when men have planted oak, they have not planted for themselves, or for their children, but for distant posterity; and even *they* could never be repaid where land bore any annual value: and to the planter himself little pleasure could arise from trees of such very slow growth. But the same person who plants the iron oak may possibly live to reap some little profit as well as pleasure; and it is not at all unreasonable to suppose his immediate successor may see it arrive to some degree of perfection. From what I have seen of the wood of this oak, and from the account given by Mr. Babington of the gates and pales made with it, there is great reason to suppose it will be equally useful as the English oak for any purpose whatever.

The general decrease of timber in this island, the many waste lands unemployed, and the bill now proposed to be brought into Parliament by that great friend to agriculture Sir John Sinclair, will be my apology for troubling the Society with this paper; for the planter ought certainly to be furnished with every advantage, and every possible inducement should be held out to him for promoting so useful and so national a work.

#### Explanation of Plate XV. fig. 3.

A.a.a. Leaf, acorn, and prickly cup of the Iron, Waincot, or Turkey Oak.

B.B.b.b. Leaf, acorn, and prickly cup of the *Quercus Cerris* of Linnaeus, fol. oblongis, lyrato-pinnatifidis; laciniis transversis, acutis, subtus subtomentosis; calyce hispido; glunde minore. Small-acorned Spanish Oak with prickly cups.

## II.

Abstract of a Memoir of M. PROUST on the Tanning Principle\*. By Citizen DESCOTILS.

A SOLUTION of muriate of tin, at any degree of oxydation whatever, being poured into a decoction of nut-gall, forms an abundant yellowish precipitate. After having di-

\* Annales de Chimie, xxv. 225.

luted this mixture with a quantity of water sufficient to separate the last portions of precipitate which the acids may hold in solution, it is filtered, and the liquor contains the acids of galls, muriatic acid, and muriate of tin. The precipitate is a combination of the tanning principle, and the oxyde of tin. It cannot contain the gallic acid, because this acid is not precipitated by tin. The solutions of lead, on the contrary, precipitate the gallic acid, as well as the tanning principle; and on this account it was that Scheele could not separate the acid of galls from the principle of astringency.

In order to obtain the acid of galls, the tin must first be separated by sulphurated hydrogenous gas. The sulphurated oxyde falls down in the form of a brown powder. After having left the fluid exposed to the sun, with a covering of paper, in order that the excess of sulphurated hydrogen gas may be dissipated or entirely decomposed, it is to be filtered, and then evaporated, in a vessel of silver, to the requisite point for the crystallization of the gallic acid by cooling. The crystals being then thrown on a filter must be washed with a small quantity of cold water. The remaining fluid is to be treated by evaporation, and cooling in the same manner, in order to deprive it as much as possible of gallic acid. At the end of the operation, it is necessary to use capsules of glass, because the muriatic acid becomes concentrated. The earths which some chemists have pretended to exist in the decoction of nut-galls ought to be found in this residue. Mr. Proust met with none; but he proposes to examine the residue with more particular attention to this point.

To separate the tanning principle from its combination with the oxyde of tin, this last powder is diffused in a quantity of water, through which a current of sulphurated hydrogenous gas is passed. The sulphurated oxyde falls down, and the pure tanning principle at the same time becomes dissolved in the water. After filtration, the solution is evaporated in a silver basin, in which it acquires the deep colour of a decoction of nut-galls, and its peculiar smell, which is one of the distinctive characters of the astringent principle. Its taste is exceedingly acerb, and rather bitter, without being disagreeable. It lathers like soap-water, without feeling unctuous to the touch. It becomes turbid by cooling, and lets fall a light brown powder, which is dissolved again by heat. It resembles the last decoctions of nut-galls, which do not become mouldy, and are thought by Mr. Proust to contain the tanning principle nearly in a state of purity.

The substance which remains after its evaporation is dry, brown, friable, breaks with a vitreous fracture, like aloes, and does not attract the humidity of the air. Its taste is very rough and harsh: it is totally soluble in hot water, and still more readily in alcohol.

All the acids precipitate the tanning principle from its aqueous solution, by uniting with it. As this fact belongs to the analysis of galls, which Mr. Proust has made by means of the sulphuric and muriatic acids, he promises to treat of it again in the second part of his work.

The aqueous solution of the tanning principle, poured into a solution of glue, immediately converts it into a magma, which possesses the elastic properties of the gluten of wheat. This substance, when left to dry, contracts in its dimensions, and, when perfectly dry, has the appearance of a brown mass, vitreous in its fracture, nor capable of putrefaction, perfectly insoluble in water, yielding little to alcohol, and capable of resuming its elastic property by softening it in hot water. It is, in a word, says the author, the combination discovered by Seguin, the preserving principle of tanned leather. It is also obtained with the solution of the nut-gall.

Albuminous



Albuminous liquors are precipitated by the tanning solution, but the result is not a magma susceptible of the same concentration.

The green sulphate of iron is no more altered by the tanning principle than by the acid of galls\*.

The red sulphate, on the contrary, is precipitated in a somewhat tarnished blue deposition, which is abundant, coarse, and of a black colour when dry. If the precipitate which this sulphate affords with the gallic acid be compared with this, it will be seen that they greatly differ. The former is of an extreme subtlety, and remains long suspended in water: it is perfectly black, &c.

The gallate of iron is soluble in acids; the tannate of iron is decomposed by those salts. It abandons its iron, and the tanning part falls down.

If the red sulphate be poured, rather in excess, into a solution of the tanning principle, the disengaged sulphuric acid re-dissolves the precipitate, and affords a black fluid in its greatest intensity, which is blue when much diluted. To separate the precipitate, without depriving it of the tanning principle, it is necessary that the excess of acid in the fluid should be gradually saturated by pot-ash. With a little attention, the operator may succeed in rendering the liquor colourless, without affecting the superabundant portion of sulphate; or otherwise, if a small portion of oxide of iron has been thrown down along with the black precipitate, which may be known by the rust which is formed upon the filter, a few drops of acid will re-dissolve it.

But it is a remarkable fact, that all the red sulphate remaining in the fluid is brought back to the state of green sulphate. The quantity of oxygen which constitutes the difference between the one and the other, is seized by a portion of the tanning principle. The latter thus oxydized, and by that means rendered incapable of precipitating the red sulphate, remains in solution. This oxydation of the tanning principle is directly and easily obtained by the oxygenated muriatic acid. The solution of the tanning principle, instead of becoming clearer, acquires a deeper colour, and loses all its peculiar characters in its transition to a new state, which Mr. Proust has not yet examined†.

The gallic acid undergoes the same alterations by the oxygenated muriatic acid, and, like the solution of the tanning matter, becomes incapable of precipitating the red sulphate. This is the effect which age produces upon ink. The gallic acid is destroyed, the red oxide remains, and may easily be rendered blue by the method of Blagden.

As the astringent juices cannot afford indestructible inks, we must therefore return to the method of the ancients, who used coal for this purpose. Perhaps, says Mr. Proust, there is no substance so proper as Spanish chalk. This stone, which is neither bituminous nor amelite, is found with the amianthus, and is composed of alumine with six or seven per cent. of pure charcoal.

The same phenomenon of the oxydation of the blackening principle at the expence of the red sulphate, is also observed in black dyes made with fumac and this metallic salt. Mr. Proust had occasion to observe it in a bath of this kind, which no longer added to the black colour of the piece goods. The liquor was greenish; the red sulphate, or the oxygenated muriatic

\* On this subject, see the interesting paper of this author, in our Journal, I. 453. N.

† See the observations of Seguin, *Philos. Journal*, I. 275.

acid, immediately formed black. Hence he concluded that the gallic acid, or the astringent principle and the green sulphate, existed together in the bath. The red sulphate which was poured in, combined with the vegetable principle; the oxygenated muriatic acid, by converting the green sulphate into red, gave it the facility of combining and affording black with the same principle.

From these facts it follows, that, in the process of dyeing black, a portion of the blackening principle, whether it be the tanning matter or the acid of galls, is destroyed by oxydation; that a certain period arrives at which the bath can no longer afford the black dye, unless, by exposure to the air, the iron can resume the quantity of oxygen necessary to bring it to the state of red oxyde; and lastly, that dyers would considerably accelerate their work by using the red sulphate. It would only be necessary in that case to provide against the excess of sulphuric acid, which always is found in the sulphate of iron when it passes from green to red. With regard to the use of logwood, verdigris, &c. Mr. Proust is convinced that these substances are not necessary, and that the most beautiful blacks may be obtained by astringents alone with iron.

### III.

*Notice of a Memoir of Citizen GURTON, upon the Tables of the Composition of Salts, and the Means of verifying the Proportions indicated by those Tables\*.*

THE little agreement between the tables of Bergman, Wenzel, and Kirwan shews all the difficulty of determining with exactness the proportions of the component parts of salts. A method of verifying such tables would therefore be useful to the progress of science. The following appears so simple and appropriate, that it is astonishing it has not yet been attempted. It consists in a comparison of the results of experiment and computation, with regard to the agreement of the very perceptible effect arising from the excess or deficiency of one of the substances after mutual decomposition.

For example, according to Bergman :

Sulphate of Barytes contains	Acid	13.
	Barytes	84.
Sulphate of Soda contains	Acid	28.
	Soda	16.
Muriate of Barytes contains	Acid	23,57
	Barytes	76,43
Muriate of Soda contains	Acid	52.
	Soda	42.

Neglecting the water, which, though of some consequence in the effect, is of none in the result; we see that, in the case of an exchange of bases between two salts, the result of the mixture must either be neutral, or possess an excess of acid, or an excess of the base; and that, by rendering the decomposition of one of the salts complete, we ought to obtain the same result from calculation as that which shall be afforded by experiment.

\* Read to the National Institute of France, the 16th Prairial, in the year V: This notice is translated from the *Annales de Chimie*, xxv. 292.

Now,

Now, if by the sulphate of soda we decompose a quantity of muriate of barytes containing, according to the above table, 25,906 of muriatic acid, there ought to remain disengaged, or in excess, 16,710;—nevertheless the mixture remains neuter.

The fluid remains also neuter, if we cease to add the solution of sulphate of soda when the precipitate no longer falls down. The computation shews, that in a hundred parts of the mixture of these two salts, in which the muriatic acid would be represented by 20, there ought to remain 12 unsaturated, or in excess.

Let us apply the same method of verification to the proportions determined by Kirwan in the new tables in the second edition of his Dissertation.

It is known, that an exchange of bases takes place between sulphate of soda and muriate of magnesia. The computation established on the proportions indicated by the tables so rigorously assumed from the real acids, always shews a notable excess of acid, either muriatic or sulphuric, accordingly as the quantity of one or the other of these salts is increased in the mixture.

The doubts entertained by certain chemists on the respective decomposition of these two salts, have induced me to apply the same calculation to the inverse operation, that is to say, by mixing the solutions of sulphate of magnesia and muriate of soda; and in this case also the results do not agree.

Lastly, the method has been tried on a case of affinity, in which the decomposition immediately shews itself by unequivocal signs; that is to say, in the mixture of a solution of sulphate of potash and nitrate of lime.

According to the last data of Kirwan, the proportions of the component parts of these two salts, and the two others which ought to be formed, are as follows :

Sulphate of Potash	-	{	Acid 100
		{	Potash 108,7
Sulphate of Lime	-	{	Acid 100
		{	Lime 80,6
Nitrate of Potash	-	{	Acid 100
		{	Potash 83,33
Nitrate of Lime	-	{	Acid 100
		{	Lime 34,4

It is easy to form the mixture of a quantity of sulphate of potash, of which the acid shall be represented by the number 100, together with so much of the nitrate of lime that there shall be more than sufficient to cause the whole of the sulphuric acid to pass into a new combination. It is evident that, to effect this, 80,6 of lime will be required, which will therefore be disengaged from 234,4 of nitric acid; a quantity which would require for its saturation 195,32 of potash. But it finds only 108,7. The quantity of nitric acid remaining without any base, or in excess, must consequently be 64,87.

Such is the result of calculation; but experiment pronounces otherwise. The fluid diluted or concentrated, and even carried to crystallization, did not at any period present the least trace of disengaged acid.

The author declares in conclusion, that his intention is not to criticize experiments made with no less care than sagacity; but to offer to chemists a problem which is interesting in many respects, theoretical, practical, and even pharmaceutical; a problem of which the solution is no longer to be sought in the amendment of such errors as are unavoidable in

manipulations of great delicacy ; but which also leads us to consider the consequences of the affinities in saline mixtures under a new point of view, in order to discover the cause of a result so little agreeing with all the observations which have yet been made upon their composition\*.

## IV.

*On Pasigraphy; or, the Art of Writing which shall be intelligible to all Nations†.*

**C**ITIZEN Memieux has promised to assist us with a pasigraphy, or universal writing, by means of twelve characters which may be learned in twelve hours. This extreme simplicity is entitled to the utmost attention of philosophers, and seems to indicate a very superior mind in the author. We must nevertheless enquire, what may be the nature of these characters ; for their form is of no great consequence. Are they alphabetical ? Or, are they a kind of hieroglyphic, each expressing an idea, like the characters of the Chinese writing ?

If the question relate to an alphabet, the secret is certainly not uncommon ; but, if the characters be hieroglyphical, it will be difficult to comprehend how twelve primitive ideas and their representative signs can be sufficient for every combination. In either case, there is a new language to be learned, which, it is expected, the various nations will adopt. A Portuguese receives the information that a certain character, a cross for example, is to be made use of to denote a ship ; the same information is communicated to an Indian ; and, when these two men meet, if they trace a cross upon their tablets, they will both comprehend that a ship is meant,—and so of the rest. Without proceeding so far from the ordinary practice, I might say both to the Portuguese and the Indian, Write the word *navis*, and you will equally understand each other ; or, in short, I might command all the people on the face of the earth to learn Latin, or any other common language, and then acquaint them, that this qualification would enable them to understand each other.

In this manner it is, that all the nations who have adopted the Arabian cyphers can readily communicate all their ideas of numbers and their combinations, notwithstanding the difference of their languages. When a native of France sees a German write 234, both have a perception of the same object, and understand each other perfectly, though the one in oral speech would say *deux cents trente quatre*, and the other *zwey hundert vier und dreissig*. Let the German continue to write numbers, and to perform the most complicated operations, the Frenchman will follow and comprehend them without difficulty. The algebraists of all nations understand at first sight the processes written by nations possessing other idioms. The same thing happens with regard to the signs of pharmacy, the ancient chemistry, astronomy, and music. Let a number of Italians, English, French, and Germans, be collected

\* At the end of this paper, the author does justice, in a note, to M. Richter, who, in his *Neuern Gegenstande der Chymie*, &c. makes the same observation, to which he was led by the great difference between the proportions indicated in the tables already known, and those he had determined by a new method, which he calls *stachiometrique*. I hope to procure this work soon. N.

† From the *Spéctateur du Nord*, Mai 1798. The signature V\*\*\* is annexed to this paper. I have omitted the two first paragraphs, which form no essential part of the subject, and are written in a style of levity which can scarcely accord with the rest of this excellent paper.—Reference to two Memoirs on the same subject, with a few remarks, is made in our Journal, II. 189—191. N.

together in an orchestra, perfectly unacquainted with each other, let the parts of the same symphony be distributed among them, and they will, by performing the musical composition, afford a conviction, that, in spite of the difference of language, the musical pafigraphy represents the same ideas to them all.—But in these several cases of arithmetic, algebra, music, &c. it is evident, that nothing more has happened, than that all the professors have been previously induced to learn one common language. This confideration causes the miracle to vanish.

As an univerfal language, the pafigraphy is therefore a mere pretension : but there is another point of view under which it may be confidered ; as a philosophical language, better constructed than all our ancient languages, which are the products of chance, caprice, and frequently of ignorance. There is no doubt but a profound metaphysician, meditating at leisure in his closet, and modelling all his expressions from the type of the original ideas, might succeed in forming a language more simple, and in every respect superior to that which we speak. This would be the language which we might then advise all nations to adopt, if they would consent to abandon their mother-tongue and acquire a language common to all men : but this language, perfect as it might be, would not, in its own nature, be an univerfal language. We should always speak erroneoufly, if we were to assert, that the Portuguese and the Indian, each writing his own language but with other characters, might understand each other. On the contrary, we ought to say, that if both acquire the philosophical language, or pafigraphy, they will understand each other.

The whole edifice of this pretended discovery is therefore reduced to the propofal of a new language more simple, more accurately established upon the generation of our ideas, and which, on that account, is better adapted than any other to become the univerfal language, admitting it to be possible to introduce fuch a general medium.

But this propofition is not at all new, and cannot therefore be called a discovery. It has already been made by several men of the first rank in point of ability, who have acquired nothing more than the name of ingenious inventors, without prevailing on other persons to fecond their efforts.

A good history of pafigraphy would not be an ufelefs work ; neither would it be very eafy to write fuch a treatife. The materials are fcattered through a multitude of fcarce books not eafy to be collected.

In this history we should fee what efforts have formerly been employed to confine human language within general and determinate rules ; what principles have already been laid down, and the progress which this art has made in a few philosophical minds. We should judge to what extent the modern pafigraph has availed himself of, or departed from, the ideas of his predecessors. We can only present a few unconnected portions of this history\*.

It is known, that lord Bacon of Verulam comprehended nearly the whole of human knowledge ; that he difcerned almost every thing which was wanting in the entire system, and forefaw most of the discoveries which have fince been made. He laid the foundations of an Encyclopædia, and was very near discovering various important philosophical results

\* In the *Genius der Zeit*, i. 108, for the present year, an article is found, to which recourfe has been had in drawing up the present memoir. Note of the Author.

of experiments, such as the weight of the air, &c. If we open his book on the progress of the sciences, we shall find the notion of a pafigraphy in the chapter entitled The Instrument of Difcourse. "It is possible to invent fuch figns," fays he, "for the communication of our thoughts, that people of different languages may, by this means, understand each other; and that each may read immediately, in his own language, a book which fhall be written in another." But Bacon did not think of confining this to twelve characters: on the contrary, he requires a great number, at leaft as many as the number of radical words; on which head he quotes the example of the Chinefe; "and although," adds he, "our alphabet may appear more commodious than this method of writing, the thing itfelf is nevertheless well deferving of attention. The problem relates to the figns by which thoughts may be rendered current; and as money may be ftruck of other materials as well as gold and filver, it is poffible likewife to difcover other figns of things as well as letters and words\*."

Des Cartes, in his third letter to father Merfennus, difcuffes the invention of a Frenchman, whom he does not name, but who, by means of a certain language and an artificial writing, pretended to underftand all the different idioms. He remarks on this fubject, that it would be very poffible to compofe a fhort and convenient grammar, with general figns, which fhould render all foreign languages intelligible. Here are already two works on the pafigraphic art; but we fhall proceed to exhibit circumftances of a more pofitive nature.

In the year 1661, John Joachim Becher published a Latin folio, the title of which was "Characters for the Universal Knowledge of Languages: a Steganographic Invention hitherto unheard of"†. This unheard of invention was no other thing than what is now announced to us under the name of Pafigraphy; it is a method of making one's felf underftood by all foreigners by writing in one's own language, and alfo of comprehending what they write in theirs. It was truly at that time a thing unheard of; for Becher, being the firft who had given a complete treatife on this art, may be confidered as the inventor. He begins his work by a fet of very delicate and highly interefting obfervations upon general grammar, and the fundamental relations of all languages with regard to each other. He gives a learned comparative table of the relations and harmony of the Latin, the Greek, the Hebrew, the Arabian, the Selavonian, the French, and the German. This work cannot be too highly efteemed, and affuredly was not unknown to the author of the work *Du monde primitif*. A Latin dictionary then follows, in which every word correponds with one or more Arabic numeral figures arbitrarily taken. Every number is affumed as diftinctive, or denoting the fame word in all languages; and confequently nothing more is required than to compofe a dictionary for each, fimilar to that which he has given for the Latin. There is likewife a table of declenfions and conjugations, which prefents certain determinate numbers for all the cafes, moods, tenfes, or perfons. By means of this general difpofition, when a Frenchman is defirous of writing to a German the following phrafe, *La guerre eft un grand mal* (war is a great evil), he feeks in his index, *guerre, être, grand, mal*; and he writes the correpondent numbers,

13, 33, 67, 68.

\* I have tranfated from my original, inftead of recurring to the book of chancellor Bacon, which I do not poffefs. The paffages marked as quotation, are not therefore the words of Bacon, but his fenfe. N.

† The author has not given the Latin title of this work. I have therefore tranfated his French title word for word. N.

The sentence might be understood by these four characteristic numbers ; but to leave no room for ambiguity, he says, *Guerre* is the nominative case, and finds, as the characteristic of the nominative, the Arabic figure 1. *Est* is the third person singular of the indicative mood, present tense, of which the characteristic is 15. *To grand*, and to *mal*, belong likewise the figure 1, for the nominative case ; he will therefore write

13.1 | 33.15 | 67.1 | 68.1 |

where the numbers are separated by small vertical bars to prevent confusion. It may easily be conceived how, by the inverse method, the German will find in his tables the words denoted by the cyphers, which will form *Der krieg ist ein grosses uebel*.

This invention of Becher, which is the same thing nearly with regard to language, as algebra is to arithmetic, could not have cost him any great effort of the imagination ; and it evidently reduces itself, as I have observed with regard to all paligraphy, to the learning a new language, or having a dictionary at hand for use. It is besides possessed of considerable simplicity, and even a few hours practice will render it easy. If any reader should be curious to see more applications of this kind, he may have recourse to the Latin work of Sturmii, *Essais d'Expériences curieuses* \*.

In the same year George Dalgarno, an Englishman, published at London, a work of which the prolix title is sufficient to shew its object. It is, "The Art of Signs, or an Universal Character and Philosophical Language, by Means of which, Men of the most different Idioms may, in the Space of two Weeks, learn to communicate, whether by Word of Mouth or by Writing, all their Thoughts, as clearly as in their Mother-tongue. Besides which, young Persons may therein learn the Principles of Philosophy, and the Practice of true Logic, more speedily and more readily than in the ordinary philosophic Writings." The book of Dalgarno is written in Latin †. Becman accuses him of extreme pedantry. I do not know whether this book is common in England, but it is very scarce on the Continent. His characters likewise were cyphers.

Joachim Frischius, professor at the Gymnasium at Riga, was employed on a similar attempt, namely, to introduce a natural, rational, and universal language, of which some sheets printed at Thorn in 1681 may give an idea. The death of the author interrupted his labours. He purposed to call his new language Ludovicean, in honour of Louis XIV. under whose patronage he pursued his labours ; a prince who extended his generosity to the learned of all countries.

The curious in researches of this nature may also find a project of this kind in a folio volume published at Rome in 1665, by the celebrated mathematician Athanasius Kircher, the title of which is, "A New and Universal Polygraphia, deduced from the Art of Combination ‡ ;" and by means of which, says Morhoff (Polyhistor. l. ii. c. 5.), he who understands one single language only may correspond in writing with all the nations of the earth.

\* I suppose this work to be the *Collegium experimentale, sive curiosum, &c. Joannis Christophori Sturmii*, 4to. Norimberg, 1701. N.

† I know nothing of this author, nor his work. The title in the text is translated verbatim from the French. N.

‡ Neither Moreri nor any other biographer at present within my reach has given the title of this work of the voluminous Kircher. N.

It would be perhaps unjust to pass in silence the little-known work of father Bessier, Jesuit, who in a book entitled *La Réunion des Langues, ou l'Art de les apprendre toutes par une seule*, that is, The Union of Languages, or Art of learning all Languages by one alone, printed at Paris in 1674, has given several intimations which lead directly to pasigraphy.

The most remarkable work of all which has been written on this subject is, perhaps, that for which we are indebted to bishop Wilkins, the brother-in-law of Cromwell. It is entitled, *An Essay towards a Real Character and a Philosophical Language*, London 1668. It is divided into four parts. I. Considerations on the various languages, their defects and imperfections, from which a philosophical language ought to be exempt. II. Philosophical enquiries respecting all the things and notions to which proper names ought to be assigned. III. The organic science of native grammar considered as the necessary means of representing simple ideas in discourse. IV. The application of the general rules to every character and language. Examples, &c. This concise outline sufficiently shews the importance of the work \*.

In his appendix, the author explains the utility of a method of writing without alphabetic characters, by means of signs, which are to be used to denote all the principal ideas, the relative attributes being designated by small strokes added at right, acute, or obtuse angles, to the right or left, &c. Of principal or chief ideas he admits but forty, under which he ranges all the others, by that means forming a kind of categorics. His new language is calculated to afford great facility of comprehension, and new openings to the reasoning processes of science. A learner might make more progress in it in a month, than in the Latin in several years †.

After so many attempts more or less philosophical, and of different degrees of perfection, with others probably of which we know nothing, we must not overlook the efforts of the celebrated Leibnitz for the introduction of a pasigraphy. His *History and Developement of a Characteristic Universal* ‡ Language is in every library. Leibnitz exhibited, and with reason, his universal characteristic as the art of inventing and judging. He was convinced that an alphabet might be formed, and of this alphabet such words as would afford a language capable of giving mathematical precision to all the sciences. "Men may thus acquire," says he, "as it were a new organ, which would add energy to their moral faculties, as the microscopic lens increases the power of the eye. The compass is not more highly

\* Wilkins's *Essay towards a Real Character and a Philosophical Language* is a thin folio of 454 pages. A dictionary of English words, referred to their places in the forty tables, was printed at the same time (1668), and of the same size. These books are not very scarce. They contain a treasure of information with regard to the objects comprehended in the scheme for a philosophical language; but neither this, nor the character itself, as the bishop has left them, appears to be enough completed to attract the attention of the world by their facility. Dr. Robert Hooke, whose prodigious abilities give a sanction to whatever he approved, did actually learn it, and published some valuable philosophical information on an engraved plate in this universal character, with a view to excite others to acquire the means of perusal. N.

† The Chinese writing, which is a very complicated pasigraphy, has given two learned men the idea of forming one upon a more simple plan. The first is Caramuel, in his *Apparat Philosophique*, page 128; and the other Andrew Muller Griessenhag, in his *Clé Chinoise*. The latter promised to teach to women and children, in the course of a few days, a kind of writing by which all the several languages should be rendered intelligible to them. Note of the Author.

‡ *Historia et commentatio linguæ charactericæ universalis, quæ simul sit ars inveniendi et judicandi.* Oeuvres Philos. Lat. et Françaises de Leibnitz, données par M. Raspe.

valuable



valuable to the navigator, than this philosophical language would be to him who embarks on the sea of reason and experiment, which is now so full of danger."

We must not forget the ingenious method of the abbé de l'Épée, who, by means of the same identical gestures, dictated to his deaf and dumb pupils certain discourses, which they wrote with equal readiness in four languages. Nothing can more assuredly resemble a true pasingraphy than these gestures.

The abbé de Condillac, a subtle metaphysician, particularly with regard to human language, has not neglected signs and characters. In his *Art of Thinking* (*Art de penser*), in his *Art of Writing* (*Art d'écrire*), and more particularly in his *Logic*, he has shewn the advantages of a philosophical language, which should proceed perfectly in the order of the ideas, and of which the signs should be the most simple and analytical. He expressly reduces the study of all the sciences to that of a well constructed language, and quotes, as an example, algebra in the mathematical sciences.

Strengthened by such assistance, supported by the researches of so many predecessors, it must no doubt have been easy for Citizen Memieux to have conceived and executed the idea of a pasingraphy. To judge how far he is an inventor, to distinguish what exclusively belongs to him, it is necessary to examine and perpetually compare his work with all those we have pointed out, and no doubt with many others which have escaped our notice.

Will this new author be more fortunate than Becher, Kircher, Wilkins, and Leibnitz? Will he easily persuade the nations to learn the new language he proposes? Of this we may still reasonably entertain some doubt. However this may be, it is to be hoped that a pasingraphy will hereafter be established. In those ages of peace, leisure, and union, of which the prospect is held out to us by M. Kant, men will have no object of more importance than to give perfection to their language, and to listen to the voice of philosophers, who shall propose to them a better system of speech. If it should not be admitted in the lower classes of society, it will at least become the learned language of all the earth. On this subject I will venture to ground a comfortable hope, which tends to encourage the mind under its astonishment at the dreadful deluge of books which at present inundates the world.—Those books only will be translated into the learned language which deserve the trouble: the most salutary scrutiny will take place; and the rest will go to heat the baths. Philosophers, literary men, authors of every description, be assiduous, compose works of value, and be assured that your writings and your names will escape oblivion by means of the pasingraphic invention. In the mean time, till the arrival of this great epocha, one of the languages at present in use, one of the most imperfect in many respects, becomes every day more extended in its use \* \* \* \* \*

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## V.

*Observations on the Natural History of Guiana. In a Letter from WILLIAM LOCHEAD, Esq. F.R.S. Edin. to the Rev. Dr. WALKER, F.R.S. Edin. Regius Professor of Natural History in the University of Edinburgh.*

[Concluded from Page 304, Vol. II.]

**B**ESIDES these two kinds, there are also what we may denominate half savannahs, formed upon the tops of sand-hills, higher and more irregular than in the case of those just described.

described. Some of these are also very extensive. Few herbaceous vegetables are to be met with upon them. Broad spaces of arid sand are intersected by clumps of shrubbery. Nothing grows to the height of a tree; but a particular set of plants, different from those in other parts of the country, find subsistence enough to rise to fifteen or thirty feet. How Nature, after all her efforts, should have failed to induce a soil upon these is surprising. It appears chiefly owing to the great porosity of the sand, which every where admits the decayed vegetable matter destined for that purpose to be carried down through it, and filtered off by the rain. Even those sand-hills which are covered by tall trees, still shew proofs of this. The trifling layer of mould formed upon them is exceedingly thin. When cleared, they are very barren; and when you dig in them to a great depth, you still find small portions of black vegetable earth dispersed among the sand. What corroborates the above supposition is the appearance of the springs. Abundance of these are found gushing out copiously round the verges of the hills; and notwithstanding the extreme whiteness and purity of the sand from whence they flow, there is not one in an hundred whose waters are limpid. They come out not muddy, but of a brownish colour, very much like the water which runs from peat-mosses, and they are certainly tinged by the same cause. The rotten leaves of trees, and other decayed parts of vegetables on the hills, instead of being collected on the surface to form soil, are washed down into the sand strata by every rain; so that the reservoirs of the springs, and the water which proceeds from them, are always coloured with these substances. There follows a corollary also from this general principle, and, when compared with facts, I believe it will hold good: The more the sand is concreted into stone in any of the hills, the more and better will be the soil upon them. Where clay in small beds, or in a certain proportion, is mixed with the sand, the vegetable mould will likewise be better retained.

*Rivers.*—I will next give you what general observations I have been able to make upon the rivers and creeks of this part of America. The course of nearly all those of Guiana is from south to north. They originate in a chain of hills running east and west, which separates Guiana from the country of the Amazons, and likewise gives rise, on its south side, to the numerous branches which fall into that river. The Demerary is a considerable stream, equal if not superior to the Thames; yet it is by no means among the largest of them. The Essequibo is five times larger at its mouth, forming a whole Archipelago of islands; but its stream soon divides, and, on account of rocks, shallows, and rapids, none of its branches are navigable so high up as the former. Most of the particulars I am now to give you, must be understood as applying to the Demerary. The bar, if it may be so called, is common to this with many other rivers which discharge themselves into a shallow sea; but still with circumstances in the present case which distinguish it from others where the bottom is not mud but sand. It does not run like a single narrow ridge, across or nearly across the mouth of the river; but it is of great extent, and is properly a continuation of the mud-bank which runs all along the coast. To the east and west, and for two miles or more in the offing, you have ten or twelve feet water, with the utmost uniformity; and standing in with the mouth of the river open, you neither deepen nor shallow till you enter it, when you find two, three, four, and five fathom; and it continues to average that depth for a long way, so that any vessel which can enter, may, for draught of water, proceed up the river for 100 miles or more.

The

The mouth of the Essequibo, from the sand-hills and rocks being very near it, is exceedingly different. Three large islands present themselves in a breast, and divide its entrance into four channels. The length of these islands is, with the current, south and north; and from the tail or north end of each of them, as also from the banks of the main, or either side, run out sand-banks to a good distance. They are perfectly firm, quick in very few spots, and the body of them is above the level of low water. On the outside of them, you have the continuation of the mud-banks and shallow water, as above, only that the entrance of these channels is still shallower than that of the Demerary. The stream of this river runs very brown and muddy, and the sea is stained with it for some leagues off. A stranger naturally imputes this to the washings of a large flat country, or the stirring up of the muddy bottom by the tides. The latter may in part be a cause, though I believe it contributes to it but very little, and the former in a state of uncultivation, none at all. On ascending forty miles or so, you find the water clear again, or rather of a darkish hue; and so it continues above that. I was at first at a loss how to account for this; but, from a number of circumstances, was soon led to conclude that the thickness and light brown colour of the water near the mouth of the river, and on the coast, were almost entirely the effect of cultivation. Numberless ditches and canals have been opened by the inhabitants, which are receiving or discharging water every tide; and each particular piece on a plantation is every way intersected with open little drains, which communicate with these ditches. In digging and hoeing this clayey soil, much of it is suspended in the water, and carried off by the current of the tides. Nothing can be more certain, than that all up the river, and in all the creeks which discharge themselves into it, the colour of the water is constantly clear or blackish, even in the rainy seasons when it is swollen. On considering these circumstances, I have been led to this general conclusion, which is submitted to the proof of observation in different parts of the world. The reddish brown colour so common in freshes of rivers in Europe, and we may add every where, is almost entirely the effect of cultivation; and the natural colour of rivers, even in the highest and longest continued floods, where all the country is still in woods or pastures, is ever that of a dark brown, or blackish, pretty much like that of the streams which rise among peat-mosses, but rather more diluted. It is comparatively very clear, and deposits but a trifling sediment. The other is thick and opaque, and its sediment copious. Thus is man, in his little workings, made in a small degree one of the engineers of Nature. We cannot doubt that entire strata will owe to him their existence, accumulated in a series of ages at the bottom of the sea, and destined in future revolutions to act a more distinguished part. It may be curious, too, to consider the differences that may be expected betwixt the strata formed by these different depositions, which may be supposed between them to have been the origin of most of the clays upon our globe. Clay, earth, or loam, stirred up by the labourer, gives rise to the one: minutely decayed parts of vegetables form the body of the other.

It must also be observed, that clearing the ground along the coast, by cutting down trees, and opening ditches for the discharge of water, has exposed the land very much to the washing of the sea. The roots of the mangroves formed a plexus able to resist its force, and the former equal and very slow deepening of the water prevented its making a strong impression on any place. The discharge from the ditches at low water cut out channels in the mud, and left the sides of these channels more exposed to the returning waves, which

here beat continually upon a lee shore. We find therefore on the coast, that the sea has made here and there considerable encroachments, which generally begin on the west side of the canals or ditches, as being the most acted upon by the waves. The mouth of the Demerary itself furnishes us with a strong instance. That river is now nearly twice as wide as it was when the country first began to be cleared, the sea and the stream together having since that swept away a large portion of land from the western shore.

*Creeks.*—A number of creeks fall into the Demerary on both sides, but so small that they bear no proportion to the size of the river. You can hardly distinguish their mouths in the woods which overhang the banks. They are so narrow, that it is difficult to run a small boat in them; yet you will find in them throughout from two and a half to four fathom water, and they run winding so far back that it will take five, six, eight hours, or more, to carry you up to their heads, where they terminate in small streams from among the sand-hills. The banks of the creeks, at their mouths, are of the same height as those of the river close, from five perhaps to twelve feet above the water in the dry season. As you ascend the creek, you might naturally expect to find them rise. It is, however, the very reverse; they become gradually lower and lower, till at last all round them is a swamp: and the trees on each side, in like manner, become smaller and smaller, and of different species from what they were. It is now, in short, exactly a mangrove swamp, with this difference, that the water is quite fresh, the vegetables are not the same, and there are abundance of arunis and other low herbaceous plants. A little higher up, you lose the wood altogether, and find yourself in a beautiful deep canal, winding through a spacious wet savannah, which is sometimes many leagues in circumference. The first time we went up one of these creeks (called Camouni), I was surprised at this appearance, and thought it must be a mere local circumstance peculiar to it. We found afterwards the same in one or two more instances, and were satisfied upon enquiry that it is common to them all. It was natural to look for an explanation of this phenomenon, and I soon found it in one of those laws which probably extend to all rivers subject to frequent inundations. It has been observed in particular of the Ganges\*, that the banks of that river are higher than the adjacent lands at a distance from the stream, owing no doubt to the annual depositions of mud, &c. during the swell of the river. Apply the same rule to the Demerary, and the difficulty will be solved. The wet savannahs behind, and the swampy woods around them, are the body of the low country, at its natural level scarcely a foot or two above the sea. Whatever additional height the land has in the vicinity of the river, from the time you have ascended about twenty miles or so, is all acquired. It has risen from the sediment of the river during the rainy season, when the country is overflowed, so as that all the lower part of it is under water. This deposition must be always more copious in proportion as it is nearer the stream, where additional quantities are always brought, and where it is kept in motion both by the current and the tide.

Every thing we afterwards saw confirmed this theory, and nothing more directly than the canals which run out at right angles from the river. Some of these extend four miles inward, and they prove to a demonstration that the land becomes lower and lower the farther you recede from the river. The maps of the colonies confirm it; for in all of them

\* Account of the Ganges, &c. Phil. Transf. 1781, by Major Rennell.

the main body of the low land of Guiana is laid down as savannah; and the woody country, which a stranger or superficial observer would suppose to be the whole, or much the greater part of it, is in fact only a border on the sides of the rivers and of the sea, but of considerable breadth, more or less in proportion to the size of the adjoining river, or, which is generally the same thing, to the acquired height and extent of the soil on either bank. It followed as a consequence, and as far as we had opportunities of observing found it to be the case, that the low land was somewhat higher, and continued so farther down about the Essequibo, than the Demerary; the woods, consequently, were of greater extent. We found, besides, in the soil adjoining the Essequibo, at least upon the east side, a mixture of sand. The river is full of sand-banks; and it appears, that the finer parts of even this less suspensible substance are raised by the floods, and carried among the adjacent woods, to be deposited with the mud. The Mahayka, a small river or creek, which falls into the sea about twenty or thirty miles to the eastward of the Demerary, though it runs a long way up the country, and spreads into many branches, has but a very narrow and often interrupted border of wood upon its banks; it runs through an immense savannah, and so do its branches, with little or no wood till they approach the sand-hills. The Deltas of the river of Orinoco, and its numerous mouths, make a figure even in the map of the world. It is to be regretted that its noble stream has been so long hid from science. What I learned in Trinidad, from a gentleman who had sailed from its mouth to the Angaitaras, about 300 miles up, confirms and illustrates, in the fullest manner, the above general rule. The western mouths of it, opposite Trinidad, are navigable only for launches drawing six or seven feet water. At and opposite them the bottom is shallow and muddy, and the coast a low mangrove swamp, resembling in all respects that of Guiana. You must ascend those branches several days before you reach the main stream; and in doing so you find the same phenomena as in ascending the Demerary, but in a still greater degree. At first you have the mangrove, or some similar swamp, and behind it, on both sides, for about twenty leagues, the land, if you can call it so, hardly emerging from the water. Afterwards the ground appears; and, as you go up, rises still higher and higher on the banks above the common level of the stream. The trees become in the same manner of different species, and much taller than they were below. The channel in which you are, from being wide grows narrower by degrees. It is from about one and a half to three-fourths of a mile broad near the entrance, and when it joins the main stream is not more than about 200 yards. It has then acquired a considerable depth, and the banks may be about 20 feet high. Along the main stream of the river, or Boca de Nafios, the gradual rise, and other circumstances attending it, are quite similar. All this height of the bank, I can make no doubt, is entirely acquired ground, formed by the sediment of the floods, greater near the streams than at a distance from them; and though I have no knowledge of the nature of the land in the Deltas and their vicinity, I would not hesitate to say, that great part of the interior body of each island, and most probably of the main, on either side where it is low country, consists of nothing else than wet savannahs.

*Floods.*—Before we leave the rivers, it may be proper to take notice of their floods. In no instance of a large river does the universal law within the tropics fail, that they annually overflow their banks for a certain season. What was a prodigy in the Nile during the

the infancy of science, is now a well-known phenomenon to every inhabitant of a continent in the torrid zone. From the situation of the river Amazons, it amounts to a certainty, that the Demerary, Essequibo, and other rivers of Guiana, cannot originate very far up in the continent of South America. This is confirmed by what I could learn of the rise and duration of the floods of these two rivers. Enquiring about them at the plantations below is to little purpose, for there the floods are hardly discernible; but by the post-holder, and the settlers farthest up, I was informed that they are there sensible enough, and that, independent of all partial swells from accidental rains, the Demerary generally rose every year in the month of June, and continued high through July and part of August. The rise there upon the whole might be about twelve feet; it is sufficient to lay the level parts of the country under water, and to render the woods that cover them in several places passable in canoes. We could have wished for more exact information. This, however, was sufficient to prove that the rivers did not rise very far inland, else the floods would have been later in the year; but at the same time that they were of extent enough to follow the rule of all considerable intertropical rivers, so as to have a flood in the rainy season, that is, in the months when the sun is upon the same side of the line on which they have their origin and course.

The great Oronooko, I have been informed, begins to rise a little in May: it continues increasing through the summer months, and the inundation is at its height in September. At that time, as far up as the Angusturas, the rise is above forty feet perpendicular above the low water mark. It diminishes as you descend, till about the mouth, where it is only a very few feet.

*Tides* are of the utmost consequence to the inhabitants of the coast of Guiana. They enable them to drain a country which otherwise would never have been cleared, and they ascertain their journeys, which are made by water up and down the rivers, and even along the coast. At the mouth of the Demerary it is high water at about half past five, at new and full moon. The rise in spring tides, a little way up, is twelve feet or more above low water mark. The tide runs very rapidly near the mouth of the river, seldom less than four or five miles in the hour. It continues to run with force for a long way up, and was sufficient without wind to carry us up or down at 150 miles from the mouth. Above that it becomes feebler; and for a considerable distance below the Rapids, though there is a sensible rise and fall of two or three feet, yet even in the dry season the current is constantly down, only more gentle during the rise or flood; and there also the continuance of the rise is very short, not more than two or three hours.

Some observations upon the soil of the different parts of the country may be the subject of a future communication. I will only add, at present, what I think has more than conjectural foundation; viz. that this most recent of countries, together with the large additional parts still forming on its coast, appear to be the productions of two of the greatest rivers on the globe, the Amazons and the Oronooko. If you cast your eye upon the map, you will observe, from Cayenne to the bottom of the Gulph of Paria, this immense tract of swamp, formed by the sediment of these rivers, and a similar tract of shallow muddy coast, which their continued operation will one day elevate. The sediment of the Amazons is carried down thus to leeward (the westward), by the constant currents which set along from the southward and the coast of Brazil. That of the Oronooko

nooko is detained, and allowed to settle near its mouths, by the opposite islands of Trinidad, and still more by the mountains on the main, which are only separated from that island by the Bocos del Drago. The coast of Guiana has remained as it were the great eddy or resting-place for the washings of great part of South America for ages; and its own comparatively small streams have but modified here and there the grand deposit.

W. LOCHHEAD.

VI.

*On the supposed Revival of Insects after long Immersion in Wine or other intoxicating Liquor. By Mr. JOHN GOUGH.*

TO MR. NICHOLSON.

SIR,

Kendal, Oct. 7, 1798.

**T**O attack the opinions of any man is a disagreeable task, especially if such opinions have been favoured by persons of the first reputation in their respective pursuits. The force of the preceding reflection has embarrassed me not a little in my present attempt, which presumes to controvert a notion relative to the nature of insects, supported by the authority of Dr. Franklin. This acute and industrious philosopher maintains, that flies drowned in wine will revive, days, or even months, after their immersion, upon being exposed again to the air and sun. The Doctor does not profess himself to be the author of the opinion, but supposes he saw it confirmed by an incident which he witnessed in London, where discovering two flies in a vessel that was employed to decant a bottle of Madeira wine that had been brought from Maryland, and concluding, perhaps too hastily, that the flies were imported from America in the bottle, he exposed them to the sun: one of the two revived in a little time, and flew away; but the other could not be restored to life by this artless method of resuscitation. After stating the fact, the Doctor proceeds to entertain his readers, according to his custom, with some lively reflections, which would have been not less important than they are amusing, had the premises been well founded; but I am persuaded that a more careful repetition of the experiment would have determined this ingenious observer to relinquish a notion which his high name has made current with the physiologists of the present day.

In order to obtain that information and certainty respecting the subject which cannot be had from casual observations, I have repeated the experiment on a number of insects, drowned for the purpose in wine and other intoxicating liquors; an account of which trials is related in the sequel of this letter.

*Experiment 1.* Two large blue flesh flies (*musca vomitoria*), which had been immersed in a phial of red wine, with a view to this experiment, on the 12th of July 1793, were exposed again to the sun and air, on a piece of black silk, in a window, on the 20th of August the year following. In this situation they remained two days without shewing any signs of returning life; on the contrary, they were found dry and shrivelled, though their bodies appeared plump and in high preservation when taken out of the bottle.

*Experiment 2.* I repeated the preceding experiment on a number of flies, making use of Madeira and other kinds of wine, as well as brandy and beer; the time of immersion  
being

being varied from two hours to three or even four days; but the same want of success attended all these trials; for not one fly which had been rendered torpid by intoxication could be restored to life.

*Experiment 3.* Flies which were taken out of the wine two or three minutes after they ceased to shew indications of life, recovered, not only when exposed to the sun, but also when placed in a temperature kept high by means of the human breath for the space of six or eight minutes.

The two first experiments taken in conjunction with the last seem to insinuate, that the doctor was deceived in supposing his flies to have been imported from America; for the eagerness with which these insects repair to vessels containing spirituous and fermented liquors, makes it much more probable, that they were attracted by the smell of the wine into the funnel, from which they were extricated in a little time, but not before one of the two had been too long immersed to recover; and unless some circumstances of importance have been overlooked in the preceding attempts, flies made insensible by vinous spirit are subjected to the same law of suspended animation which determines the fate of animals drowned in water.

I will even venture to advance a more decisive proposition on this head, in pronouncing alcohol highly pernicious to the living principle in insects, which it destroys with certainty, but not with equal expedition in every kind; for, if the effects of this fluid on these diminutive animals be compared with the injuries they experience in the air-pump, intoxication will appear to kill them in a much shorter time than the absence of oxygen. Dr. Derham found, that several insects, which he specifies, revived on the re-admission of the air, after remaining torpid, in some instances, as long as 16 hours in an exhausted receiver. (See *Physico-Theology*, chap. 1. note F.) But life seems to be extinguished in a much shorter period by spirituous liquors.

The foregoing experiments being confined to common flies, or those with one pair of wings, I thought it not improper to extend the enquiry to a greater variety of insects, which was done accordingly in the course of the last summer. But these trials discovered nothing remarkable, except the power of the Nut Weevil (*Curculio Nucum*) to resist the destructive effects of alcohol\*. This constitutional singularity has been noticed prior to the present essay; but unless my experiments were made under some unfavourable circumstances, the property in question appears to be exaggerated.

*Experiment 4.* I immersed several maggots taken out of hazel-nuts in brandy: these were afterwards inclosed in fresh nuts opened for their reception, and placed in a temperature varying from 70° to 80°. The maggots, which had been confined in spirit for a period not exceeding 17 hours, revived; but when the time of immersion was prolonged to three, or even two days, every attempt to restore them proved fruitless.

Having now stated the most striking facts of my experiments fully, I will close the subject by forming the rest into a table, expressing the number of insects of each kind that have been killed by immersion in wine, brandy, and beer. Those marked with an asterisk, being such as delight in the shade, were shut up in perforated chip boxes after immersion, and placed in a warm temperature.

\* The maggot of the filbert will remain torpid, but not destroyed, in a phial of brandy, though bottled up for many weeks. (*Good on the Diseases of Prisons*, p. 174.)

Name.



Name.	Wine.	Brandy.	Beer.
Scarabæus fimetarius, - - -	—	1	—
Curculiones Nucum *, - - -	—	18	—
Grylli domestici *, - - -	4	2	1
Erucae Phalarum, - - -	—	20	—
Panorpæ communes, - - -	—	2	—
Vespa vulgaris, - - -	—	1	—
Apes mellificæ, - - -	2	1	—
Tipula oleracea, - - -	—	1	—
Muscæ vomitoria, - - -	2	—	—
Muscæ carnaria, - - -	4	2	1
Muscæ domestica, - - -	7	5	—
Tabani decutientes, - - -	6	4	3
Afilus crabroniformis, - - -	—	1	—
Aranæ domestica *, - - -	4	2	6
Onisci Afelli *, - - -	3	2	3
Scolopendræ forficata *, - - -	1	1	2
	33	63	16

JOHN GOUGH.

## VII.

*Various Notices respecting the Practice of the Arts in Turkey.—Jeweller's Foil.—Glue, or Mastic, for Stones and Metals.—Casting of Malleable Iron.—Filtration by Ascent.—Butter preserved without Salt.—Extemporaneous Yeast\*.*

**T**HE Armenian jewellers set precious stones, particularly diamonds, to much advantage, with a foil, which, under roses or half brilliants, is remarkably beautiful, and is not subject to tarnish. Their method is as follows: An agate is cut, and highly polished, of the shape desired; in a block of lead is formed a cavity of about its own size; over this is placed a bit of tin, of the thickness of strong brown paper, scraped bright. The agate is then placed on the tin over the cavity, and struck with a mallet. The beautiful polish the tin receives is scarcely to be imagined. This is in general kept a secret, and such foils sell for half and three quarters of a dollar each.

The jewellers, who are mostly Armenians, have a curious method of ornamenting watch-cases and similar things, with diamonds and other stones, by simply glueing them on.

The stone is set in silver or gold, and the lower part of the metal made flat, or to correspond with the part to which it is to be fixed; it is then warmed gently, and the glue applied, which is so very strong that the parts never separate.

This glue, which may be applied to many purposes, as it will strongly join bits of glass or polished steel, is thus made:

Dissolve five or six bits of mastic as large as peas, in as much spirit of wine as will suffice to render it liquid; in another vessel dissolve as much isinglass (which has been

\* From *Eaton's Survey of the Turkish Empire*, octavo. London 1798.

previously soaked in water till it is swollen and soft) in French brandy or rum, as will make two ounces by measure of strong glue, and add two small bits of gum galbanum or ammoniacum, which must be rubbed or ground till they are dissolved; then mix the whole with a sufficient heat; keep it in a phial stopp'd, and when it is to be used set it in hot water.

A remarkable instance occurred to my knowledge of an individual fact which might have been of the utmost use to society, but which, owing to the state of knowledge and government in Turkey, was wholly lost to the world. An Arabian at Constantinople had discovered the secret of casting iron, which, when it came out of the mould, was as malleable as hammered iron. Some of his fabrication was accidentally shewn to Mr. de Gaffron, the Prussian chargé d'affaires, and Mr. Franzaroli (men of mineralogical science), who were struck with the fact, and immediately instituted an enquiry for its author. This man, whose art in Christendom would have insured him a splendid fortune, had died poor and unknown, and his secret had perished with him! His utensils were found, and several pieces of his casting, all perfectly malleable. Mr. Franzaroli analysed them, and found that there was no admixture of any other metal. Mr. de Gaffron has since been made superintendant of the iron manufactory at Spandau, where he has in vain attempted to discover the process of the Arabian.

I have seen practised a method of filtering water by ascension, which is much superior to our filtering stones or other methods by descent, in which in time particles of the stone or the finer sand make a passage along with the water.

They make two wells from five to ten feet, or any depth, at a small distance, which have a communication at bottom. The separation must be of clay well beaten, or of other substances impervious to water. The two wells are then filled with sand and gravel. The opening of that into which the water to be filtered is to run, must be somewhat higher than that into which the water is to ascend; and this must not have sand quite up to its brim, that there may be room for the filtered water, or it may, by a spout, run into a vessel placed for that purpose. The greater the difference is between the height of the two wells, the faster the water will filter; but the less it is the better, provided a sufficient quantity of water be supplied by it.

This may be practised in a cask, tub, jar, or other vessel. The water may be conveyed to the bottom by a pipe, the lower end having a sponge in it, or the pipe may be filled with coarse sand.

It is evident that all such particles, which by their gravity are carried down by filtration by descent, will not rise with the water in filtration by ascension. This might be practised on board ships at little expence.

The butter which is mostly used in Constantinople comes from the Crim and the Kuban. They do not salt it, but melt it in large copper pans over a very slow fire, and scum off what rises; it will then preserve sweet a long time if the butter was fresh when it was melted. We preserve butter mostly by salting. I have had butter which, when fresh, was melted and skimmed in the Tartan manner, and then salted in our manner, which kept two years good

good and fine tasted. Washing does not so effectually free butter from the curd and butter-milk, which it is necessary to do in order to preserve it, as boiling or melting; when then salt is added to prevent the pure butyrous part from growing rancid, we certainly have the best process for preserving butter. The melting or boiling, if done with care, does not discolour or injure the taste.

The preservation of yeast having been a subject of much research in this country, the following particulars may perhaps deserve attention:—On the coast of Persia my bread was made, in the English manner, of good wheat flower, and with the yeast generally used there. It is thus prepared: Take a small tea-cup or wine-glass full of split or bruised pease, pour on it a pint of boiling water, and set the whole in a vessel all night on the hearth, or any other warm place; the water will have a froth on its top next morning, and will be good yeast. In this cold climate, especially at a cold season, it should stand longer to ferment, perhaps twenty-four or forty-eight hours. The above quantity made me as much bread as two sixpenny loaves, the quality of which was very good and very light.

## VIII.

*Observation of the Passage of a Comet over the Disk of the Sun. By Citizen DANGOS.*

ON the 18th of January 1798, Dangos observed a black, round, and well terminated body, which crossed the disk of the sun. The time of its passage lasted 20 minutes. He thinks it could be nothing but a comet. He recollects having observed a similar phenomenon in 1784.

Lalande remarks, that Mercury and Venus have been well observed crossing the solar disk in the form of black spots; but that comets had never been in that situation\*.

\* This notice, which is taken from the *Journal de Physique* for February 1798, leaves much to be desired. What might be the diameter of the spot; whether its course was direct or retrograde, with its inclination to the ecliptic, or to the solar axis; and particularly the chord it described; are objects of enquiry, concerning which we shall probably learn more in future, when a fuller account shall appear. If we admit that it was a comet, and, by way of obtaining a rough notion or guess of its distance, we suppose it to have described a whole diameter of the sun, we shall, from the time and the angular space, deduce, that it was about seventy times nearer the sun than our planet. This, on the supposition of its being near the perihelion, which however is mere supposition, would rank it among those comets which approach the nearest to that luminary. (See a Table of the elements of a considerable number of comets in Pingre's *Cometographie*, which is copied into Hutton's Dictionary, article Comet.)

If we consider how very seldom the inferior planets, Venus and Mercury, cross the sun, notwithstanding their short periods, and the little inclination of their orbits, it will be less surprising that the comets, though very numerous, should not often be found together with the earth in the line of their nodes; or that an unforeseen event, of such short duration, should scarcely ever meet the eye of the astronomer. N.

## IX.

*Analysis of the Aqua-marine or Beryl; and the Discovery of a new Earth in that Stone.*

*Read before the French National Institute 26 Pluviose, in the Year VI. (Feb. 14. 1798.)*

*By Citizen VAUQUELIN\*.*

*Section I. Introduction.*

THE analysis of minerals is one of those operations which are usually considered to be of little importance, and are submitted, by chemists of the first order, as unworthy of their care, to the manipulation of their pupils.

I am well aware that the greater number of analyses afford results of little importance, which do not repay the labour and the time bestowed in obtaining them. I am likewise aware that they do not offer so brilliant a prospect, nor promise to afford results of so general a nature, as the plan of operation which has been formed with regard to some of the most important points in chemistry. But I am not, from these reasons, of opinion, that this class of processes, which has likewise its difficulties, and requires, for its successful conduct, a certain series of reasoning, and particularly an exact knowledge of the bodies described;—I am not of opinion that it is so little entitled to engage the attention of philosophical chemists. For they must recollect, that it has afforded them the solid foundation of their theories, and new objects for the exercise of their abilities.

From a disregard of this kind it was that Bergmann, whose active mind could not submit to the details of experiment, has committed so many faults, by trusting his operations to young pupils, who had not acquired the habit of distinguishing new substances from those which were already known.

The analysis of the beryl, already made by Bindheim, will be a proof of what I here advance. It is composed, according to him, of silice 64, alumine 27, lime 8, and iron 2.

Citizen Haüy having found a perfect agreement between the structure, hardness, and weight of the beryl and the emerald, engaged me, some months ago, to compare these two stones by chemical means also, in order to know whether they were composed of the same principles in similar proportions.

The most interesting circumstance to the Institute in this result being a new earth, which I have discovered in the beryl, I shall pass slightly over the other objects, and dwell more particularly on its distinctive properties.

*Section II. The Method of Analysis.*

*Experiment 1.* One hundred parts of beryl reduced to fine powder were fused with 300 parts of caustic potash; the mass, after cooling, was diffused in water, and treated with the muriatic acid: by this means the solution was completed.

The muriatic solution was evaporated to dryness; towards the end of the evaporation the fluid assumed the form of a jelly: the dried matter was then diffused in a large quantity of water. Part of the matter was dissolved; but a white, granulated transparent powder remained. This substance, collected on a filter, washed with much water, and dried by ignition, weighed 69 parts. It had all the properties of silice.

\* Annales de Chimie, xxvi. 155.

*Experiment 2.* The fluid separated from the flex was precipitated by the carbonate of potash of commerce; the precipitate collected and drained was treated with a solution of caustic potash. The greatest part of the matter was dissolved; but there remained a certain quantity of earth which was not taken up. This being separated, washed, and dried by ignition, was of a brown greyish colour; it weighed nine parts. In these nine parts it is that our new earth is contained. We shall speak of it again in the subsequent part of this Memoir.

*Experiment 3.* The alkaline solution of the foregoing experiment was super-saturated with muriatic acid, until a perfect solution took place, and this was again precipitated by the carbonate of potash of commerce: the deposition, washed and dried by a red heat, weighed 21 parts.

This substance appeared to me at that time to be pure alumine. We shall see what conclusion ought to be made, after examining the properties of the new earth which I have announced.

*Experiment 4.* The nine parts in *Experiment 2.* remaining after the action of the potash, and in which I announced the existence of a new earth, were dissolved in the nitric acid; the solution was evaporated to dryness, and the residue again dissolved in water. The solution of this substance having assumed a reddish yellow colour, which indicated the presence of iron, a solution of the hydro-sulphuret of potash was mixed with it; a black voluminous precipitate was formed; the fluid was heated in order to favour the union of the parts, after which the fluid was decanted clear and colourless. The black precipitate by calcination became of a red brown colour, and weighed one part. When dissolved in the muriatic acid, and the solution evaporated to dryness, it afforded a beautiful blue when an atom of the matter was thrown into a solution of the Prussiate of potash: it was therefore the oxide of iron.

The fluid from which this oxide had been separated was again evaporated to dryness; and though at first it had no colour, it nevertheless became red towards the end of the process, and the residue preserved that colour. This residue having been left for 24 hours in a capsule, became reduced into a kind of gelatinous fluid, of a yellowish red colour: cold water poured thereon dissolved the whole mass; but the solution was red and turbid: by exposure on the heated sand-bath, red flocks were separated, and the fluid became as clear as water. These flocks, carefully separated, washed and dried, weighed half a part: it was also the oxide of iron; which, with the part before obtained by means of the hydro-sulphuret of potash, makes one part and a half of that substance.

*Experiment 5.* The earth being thus perfectly deprived of the oxide of iron, I separated it from the nitric acid by means of the common carbonate of potash; and I obtained 12 parts of a white earth, soft beneath the fingers, and soluble in acids with effervescence.

We see that this earth, in its separation from the nitric acid, did absorb four parts and a half of carbonic acid; since out of nine which were subjected to experiment, one and a half of the oxide of iron were obtained; which leaves 7,5 for the earth contained in the 12 parts of carbonate last precipitated.

*Section III. Exposition of the Properties of the new Earth contained in the Beryl.*

*Experiment 1.* The 12 parts of earth united with carbonic acid, as before mentioned, were put into sulphuric acid, which dissolved them completely with effervescence. The solution had a very saccharine taste at first, and astringent at last. The solution left till the following day afforded irregular crystals, very solid and saccharine like the solution which afforded them.

*Experiment 2.* These crystals were again dissolved in water; the solution mixed with sulphate of potash did not afford alum either immediately nor by evaporation, as happens in the alumine when combined with the sulphuric acid. Each of these salts crystallizes separately, without contracting any union.

I repeated this operation five times in succession, with different doses of sulphate of potash, without obtaining more success than at first. At last, to convince myself of the difference between this earth and alumine, I took equal quantities of the one and the other; and after having dissolved them in the sulphuric acid, I mixed them with like quantities of sulphate of potash. I constantly obtained octahedral alum with the alumine; but the earth of the beryl afforded only an irregular salt.

These differences first discovered between the two earths, induced me to seek for others, by comparing them in a greater number of points.

*Section IV. Comparison of the Properties of the Earth of Beryl with those of Alumine.*

*Experiment 1.* For this purpose, I separately dissolved equal quantities of alumine and of the earth of beryl in nitric acid, to perfect saturation.

The salt which arose from the combination of the earth of beryl with the nitric acid, did not appear susceptible of crystallization; it strongly retains moisture; by desiccation it becomes converted into a kind of ductile paste, which, when exposed to the air, powerfully attracts moisture. Its taste is at first very sweet, and afterwards astringent.

*Experiment 2.* The nitrate of alumine likewise crystallizes with considerable difficulty; but it does not attract moisture so strongly. Its taste is not saccharine, like that of the nitrate formed with the earth of beryl.

I made the following comparative essays of the solutions of these two salts, using equal quantities of each.

1. The nitrate of alumine, mixed with a solution of nut-galls in alcohol, afforded no precipitate. The fluid simply acquired a slight greenish colour, and lost somewhat of its transparency; however, at the expiration of some hours, the fluid, having been diluted with water, let fall a greyish precipitate.—2. The salt of the earth of beryl, mixed with the same re-agent, immediately afforded a deposition in flocks of a yellow brown colour.

3. The nitrate of alumine, mixed with the oxalate of potash, immediately afforded a precipitate in the form of very abundant white flocks, which subsided to the lower part of the vessel, and left the superior fluid perfectly clear.—4. The salt of the earth of beryl, with the same re-agent, did not afford the slightest appearance of a precipitate, even after several days.

5. The

5. The nitrate of alumine, mixed with tartrite of potash, immediately formed a deposition in flocks, and the super-natant liquor became clear and colourless.—6. The salt of the earth of beryl, with the same re-agent, did not produce any sign of precipitation after several days.

7. The nitrate of alumine, mixed with a solution of the phosphate of soda, afforded a gelatinous semi-transparent precipitate, which subsided very slowly.—8. The salt of the earth of beryl also formed a precipitate with the same re-agent; but it was less gelatinous, and less transparent, and it also fell down more speedily.

9. The nitrate of alumine, mixed with very pure Prussiate of potash, instantly afforded a very abundant whitish precipitate, which became green at the end of a few hours.—10. The salt of the earth of beryl, with the same re-agent, afforded no precipitate, even after several days.

11. The nitrate of alumine, mixed with a saturated solution of potash, afforded a gelatinous magma, which was semi-transparent, and soon became filled with numerous bubbles of gas, which raised it to the upper part of the fluid.—12. The salt of the earth of beryl, mixed with the same re-agent, afforded a precipitate in flocks, which was not filled with bubbles like the foregoing, and which fell to the bottom of the liquid.

13. The nitrate of alumine, mixed with a solution of caustic potash, at first afforded a gelatinous deposition, which was afterwards taken up by the excess of alkali.—14. The salt of the earth of beryl, treated with the same re-agent, was affected in the same manner, excepting only that a larger quantity of alkali was required for the second solution.

15. The nitrate of alumine, mixed with a solution of carbonate of ammoniac, formed a precipitate which was not re-dissolved by an excess of alkali.—16. The salt of the earth of beryl, mixed with the same re-agent, afforded a precipitate which was entirely re-dissolved by an excess of alkali.

We see by most of these experiments, that the earth of beryl essentially differs from alumine, which however it resembles much more than any other earth, and with which it may even be easily confounded in certain respects.

But the experiment which induced me to fix my opinion irrevocably with respect to this earth, was that which shewed the comparative degree of attraction of the two earths for the same acid.

To acquire this knowledge, I dissolved in the nitric acid twelve parts of very pure alumine, and evaporated to dryness, in order to expel the surplus of acid which was in the fluid. The residue being dissolved in water, I added to the solution ten parts of the earth of beryl, recently precipitated from its solvent, well washed, and still moist.

I added no more than ten parts of the earth of beryl, to precipitate the twelve parts of alumine, though I had ascertained, by other experiments, that a somewhat greater quantity of the earth of beryl than of alumine was necessary to saturate the same quantity of acid; but I chose rather that a small quantity of alumine should remain in solution, than that any portion of the earth of beryl should mix with the precipitate. When therefore the mixture had thus been made, I boiled the fluid for a quarter of an hour, then filtered it, and retained on the filter the earthy precipitate. After washing this, I combined it with the sulphuric acid, and evaporated to dryness, in order to expel the excess of acid; after which,  
re-dissolving.

re-dissolving it in water, I added a few drops of sulphate of pot-ash to the solution, and obtained octahedral crystals of alum.

Now it is evident that the earth of beryl has more affinity with the nitric acid than alumine has, and consequently that it is not the same earth. If the earth of beryl be not alumine, there is much greater reason to decide, that it is not one of the other known earths; for it differs much more from them than from alumine. I therefore consider this earth as a new substance, different from all those we are yet acquainted with. It is true, that it in some measure resembles alumine, namely, in its softness to the touch, its adhesion to the tongue, its levity, its solubility in pot-ash, and its precipitation from its solutions by ammoniac. But it differs from alumine in its other properties. Its combinations with acids have a very saccharine taste; it has a stronger affinity with those solvents; it does not afford alum with the sulphuric acid and potash; it is totally soluble in carbonate of ammoniac; and lastly, it is not, like alumine, precipitable from its solutions by the oxalate and the tartrate of pot-ash.

This earth being soluble in caustic pot-ash, like alumine, we can no longer trust to this simple character to ascertain the presence of the latter earth; for it may happen that the earth of beryl should be taken for alumine, or a mixture of both for one or the other of these pure earths. It will therefore be necessary, whenever an earth soluble in pot-ash is found, to endeavour to convert it into alum by the known methods. If it do not afford alum, it may be certainly concluded that it is not alumine. But it may possibly afford alum, and nevertheless contain the earth of beryl; a mother water will then remain, in which this last earth will be suspended.

To separate the small quantity of alum, which likewise remains in this mother water, it will be proper to decompose it by a solution of the carbonate of ammoniac added in excess; by this means alum will be entirely precipitated, and the earth of beryl will remain dissolved in the carbonate of ammoniac. This earth may afterwards be readily separated, by boiling the solution for a certain time. The heat will drive off the carbonate of ammoniac, and the earth will fall down in the form of a powder.

By comparing the results of the analysis of the beryl with those which Klaproth and myself obtained from that of the emerald, we might conclude, that these two stones are very different from each other; for I found that the emerald was composed of 64 of silex, 29 alumine, 2 lime, between 3 and 4 of the oxide of chrome\*, and 1 or 2 of water; whereas the beryl is composed of 69 silex, 21 alumine, 8 of the peculiar earth, and  $1\frac{1}{2}$  of the oxide of iron.

But since that time I have found that the emerald likewise contains this new earth; whence it follows, that the emerald and the beryl are one and the same substance, differing only in their colouring matter.

With regard to the proportion in which I have obtained this earth of the beryl, I do not give it as strictly accurate; for it is possible that part may have been dissolved at the same time as the alumine by the pot-ash †.

\* This is the metallic acid discovered in the red-lead of Siberia, of which a short account was given in our Journal, II, 145. The memoir at length, with additional information, will appear in our next. N.

† Since the above was written, I have ascertained, that there was in fact a certain quantity of the earth of beryl dissolved by the pot-ash with the alumine; and that instead of 8 per cent, the beryl contains 16.



I have not yet thought it proper to give a name to this earth. I shall wait till its properties are better known ; besides which, I should be glad to have the advice of my brother-chemists on the subject \*.

In a second memoir, I shall speedily give the most complete account in my power, of its combinations with the acids and some of the combustible bodies †.

## X.

*Description of a new-invented detached Escapement for Pocket Watches, &c.*

*By Mr. JOHN PRIOR†.*

**F** I G. 1 and 2, Plate XV, represent the principal parts of the escapement.

*Fig. 1.* A B, the pillar-plate ; and A B E F, *fig. 2*, is a section of the frame without pillars or potance. C, the cock screwed to the potance-plate at D. G, the balance, the lower part of which runs in the plate (but perhaps a potance will be more convenient for it in a pocket watch). H, the regulating spring, pinned fast in the stud at I. O, the friction wheel. N, its arbor, the higher pivot of which runs in the collet the balance is rivetted to, and the lower in a collet, screwed fast on the balance-arbor at P ; so, when the balance turns round, the friction-wheel is taken along with it.

M, the intermittent lever-wheel. L, its arbor. R, its pinion, in the rim of which wheel are put an equal number of pins on each side. The pins on one side of the wheel are put exactly at the middle distance of those on the other side.

T, the intermittent lever, screwed fast to K, its arbor. Q Q are its banking screws.

The lever is all made of one solid piece of steel. That end of it next the wheel is cut open wide enough to receive the rim, without touching either side of it, as far as the pallets or inclined planes are ; at the end of which it is cut wider, to admit the pins of the wheel. (See *fig. 2*, a section of it as under L.)

The distance from the centre of the balance to the extreme part of the lever, or end of the pallets, is divided in a right line into ten equal parts, and the pivot-hole of the friction-wheel is made at the first tenth division from the centre of the balance. There are thirteen semi-diameters of the friction wheel from its centre to the end of the pallets.

When the balance is at rest, then the centre of the balance, the centre of the friction-wheel, and the centre of the lever, will be all in one right line. (See *fig. 1*.)

The lever is divided into three equal parts. The distance from the end of the pallets to

\* The most characteristic property of this earth, confirmed by the latest experiments of our colleague, being, that it forms salts of a saccharine taste, we propose to call it Glucine, from *γλυκός* sweet, *γλυκὸν* sweet wine, *γλυκαίνω* to render sweet. This denomination will be significant enough to assist the memory ; it does not derive its etymology from a sense too strictly determined ; neither does it present ideas falsely exclusive, like these names which might be taken from the name of the stone which afforded the first specimen of the new substance, the name of the first village where it was met with, &c. &c. These, we apprehend, are the true principles for the advancement of science, and facilitating its study, by means of nomenclature. Note of the Editors of the *Annales*.

† Cit. Vauquelin has published an appendix to this paper, on the general properties of this earth, which will appear in our next. N.

‡ Transactions of the Society for the Encouragement of Arts, 1798. A premium of thirty guineas was given to the inventor. N.

the centre of motion of the lever, one part ; and from the centre of motion to the centre of the friction-wheel, when the balance is at rest, two parts : so that, whatever the velocity may be at the end of the pallets when in motion, that part of the lever, or the forked part of it, which is opposite the centre of motion of the friction-wheel, will be twice as much.

SS represents a superficial view of the pallets. The pallet below the pillar-plate is taken off from the other, otherwise the place of action could not be seen in the drawing.

In *fig. 1*, we have a view of the lever before the machine is wound up : a pin is half-way down the pallet ; but when the pin impels the pallet, the lever moves, and gives motion to the balance, and the pin is disengaged from the pallet at the angle : that instant a pin, on the opposite side of the wheel, falls upon a circular part of the end of the other pallet, or upon that part where it and the lever are entirely at rest, until the balance makes its return, and the friction-wheel comes into the forked end of the lever. Then the friction-wheel impels the lever, while the balance runs over the space of about one hundred degrees. Then the pin drops off at the end of the pallets as before, and the balance is entirely left at liberty ; at which time its velocity is so great, and its motion so easy, that it turns once round upon its pivots, and two hundred and forty degrees every vibration.

It is to be observed, that, in the ends of the fork, one part is turned up and the other down, in order they may not both go through one notch in the balance arbor ; for, by having two different parts cut away, the fork is locked fast in every vibration, and is unlocked only by the friction-wheel in its return for another vibration.

Notwithstanding the balance makes one turn and two hundred and forty degrees every vibration around its axis, yet, when the balance is at rest, and the same power applied to it (which keeps it in motion as above), the balance will only move through the space of about fifteen degrees, which is only one fortieth part it keeps it up to when in motion.

It must be allowed, the less the wheels, or the power, have to do with the balance, the more accurate the time will be shewn \*.

## XI.

*On Mr. CARTWRIGHT's Invention for rendering the Pistons of Steam Engines, Pumps, and other Hydraulic Apparatus tight by metallic Parts, without packing or leathering.* (W. N.)

TO MR. NICHOLSON.

S I R,

Richmond, Oct. 10. 1798.

I HAVE lately been informed, that a new method of packing steam engines has been invented by a Mr. Cartwright, which is said to be particularly advantageous in saving friction and resisting an heavy column of water in pumps. I shall be very glad to see a description of the same in your Journal, with your opinion of its effects, &c.

Your constant Reader,

A. R.

\* Artists will perceive that this is an improvement of the anchor escapement of Mudge, of which the inventor was not aware till he came to London. In this the pallets exactly resemble those of Graham's dead beat (*Philos. Journal*, II. 52.), and a tail acts against two planes in the axis of the balance. Pallets like those of Mr. Prior were adapted to a long pendulum by Mr. Crosthwaite of Dublin, in the year 1788, (*Memoirs of the Irish Acad.*, vol. II.) and since that time also by other persons in London. N.

THE

THE contrivance to which my correspondent refers, is part of a steam engine, for which a patent has been taken out. It is generally understood that in practice it is necessary to apply the packing close round the piston of a steam engine, in such a manner as to make it act strongly against the sides of the cylinder, which must occasion a considerable degree of friction: and when it has worked loose, it may be concluded that some loss of force must follow, from the escape of elastic fluid between the surfaces intended to be in contact. A similar observation may be applied to the usual leathering of pumps, in which the friction is very great, and the resistance to severe pressure very far from being effectual. Early in the present century, a contrivance was made by a Mr. Haskins to prevent friction, by substituting quicksilver instead of leather; which, for several statical reasons, and also, as I suspect, from some of a chemical nature, could not be brought into general use. The reader may see a very full description of this engine, in Desaguliers's Course of Experimental Philosophy, II. 491.

Mr. Cartwright's invention consists in using solid masses of metal instead of the packing or leathering; which, by means of springs, adapt themselves to the variations of diameter in the cylinder, and, by their mode of application above each other, are expected to prevent that escape of fluid which would else take place through the intervals between the several pieces. Imagine the piston to consist of a circular plate of metal, nearly equal in diameter to the cylinder in which it is to move. It will make no difference in the general consideration of our subject, whether this piston have a valve in it or not. Upon the upper surface of this piston are laid three or more pieces of metal, which all together compose a flat circular ring; the ends of the several pieces nearly touching each other. A very moderate portion of mechanical knowledge will suggest the manner in which these pieces might be made to recede outwards, by means of springs; so as to occupy the circumference of a greater circle externally, than that to which they would nearly correspond when regularly pressed inwards. If the piston in this situation be placed in its cylinder, it is evident, that the pieces composing the ring will be forced against the concavity; which, if of the proper curvature, they will fit, and prevent any fluid from passing through, except at the interstices, where the ring is rendered incomplete by being divided into parts. To remedy this, a second ring is laid upon the first, with its joints half-way between the joints of the former, in the same manner as we every day observe in courses of brick-work. This second ring, being urged outwards like the first, performs the same function; but any fluid that may pass downwards through the interstices of the upper ring, will be stopped by the contiguous parts of the lower; and, on the contrary, whatever fluid may pass upwards through the interstices of the lower ring will, for the same reason, be stopped by the upper.

From this description it is sufficiently clear, that Mr. Cartwright is entitled to much praise for the ingenuity and acuteness displayed in his contrivance. The practical value of that contrivance, respecting which I am requested to state the facts, will probably be ascertained from the following considerations:

In the usual stuffing or leathering, the elasticity of the organized matter made use of is supposed to act, in each individual part, so far independently, that, if there be an irregularity in the cylinder by variation of its curvature or magnitude, this irregularity will be followed up and fitted by the elastic material. But Mr. Cartwright's metallic packing possessing a determinate curvature, will fit only when the zone with which it is in contact possesses the

same curvature. If his piston be pressed into a smaller cylinder than corresponds with the actual circle his pieces are adapted to form, those pieces will be pressed in, and will touch only at their extremities; so that every joint will be immediately over a place where the opposite ring does not in fact touch the cylinder: it will therefore, in this case, leak principally at the joints. And, on the contrary, if his piston be pressed into a larger cylinder than corresponds with the circle of the rings, each portion of those rings will touch the cylinder in one point only. The most favourable point will be at the half-way between the extremities of each piece. In this position the joints indeed will not leak, but every other part of the circumference will; and the places of the most open passage will be at those points of the circumference which are equidistant between joint and joint. These considerations relate to perfect circles; but if we attend to smaller irregularities, whether convexities or concavities in the cylinder, it appears evident, that the segments of the rings being inflexible will still less effectually adapt themselves to such imperfections.

These objections, relating to the figure of the periphery of this metallic piston, are of very serious import: those which relate to its action are scarcely less so. The surfaces of the rings thus laid upon the piston and upon each other, together with the surface of the piece which confines them from rising, must all be very well adapted to each other; and the number of square inches of this surface must, in the nature of the contrivance, be considerable enough to afford much friction. It may reasonably be doubted whether these pieces, in the rapidity of ascent and descent, can obey the action of the springs during the very short times in which the pieces are opposed to the irregularities they are meant to remedy. But when a great pressure, such, for example, as the reaction of a column of 100 feet of water, comes to be exerted upon the face of this apparatus, the plates or pieces of these rings may be imagined to be confined in a vice. The pressure of such a column will amount to more than 40 pounds upon every square inch. Whence we may conclude, either that they would not move at all, or that the force of the springs must be such as greatly to load the work with friction, and damage the apparatus by speedy wear.

Lastly, it seems to be a question or doubt, which well deserves to be resolved by further experiment, whether, in any case of reiterated or long continued action, the softer metals can be made to work in contact with each other, in the way of close fitting, with as little resistance and wear as when an organised substance containing oil or fat is interposed.

When one individual speaks to the world concerning the works of another, the transaction is naturally accompanied with a sense of personality. As this sense ought not to lead men into unworthy actions, so, on the other hand, it ought not to prevent their fulfilling any duty which may call upon them. Convinced that no vindication or apology is necessary in defence of a scientific examination of every object which is offered to the public acceptance, I have spoken freely of a construction which, from its ingenuity, might be thought of greater value than it really is; but which, when carefully examined, appears to be inferior to the methods already in use.

## XII.

*Information respecting the Zoonic Acid, discovered by BERTHOLLET.*

THE fluid \* obtained by distillation from animal substances has been hitherto thought to contain no other principle than carbonate of ammoniac and an oil. Berthollet has ascertained that it contains an acid, to which he has given the name of zoonic acid. He has ascertained its presence in the fluid obtained from the gluten of wheat, the yeast of beer, bones, and woollen rags, distilled for the preparation of the muriate of ammoniac; and he thinks himself authorized to consider it as the product of distillation of all animal substances.

In order to separate this acid, he mixes lime with the fluid afforded by this destructive distillation, after having separated the oil. The mixture is then boiled or distilled, to separate the carbonate of ammoniac. When the odour ceases to be penetrating, he filters and adds a small quantity of lime to the liquid, which he again boils till the odour of ammoniac has entirely disappeared. What remains is the zoonate of lime, which he again filters. To this he then adds the aqueous solution of carbonic acid, or otherwise he blows through a tube into the liquor, in order to precipitate any lime which might exist in the uncombined state. The zoonate of lime may then be used to produce other compounds by double affinity; or the pure zoonic acid may be had by the following process:

The well concentrated aqueous solution of zoonate of lime is to be mixed with phosphoric acid, in a tubulated retort, and exposed to distillation. The zoonic acid is not very volatile, but requires a degree of heat nearly equal to that of boiling water to raise it. The fluid must therefore be boiled; and if two successive receivers be at the same time adapted, it will not be driven into the second. Part of the acid seems to be destroyed by the action of the heat; for the liquor becomes brown by the ebullition, and towards the end of the process black: whence it may be concluded that this acid contains carbone. Berthollet did not examine the other principles which are disengaged during the decomposition.

The zoonic acid has a smell resembling meat which has been roasted; a process in which it is indeed formed. Its taste is austere, and, from the few experiments of Berthollet, no remarkable properties were exhibited. It strongly reddens paper tinged with turnsol, and effervesces with alkaline carbonates. It did not appear to him to afford crystallizable salts with earths or alkaline bases. It afforded a white precipitate in the aqueous solution of acetite of mercury, and in that of nitrate of lead; so that it has a stronger attraction to the oxydes of mercury and lead respectively than the acetous and nitric acids. It does not act on the nitrate of silver but by double affinity. The precipitate which then falls down becomes brown in time, and therefore contains hydrogen. The zoonate of potash calcined did not afford prussiate of iron with a solution of that metal. A liquid, possessing all the characters of acidity, was separated from flesh, which Berthollet had kept a long time in a state of putrefaction; but it was an ammoniacal salt with excess of acid. This acid, combined with lime, appeared to him to resemble the zoonate of lime; but the quantity he had was too small to admit of its identity with the zoonic acid being accurately determined.

\* Nearly in the words of Berthollet, in the *Annales de Chimie*, xxvi. 86.

## XIII.

*Historical Notes concerning the Invention of the Air Pump with Metallic Valves; the Necessity of Alkali to produce the crystallized Salt called Alum; and the electrical Instrument known by the name of the Revolving Doubler. (W. N.)*

CITIZEN ADET, in vol. xxv. of the *Annales de Chimie*, p. 165, claims the invention of an air-pump for Cit. Ami Argand, at Paris, in the year 1776, of which that of Cuthbertson is said to be an imitation. Reference is made to the notes on the third volume of the *Leçons Élémentaires de Physique, de Sigaud de la Fond*, for a description.

I am happy in this opportunity of doing honour to a philosopher and mechanic, with whose ability I am well acquainted. That tenacity with regard to the credit arising from first thoughts or inventions, and the partiality which leads men to exult in the nationality of discovery, are estimable qualities on the whole, because they tend to the promotion of science; but they sometimes lead to insinuations of *mala fides* in cases where the coincidences of reasoning have alone produced similar results. On this occasion it seems proper to remark, that Mr. Cuthbertson has candidly displayed the source from which he derived his information, and that, upon the whole, it appears highly probable, as well from the respectable characters of the individuals, as from the general circumstances of the case, that this artist, as well as Paets van Troostwyk, Dr. Rutherford, and Sir George Mackenzie, would have done justice to the invention of Mr. Argand, if they had been acquainted with it\*.

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In the same *Annales*, xxiii. 222, there is a claim on the part of the celebrated Chaptal respecting the discovery of the nature and triple composition of alum, communicated to the Institute by Vauquelin†. To which this last chemist has answered, in vol. xxv. p. 107, that he was unacquainted with the labours of Chaptal in that respect, and had communicated his own memoir to the Institute a fortnight before Cit. Chaptal's memoir arrived; and, lastly, that the priority and merit of the discovery belong to Cit. Descroiffilles, whose researches concerning the nature of alum were published by Berthollet, in his *Art of Dyeing*, long before that time.

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Another instance in which I am in some degree concerned, affords a curious example of the slowness with which the improvements of philosophical apparatus are in some cases communicated. In the 22d number of the *Bibliothèque Britannique*, there is an account of the doubler of electricity of John Read. The process of accumulating electricity‡ by doubling, was invented by Lichtenberg and Klinckock, and greatly improved by Bennett, who applied it to Volta's condenser. In 1787, Dr. Darwin constructed a machine for performing the process in part mechanically; and in 1788, I made and communicated to the Royal Society the Revolving Doubler by which the whole act is reduced to the simple turning of a winch. Five years afterwards, namely in 1793, Mr. John Read published his "Summary View of Spontaneous Electricity, &c." in the 4th chapter of which he

\* On this subject, see our Journal, II. 28.

† Philof. Journal, I. 318.

‡ Philof. Journal, I. 396.

gives a description of my instrument in my own words, copied without acknowledgment from the Philosophical Transactions for 1788, but disguised in a small degree by a new denomination in the title, and an unnecessary lengthening of the insulating parts, which before were upwards of ten times the length of the interval between plate and plate. It is evident, therefore, that of all the individuals who have written on this instrument, the credit of the invention has, in 1798, been bestowed on the person who has the least claim to philosophical invention or candid narrative.

#### XIV.

##### *Description of an Apparatus for saturating Pot-ash and Soda with Carbonic Acid.*

*By Citizen WELTHER\*.*

THE intention of this apparatus is, to afford an uninterrupted contact between the alkali and the carbonic acid gas, and to proportion the disengagement of the gas to its fixation.

*Fig. 1. plate XVI.* represents the complete apparatus. It is composed of four distinct parts, A, B, C, D. The first, A, serves for the disengagement or production of the carbonic acid gas, whence it is distributed into the apparatus. The second, B, connects all the several parts of the apparatus together. The third, C, is the reservoir for carbonic acid gas. The principal function of this part is to supply gas to the alkali in proportion as the absorption takes place. And the fourth, D, contains the alkali which is to be saturated, in contact with the carbonic acid.

Each of these parts requires a particular description.

The part A, consists of a bottle with two necks *a, b*, (the third, *c*, is not necessary). This bottle contains sulphuric acid diluted with four parts of water, to the height *d*.—The neck *a*, receives a tube *e'e'e*, of which the upper part *e'e'*, diverges like the base of a funnel, and the lower part *e*, which passes through the cork and enters the bottle, is drawn out by the lamp, in such a manner, that the portion of tube which is in the stopper has the same internal diameter as the extremity *g* of the stem *f*, represented *fig. 2, A*; and that the orifice *e* within the bottle, is only two millimetres in diameter ( $\frac{1}{25}$  of an inch)†. Into the tube *e'e'e* enters the stem or stick of glass *f*, represented *fig. 2, A*, the lower extremity of which is bound with flax, so that it serves as a stopper in the contracted part of the tube, and does not permit its contents to flow into the bottle *a*, unless that stopper be raised.—The neck *b* contains a tube *h h h* bended into two angles, and forming a communication between A and B.

The part B, consists of a bottle with five necks *a b f i h*, of which the plan is represented *fig. 3, B*. This bottle contains water as high as *l*.—The neck *b* receives the tube *h h h*,

\* This apparatus was constructed in the first month of the third republican year. The description is translated from the *Annales de Chimie*, xxvii. 53.

† The tube *e'e'e* may be made out of a small matras with a long neck. The bottom may be taken away, leaving about half the body, which will form the part *e'e'* of the tube; the neck will afford the part *aa*, and the extremity of the neck softened and drawn out by the lamp to the above dimensions, will afford the extremity *e*. W.

proceeding;

proceeding from the bottle A.—The neck *b* contains a strait tube *g g'*, whose extremity *g'* is plunged in the water to the depth of one or two centimetres (about  $\frac{2}{3}$  of an inch.) It serves to shew the compression to which the carbonic acid gas is subjected in the apparatus.—The neck *a* contains a strait tube *a' a*, which allows a communication to be formed at pleasure from the interior part of the bottle B, with the atmosphere, by means of an apparatus of tubes shewn in *fig. 4*, B. The tube *a' a* has two stoppers at *m* and *n*: the stopper *m* enters the tube *m m*, enclosing the tube *a' a*; in the interval between these two tubes upon the stopper *m*, mercury is poured to the height *r* of twelve or fifteen millimetres (about  $\frac{1}{2}$  an inch): the tube *a' a* is then covered by a third tube *d* closed above, which being immersed at *p m* into the mercury, interrupts the communication of the atmosphere with the orifice *a'* of the tube *a' a*, and consequently closes the bottle B, when the stopper *n* is placed in the neck *a*.—The neck *i* contains a tube *i i*, composed like the former *a' a*; but of which the intermediate tube *o o o* twice recurved, establishes a communication between the bottle B and the reservoir C.—The last neck *f* contains a tube *f f f'*, twice recurved, which establishes a communication between the bottles B and D.

The part C is composed of a tub *t*, in the side of which there is a hole at a small distance above the bottom *a a*.—In this lateral hole is inserted a perforated stopper, the interior orifice of which receives a tube *b l' b'' b*, bended into a right angle, of which the portion *b'* must be bended in such a manner, that the angular part *b''* may touch the bottom *a a* of the tub, which renders it more firm in its position. In the external orifice of this cork, or stopper, there is screwed a brass cock *r*, of which the part *c* receives a tube *c c' c'' c*, bended at *c*, to rise vertically, and from *c'* to *c''* to bind against the frame *d d*. This tube, at the height *d*, is composed like that of *a' a*, described *fig. 4*, B. It there receives the tube *o o o*, which affords a communication with B.—The pieces which are applied to the hole *b* of the tub, are luted; the inner with graver's wax, and the outer with fat lute.—In the tub *t*, containing water to the height *e e e*, there enters an inverted jar C, provided with a copper cock at *l* and *m*. This jar is suspended at *g* by a string, which passes over the pulleys *b b*, and supports at *i*, a weight K, somewhat less heavy than the jar itself, when entirely plunged in the water contained in the tub.—When the jar C is totally immersed in the tub, it will rest on two pieces of wood about two or three centimetres thick, which are fixed at *l l l* near the inner circumference of the bottom *a a* of the tub (see *fig. 5*, C, which represents the plan of the tub; and *fig. 6*, which represents the vertical section as far as A B), so that the base *l' l' l'* of the jar cannot touch the tube *b*.

The part D consists of a bottle with three necks, *a, b, c*. The neck *a* receives a tube *a a' a*, proceeding to a small bottle *d*, containing water as high as *e*.—The neck *b* receives the tube *f f f'* proceeding from the bottle B; this tube ought to possess a diameter of about two centimetres ( $\frac{1}{4}$  of an inch) at its orifice *f'*.—The neck *c* contains a syphon *g g'*, communicating with the bottle *l*, of which the branch *g'* ought to be longer than *g*.—The bottle *l* has three necks, *b, i, k*.—It receives in its neck *i*, the syphon *g g'*; in the neck *k*, a tube *b b*, with one single bend; and the neck *n* remains free. It is to be corked.

After the formation of the apparatus by uniting the four parts here described, each of them demands a particular preparation before the operation can be proceeded upon.

The tube *e' e' e* of the part A is to be filled with carbonate of lime mixed with water.—Through the neck *b* of the bottle *l*, in the part D, that bottle is to be filled with alkali. The neck



neck *b* is then to be stopped with its cork, and by blowing through the tube *K* the alkali is to be forced through the syphon *g'g* into the bottle *D*. The air contained in this last bottle escapes through the tube *a a' a*, and passes through the water of the small bottle *d*. The fluid becomes nearly on a level in the bottles *D* and *I*, and the extremity of the tube *fff'*, which connects the bottles *B* and *D*, is plunged in the alkali.—Proceeding then to the part *c*, the cock *m* of the inverted vessel is to be opened, while that of the tub *r* is kept shut. By the excess of weight of the jar beyond that of *K* it sinks in the tub, and becomes filled with water, while the common air it contained escapes at *g*.—As soon as the inverted vessel, being totally immersed in the tub, reposes on the circular segments *///*, the orifice of the upper tube of the tube *b* is found in the brass receptacle *l*. It is necessary, that the level of the water, which by the immersion of the inverted vessel may have risen from *e* to *e'*, should have the elevation *be'* of the tube *b* equal at least to two centimetres higher, in order that this tube may not be closed by the water, which would prevent the gas from entering into the vessel *C*.—When this last vessel is full of water, its cock *l* must be shut, and the cock *r* of the cask must be opened.

In this disposition of the apparatus, the glass stem *f* of the part *A* is to be raised, and carbonate of lime gradually introduced, which falling upon the sulphuric acid becomes decomposed, and loses its carbonic acid. This last possessing the elastic state, passes through the tube *b b b* into the bottle *B*. The tube *d* is then to be raised, in order that the atmospheric air contained in the bottles *A* and *B* may flow out; after which, the neck *a* of the bottle *B* is to be closed, by replacing tube *d*. In this situation the carbonic acid gas, which enters *B*, (meeting less resistance in its passage to the inverted vessel through the tubes *ii* and *ooo*, than to pass into the part *D* by the tube *fff'*, which is plunged in the alkali) rises and fills the vessel *c*.—As soon as this event has happened, no more carbonate of lime is to be introduced into the bottle *A*, and the bottle *D* is filled with alkali by blowing through the tube *K* of the bottle *I*. The levels of the alkaline fluid are at the height *n* in the bottle *D*, and *m* < *n* in the bottle *I*; and the fluid tending to its level, by means of the syphon *g g'*, the surface *n* falls, and produces a vacuum in the bottle *D*.—The carbonic acid gas is then drawn into the bottle *D* by the tube *fff'*; at the same time that the water of the small bottle *d* (part *D*) rises in the tube *a a' a*; but the height of this tube is such, that the carbonic acid gas can enter the bottle *D* before the water rises to *a'* in the tube *a a' a*.—The two surfaces *n* and *m* acquire an equilibrium in the bottles *D* and *I*, and the carbonic acid gas, which occupies the upper part of the bottle *D*, combining with the pot-ash, a vacuum is formed, which is continually supplied with new gas.—When the vessel *C* is nearly exhausted of its gas, it must be filled by a new disengagement:

N. B. As the carbonic acid gas is mixed with a small portion of atmospheric air, this air, which is not absorbed by the pot-ash, accumulates in the bottle *D*, and may stop the operation. It must be driven out by blowing into the tube *b* of the bottle *I*, and filling the bottle *D* with alkali.

## XV.

*Abstract of a Memoir of KLAPROTH, on a new Metal denominated Tellurium. Read at the Public Session of the Academy of Sciences at Berlin, January the 25th, 1798\*.*

KLAPROTH, the chemist of Berlin, in the chemical analysis of the auriferous ore, known by the name of the white ore of gold (weiss golderz), aurum paradoxum, metallum vel aurum problematicum †, has discovered in that mineral, a metal absolutely different from all those which have hitherto been known, to which he has given the name of Tellurium, forming a kind of series or arrangement with the new metals discovered by him some time ago, and denominated Uranium and Titanium. Mr. Muller of Reichenstein, in the year 1782, had suspected the existence of a peculiar metallic substance in this mineral. Bergman, to whom he had forwarded a specimen of the ore, confirmed his suspicion; but on account of the small quantity upon which he operated, he did not think fit to decide, whether this fossil did actually contain a new metal, or whether it might not be antimony which he had mistaken for a new product. The numerous and ingenious experiments to which Klaproth has subjected a more considerable quantity of this ore, which was sent to him by Mr. Muller ‡, perfectly confirm the suspicions of that chemist, and of Bergman.

*The Process for obtaining this Metal from its Ore.*

1. A portion of the ore is gently heated, with six parts of muriatic acid; three parts of nitric acid are then to be added, and the mixture subjected to ebullition. A very considerable effervescence takes place, and the solution becomes complete.

2. The filtered solution is to be diluted with as much water as it can bear without becoming turbid, which quantity is very little. A solution of caustic pot-ash is then to be added, until the white precipitate, which is at first formed, shall disappear, and nothing but a brown deposition in flocks shall remain.

3. This last precipitate is a mixture of the oxides of gold and of iron, which may be separated by the usual methods.

4. To the alkaline solution (2), muriatic acid must be added, sufficient for the perfect saturation of the alkali; but not in excess. A white and very abundant precipitate is afforded, which, on the application of heat, falls to the bottom of the vessel in the form of a heavy powder. After washing and drying this precipitate, it is to be formed into a kind of paste, with a sufficient quantity of any fat oil; and this mass is introduced into a small glass retort, to which a receiver is loosely to be applied. In this disposition of the

\* This abstract was communicated on the part of the author, by M. Rose, a chemist of Berlin, and was translated and forwarded to the Phylomatic Society at Paris, by L. Hecht the younger. The French translation, which of course I must follow (as the original is unpublished), is inserted in the xxvth vol. of the *Annales de Chimie*, p. 273.

† This mineral is found in the mine called *Mariabils*, in the *Fatzbay* mountains near *Zaletzna* in *Transilvania*. See Emmerling's *Elements of Mineralogy*, II. 124. et seq. (or Kirwan, II. 324. N.)

‡ In the original the words are "M. de Reichenstein," which I suppose to be an oversight, and that I am correct in translating the words M. Muller de Reichenstein which occur a few lines before, on the supposition that M. Muller actually resides at Reichenstein, without deriving any titular name from that town. N.

apparatus, heat is gradually to be applied to ignition. In proportion as the oil becomes decomposed, brilliant metallic drops are observed, similar to those in the distillation of mercury, which line the upper part of the retort, and run down at intervals to the bottom of the vessel, immediately after which they are replaced by others. After the cooling, these metallic drops are found congealed, and adhering to the sides of the retort and the bottom of the vessel; and the rest of the metal, reduced in the form of a button with a brilliant surface, most commonly presenting a crystallized face.

*The essential Characters of this new Metal.*

1. Its colour is white like tin, but inclining to a leaden grey. Its metallic splendor is considerable; its fracture lamellated (*gerade blattrig*). It is very brittle and friable, and by slow cooling it readily acquires a crystallized surface.
2. Its specific gravity is 6,115.
3. It belongs to the class of the most fusible metals.
4. Heated with the blow-pipe upon charcoal it burns with a flame considerably brilliant, of a blue colour, but greenish at the edges; it rises totally in a grey whitish fume, and emits a disagreeable smell, which approaches that of radishes. If the flame be withdrawn before the small portion subjected to the heat is entirely volatilized, the remaining button preserves its fluid state for a long time, and, during the refrigeration, becomes covered with a radiated vegetation.
5. This metal amalgamates easily with mercury.
6. With sulphur it forms a sulphuret of a leaden-grey colour, and radiated structure.
7. Its solution in the nitric acid is clear and colourless; when concentrated, it spontaneously, in the course of time, affords small white and light crystals in the form of needles, which possess the dendritic aggregation.
8. The new metal is likewise soluble in the nitro-muriatic acid: when a large quantity of water is added to a solution of this nature, the metal falls down in the state of oxide, in the form of a white powder, which in this state is soluble in the muriatic acid.
9. When a small quantity of this metal is mixed in the cold, with one hundred times its weight of concentrated sulphuric acid, in a closed vessel, the fluid gradually assumes a beautiful crimson red colour. By the addition of a small quantity of water, added drop by drop, the colour disappears, and the minute portion of metal which was dissolved falls down in the form of black flocks. Mere heat also destroys this solution; it causes the red colour to disappear, and disposes the metal to separate in the state of a white oxide.
10. When, on the contrary, the concentrated sulphuric acid is diluted with two or three parts of water, and a small quantity of nitric acid is added, this mixture dissolves a considerable portion of the metal. The solution is clear and colourless, and is not decomposed by the mixture of a greater quantity of water.
11. All the pure alkalis precipitate from the acid solutions of this metal a white oxide, soluble in all the acids. The precipitate is entirely soluble by excess of alkali. If a carbonate be used instead of a pure alkali, the same phenomenon takes place; with this difference, however, that by an excess of the carbonate the precipitate which is formed is redissolved in part only.

12. A very pure prussiate of potash occasions no precipitate in the solutions of this metal. This is a remarkable exception to the general rule of metallic precipitations; which, however, it partakes with gold, platina, and antimony.

13. The alkaline sulphurets, mixed with the acid solution, occasion a brown or blackish precipitate, accordingly as the metal is combined with more or less of oxygen. It sometimes happens that the colour of the precipitate perfectly resembles kermes mineral, or the red sulphurated oxide of antimony. When the sulphuret of tellurium is exposed on an ignited coal, the metal burns with a blue colour, together with the sulphur.

14. The infusion of the nut-gall, combined with the same solutions, affords a precipitate in flocks of an Isabella colour.

15. Iron and zinc precipitate tellurium from its acid solutions in the metallic state, under the form of small black flocks, which resume the metallic brilliancy when rubbed, and are fused into a metallic button upon an ignited coal.

16. Tin and antimony occasion the same phenomenon with the acid solutions of the new metal. The precipitate formed by antimony proves, in the most striking manner, that tellurium is not antimony disguised, as had been supposed. The solution of tin in the muriatic acid, mixed with a solution of tellurium in the same acid, likewise produces a black metallic precipitate.

17. The oxides of tellurium, obtained from the acid solutions by alkalis, or from alkaline solutions by acids, are in either case reduced with a degree of rapidity approaching to detonation, by exposure to heat upon charcoal. It burns, and is volatilized, as has already been mentioned.

18. By the application of heat, for a certain time, in a retort, this oxide of tellurium becomes fused, and, when cold, appears of a straw colour, with a kind of radiated texture.

19. With the addition of any fat substance, the oxide of tellurium is perfectly reducible by the method before described.

The white ore of gold from *Futzbay*, aurum vel metallum problematicum, contains, in 1000 parts, tellurium in the metallic state, 925,5; iron, 72,0; gold, 2,5.

The graphic gold of Offenbanya contains, in 100 parts, tellurium in the metallic state, 60; gold, 30; silver, 10.

The mineral known by the name of the yellow ore of Nagyag contains, in 100 parts, metallic tellurium, 45,0; gold, 27,0; lead, 19,5; silver, 8,5; and of sulphur a minute portion.

The mineral known by the name of the grey foliated ore of gold from Nagyag contains, in 100 parts, lead, 50; metallic tellurium, 33; gold, 8,5; sulphur, 7,5; silver and copper, 1.

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After the above was printed in the *Annales de Chimie*, the editors of that excellent work received a translation of a manuscript addressed by M. Klaproth to Cit. Van Mons, on their behalf. From this they extracted the following mineralogical and chemical details \* :

The colour of the metallum paradoxum, of the mine of *Fatzbay*, is between the white of tin and the grey of lead: it has much metallic brilliancy. It is sometimes in lumps, and then forms an aggregate of crystalline grains. Its texture is usually fine or small grained.

\* *Annales de Chimie*, xxv. 327.

Its matrix is composed of quartz and marl. The subject of the first analysis was taken from a piece of the compact variety detached in 1780.

The graphic gold of the mine Franciscus at Offenbanya, forming the second variety, is of the white colour of tin, partly inclining to the yellow of brass: it is very brilliant, composed of prismatic crystals, flat and compressed, of which the mutual position affects the form of the characters of Turkish writing, which has given rise to its empirical name. It is usually found between the greyish blue argillaceous porphyry, bedded in grey quartz. The proportion of the constituent parts of this ore vary much: the middle term has been taken.

The third variety, called the yellow ore of gold, is of a white silver colour inclining to the yellow of brass, compact, and interspersed with quartz and brown spar. It is not known whether that specimen which presents radiations of considerable magnitude, and exhibits a lamellated texture and fracture, is of the same species: it is found in a mixture of quartz crystals, and brown red spar, and sometimes in the foliated ore.

The foliated ore, or grey foliated gold ore of Nagyag, differs, in its chemical and mineralogical characters, from the three foregoing, which are comprised under the name of white gold ores; which has caused it to be admitted, in the new system of mineralogy, as a particular species of the genus Gold. Its colour is a deep leaden grey, inclining to the iron black. It is seldom found compact, but most commonly inserted in the form of small united leaves, and likewise in thin, oblong, hexahedral tables, partly accumulated in cavities. Its metallic splendor is but moderate: its fracture most commonly exhibits contorted leaves; it is speckled, and in some specimens the leaves are slightly flexible. Its matrix is composed of quartz mixed with reddish manganese, which it has penetrated in every direction.

Scopoli, Sage, and Ruprecht, attempted to analyse this ore; but the difference of their results rendered their experiments very uncertain. They had clearly ascertained the presence of a substance volatile by heat; but they were deceived in taking it sometimes for arsenic, and in other instances for antimony.

It is to M. Von Muller, at present director of the mines at Zalathna, that M. Klaproth ascribes the honour of having first observed this new metal (in the collection of Memoirs of the United Friends of Vienna, published by Born). He expresses his acknowledgment for the specimens he sent to him, which enabled him to prove its existence. He does not omit the observation, that Bergman, though he durst not decide concerning the true character of this metallic substance, did nevertheless declare that it was not antimony.

We have seen, in the foregoing abstract, (p. 372, No. 3.) that the precipitate which is not re-dissolved by ammoniac is a mixture of gold and iron. There are, doubtless, several methods of separating these two metals; but, perhaps, it may be satisfactory to know the method which was used by M. Klaproth. He re-dissolved the whole in the nitro-muriatic acid, and precipitated the gold by a solution of the nitrate of mercury made without heat, which he poured gradually into the former solution, till the precipitate which fell down had changed its brown colour for white. The precipitate, being carefully collected, was reduced into pure gold.

"I shall give," says M. Klaproth, "in the third volume of my *Beitraege*, &c. the particulars of these analyses of the gold ores of Transylvania which contain tellurium,

after having once more repeated them, in order to determine the proportions with more certainty and precision. The researches of mineralogists and chemists will soon inform us whether tellurium is likewise met with in other places, or whether nature has exclusively appropriated this metal to the gold mines of Transylvania.

### SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

#### AMERICAN NEWS.

**I**N consideration of the general utility that would result from the citizens of the United States being enabled to procure, free from expence, an analysis of any ores, or mineral substances, "The Chemical Society of Philadelphia," on the 20th of June 1797, passed the following Resolution :

"Resolved,

"That a Committee of five Members be appointed, whose business it shall be to notify in the different papers of the United States, and by circular letters, that they will give an analysis of all minerals which may be sent them."

In conformity to the above resolution, they have given notice that they will analyze any mineral which may be sent them, provided it be forwarded free of expence, and accompanied with an account of the place and situation in which it was found.

#### COMMITTEE.

Thomas Smith, No. 19, North Fifth Street.  
James Woodhouse, No. 13, Cherry Street.  
Samuel Cooper, No. 178, South Front Street.  
Adam Seybert, No. 191, North Second Street.  
John C. Otto, No. 37, North Fourth Street.

Professor Barton of Philadelphia, who has lately published a small tract on the Vegetable Materia Medica of our country, is preparing for the press a work to be entitled "Strictures on the Arrangement of the Materia Medica, adopted by Dr. Darwin in his Zoonomia."

January 19, 1798. The *American Philosophical Society* held their annual election of officers on the first Friday of this instant, when the following were duly chosen :

President—The Hon. Thomas Jefferson.

Vice-Presidents—Nicholas Collin, D. D. Dr. Benjamin Rush, and Dr. Caspar Wistar.

Treasurer—Mr. John Vaughan.

Secretaries—Samuel Magaw, D. D. Dr. Adam Seybert, Dr. J. C. James, and Mr. Samuel H. Smith.

Curators—Mr. Charles W. Peale, Dr. Benjamin S. Barton, and Mr. Robert Patterson.

Class of Counsellors for three years—Mr. Jonathan B. Smith, Dr. William Currie,

William Smith, D. D. and Mr. Jonathan Williams, two years from January 1798.

## AEROSTATION.—August 12, 1798.

BLANCHARD ascended into the atmosphere, for the forty-sixth time, by means of an apparatus consisting of five balloons attached to a car, and a sixth small globe. He threw out a dog attached to a parachute, which descended gradually, but of whose fate no mention is made in the *Moniteur*, whence I extract this intelligence. He rose at half past noon from Rouen, ascended to the height of 2500 toises, and landed again at 55 minutes after two, near the village of Bazancourt, 12 leagues distant from the first place. We have no account of any remarkable observation made during this voyage, nor the reasons why his apparatus was so complicated.

Cit. Garnerin, on the 28th of the same month, made his eleventh ascension from Paris. His course for a considerable time was near the ground, during which he conversed with the people below. These conversations shewed how much the earth reflected sound; for all his words were repeated five or six times. He thought at first that it might be governed by some local circumstances, which indeed is very probable with regard to the repetition. He descended several times to the same level, at distances of ten leagues asunder, where he constantly observed the same effect. This great vibration of the air was not sensible to distances exceeding 150 or 200 toises. It decreases with the distance.

Does the vertical transmission of sound differ from that which is made in an horizontal direction? On this head, however, may be read the very entertaining account of David Frœdlichius, at the end of the 19th chapter of the first book of Varenus, who ascended the highest eminences of Carpathas, near Kelmärkt, in Hungary. It is copied by Derham, in the first volume of his *Physico-Theology*.

This aéronaut was accompanied by a female Citizen, Henry. They rose at 25 minutes after four, and descended at nine in the evening, at the gates of Chalons, forty leagues from Paris, and thirty-seven from the place whence they departed.

Essays Political, Economical, and Philosophical, by Benjamin Count of Rumford.

Essays VIII. and IX.

THESE two Essays have been before published in the *Philosophical Transactions*; the one as long ago as the years 1786 and 1792, and the other in 1798. As this last is inserted in our *Journal*, Vol. II. p. 106, it will be unnecessary to insert its contents in this place. Both will be highly acceptable to the possessors of the other Essays of the Count, to complete the collection of which these were wanted.

The contents of the eighth Essay are, Chap. I. An Account of the Instruments that were prepared for making the proposed Experiments. A Thermometer constructed, whose bulb is surrounded by a Torricellian Vacuum. Heat is found to pass in a Torricellian Vacuum with greater difficulty than in Air. Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat, determined by Experiment. Relative conducting Powers of dry Air and of moist Air. Relative conducting Powers of Air of different degrees of Density. Relative conducting Powers of Mercury, Water, Air, and a Torricellian Vacuum.—Chap. II. The relative Warmth of various Substances used in making Artificial Clothing, determined by Experiment. Relative Warmth of Coverings of the same Thick-

ness,

nels, and formed of the same Substance, but of different Densities. Relative Warmth of Coverings formed of equal Quantities of the same Substance, disposed in different ways. Experiments made with a view to determining how far the Power which certain Bodies possess of confining Heat depends on their Chemical Properties. Experiments with Charcoal, with Lamp-black, with Wood-ashes. Striking Experiments with Semen Lycopodii. All these Experiments indicate that the Air, which occupies the Interstices of Substances used in forming Coverings for confining Heat, acts a very important part in that operation. Those Substances appear to prevent the Air from conducting the Heat. An Enquiry concerning the Manner in which this is effected. This Enquiry leads to a decisive Experiment, from the result of which it appears that Air is a perfect Non-conductor of Heat. This Discovery affords the means of explaining a Variety of interesting Phenomena in the Economy of Nature.

*Traité de la Sphere et du Calendrier*, par Rivard, 5me Edition, revue et augmentée par Jérôme Lalande, 1 vol. 8vo. avec Gravures, à Paris. A Treatise on the Sphere and the Calendar, by Rivard, revised and augmented by Jerome Lalande, 1 vol. 8vo. with Plates.

THE modern advances in Astronomy rendered it necessary to make some alterations in this Work, which possesses a high character for Perspicuity and Accuracy. Cit. Lalande has corrected the Table of Latitudes and Longitudes, and added a Chapter on Time, besides making other Improvements.

*Philosophy of Mineralogy.* By Robert Townson, LL.D. F.R.S. Edin. 8vo, 219 pages, with three Engravings.

THIS performance is the outline of a larger work formerly announced, and intended to have been accompanied by a collection of fossils, but which did not meet with the expected support. It consists of twelve chapters. The three first contain an introduction, with an account of the simple elementary substances of which minerals are composed, and the laws of aggregation and combination by which they are governed. These are followed by an enumeration of compounds, according to Dr. Babington's excellent "Systematic Arrangement," and four chapters respectively treating upon Stratification, the Irregularities of the Earth's Surface, Veins, and Petrifications. The Author then proceeds to consider the value and use of the external characters of Minerals; and gives a very ample terminology, consisting of the appellations in English, Latin, and German, with their correspondent definitions, under the titles of Colour, Figure, Surface, Lustre, Texture, Structure, Fracture, and Fragments; Transparency, Scratch, Score, and Soiling; Cohesion, Adhesion, Sound, Feel, Coldness, Density, Smell, Taste, and Friability: To which he adds the Habitudes, or Results of Experiment. Two subsequent chapters indicate the use of these terms in Classification, Description, and Investigation; with short Instructions for collecting Specimens, and forming Cabinets. The concluding chapter contains a list of near three hundred works on Mineralogy.



**Proceedings of the Association for promoting the Discovery in the Interior Parts of Africa, &c.**

Abstract of Mr. Park's Travels. [Concluded from page 332.]

AFTER travelling upwards of a month, ascending by the side of the Niger till it ceased to be navigable, he at length sunk under his fatigues, and the difficulties of his enterprise; and at Kamalia, five hundred miles short of any friendly country, on the Gambia, he fell into a severe and dangerous fit of sickness. If in this situation had he been able to travel, great part of his way lay through a desert. He had therefore no other resource but to wait for the first caravan of slaves which might travel the same track. Such a one was expected to pass through Kamalia at the end of three months, and the chief director resided at the place. To him, therefore, Mr. Park applied; and for the value of one slave, to be paid on his safe arrival at the Gambia, this worthy negro, whose name was Karfa Taura, not only undertook to conduct him safe to Pisania, but offered him likewise the accommodation of his house until the time of the caravan's departure. Under this man's roof our traveller was confined to his mat (his only bed), by a severe and dangerous fever, for upwards of a month. Five months longer was he detained for the caravan. During this long interval, not a murmur escaped the lips of Karfa, nor of any of his wives, at the trouble and expence which their inmate brought upon them. To the kind attentions, the tender solicitude, the cheerful assiduity, and flowing hospitality, of these poor Pagans, Mr. Park declares that he is indebted, not only for his safe return to Great Britain, but also for the preservation of his life; and he admits that he made his friend Karfa but an inadequate return (though the best in his power), by presenting him, on their arrival at the Gambia, with double the sum that he had originally promised.

During this long confinement of Mr. Park, he acquired much information respecting the trade in slaves and gold-dust, the vegetable productions of Africa, the character of the natives, their agriculture and manufactures, their modes of living, manners, superstitions, wars, police, and government, which have never yet been competently described, and for which we must wait for the appearance of his work. In the mean time it may be remarked, that though the climate on the borders of the desert is prodigiously hot, yet in the southern districts, which abound with wood and water, the climate improves, and in the mornings and evenings the air is serene, temperate and pleasant. Some of the vegetable products have been noticed. To these may be added the Lotus, of ancient renown, affording a small, yellow, farinaceous berry about the size of an olive, which being pounded in a wooden vessel, and afterwards dried in the sun, is made into excellent cakes resembling the sweetest gingerbread. Most of the edible roots of the West Indies are likewise found here, together with indigo, cotton and tobacco; but neither the sugar-cane, coffee, cacao, the pine-apple, nor a variety of other fruits, were seen by him, nor known to the natives. Uncultivated lands belong to the state; but in other respects landed property is admitted, without, as it should appear, any particular feudal or other limitations. Among their manufactures may be reckoned, an excellent beer made from corn; the fabrics of cotton cloth, which are dyed with indigo; the tanning of leather, which is stained both yellow and red; the smelting of iron, though imperfectly; and the casting and working of gold.

From

From the conduct and cruelty of the wars between the petty and independent states of Africa, it appears that those who do not make slaves of their captives put them to death. With regard to religion, the Mahometans shew much zeal in teaching the Negro children to read, and avail themselves of this means to convert them from Paganism. Circumcision, which is notoriously more ancient and extensive in its prevalence than the religion of Mahomet, is practised also by the Negroes, who consider it rather as an operation of physical than religious necessity. The conviction of a future state of rewards and punishments was universally prevalent in every district visited by our traveller.

In the latter end of April 1797, the caravan being completed, and Mr. Park's health perfectly re-established, he set out from Kamalia, in company with seventy persons, of whom thirty-seven only were slaves for sale. On the 4th of June they fell in with the river Gambia; and in six days more, namely on the 10th, Mr. Park had the satisfaction to enter the house of Dr. Laidley, from which he had set out eighteen months before. On the 15th of the same month, he embarked in a slave-ship for America; which being driven by stress of weather into the island of Antigua, Mr. Park took his passage from thence in a vessel bound to Great Britain, and on the 25th of December arrived safely in London.

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\* \* The respectable Writer of a Paper on the Secondary Foci of Lenses, is informed, that a bad sort of concave Reflectors has long since been constructed and sold in London by silvering one surface of a convex lens; and that it is probable these foci (which are easily determined from the curvatures and refractive powers) may have been neglected by optical writers, because the images are in every case more dilute and indistinct than such as are produced either by refraction, or reflection alone. That the effect has not been disregarded as a source of imperfection in lenses, may be seen by consulting our Journal, II. 233.

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*Fig. 2.*

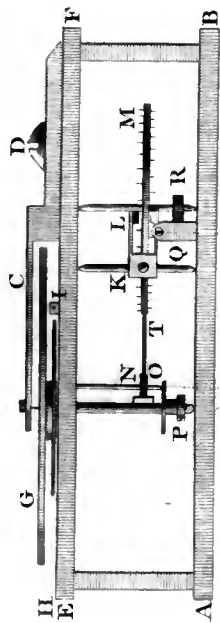
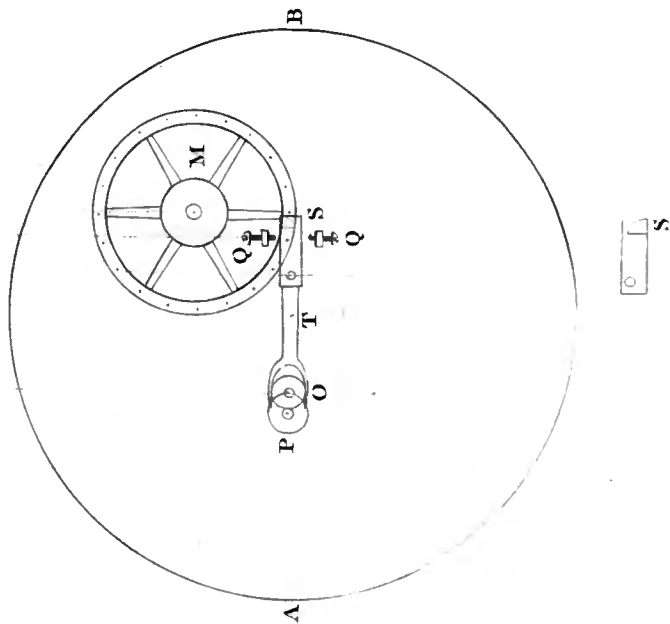


Fig. 1.



*Fig. 3.*

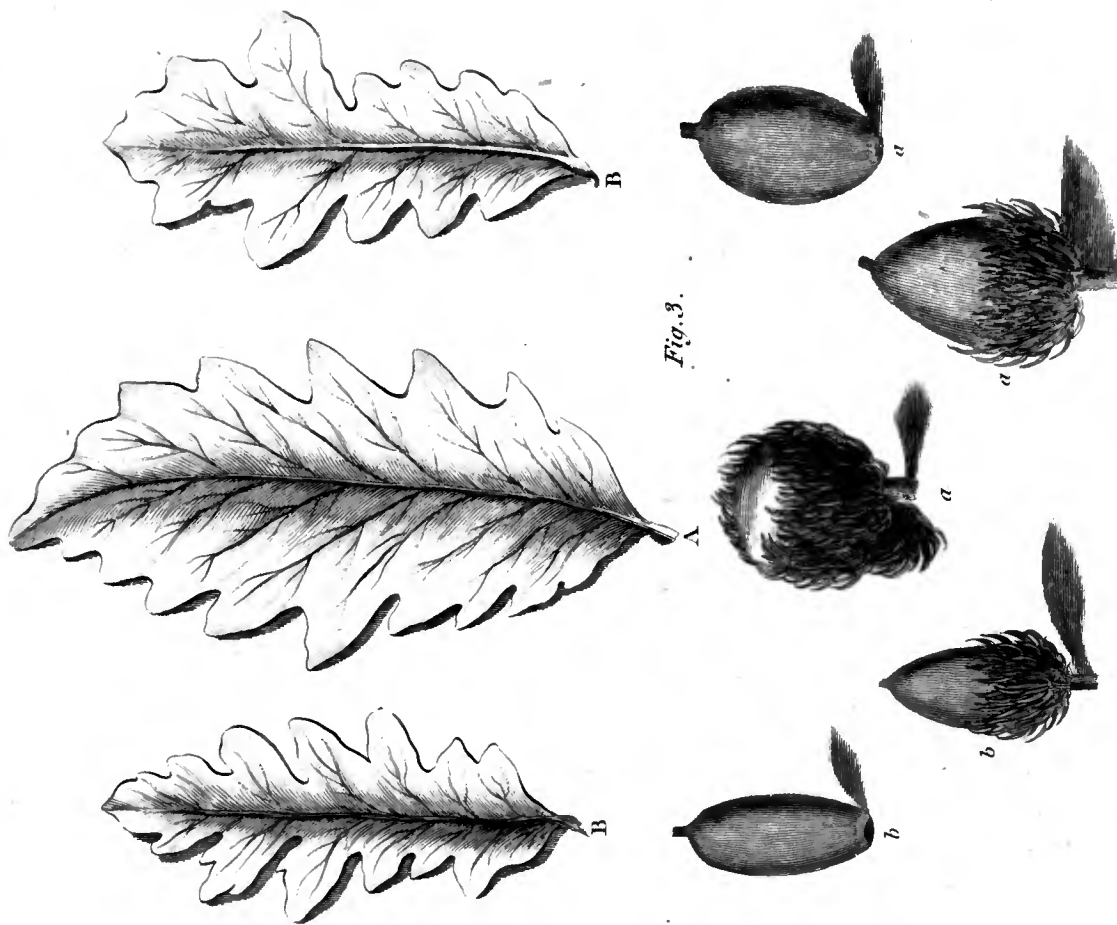
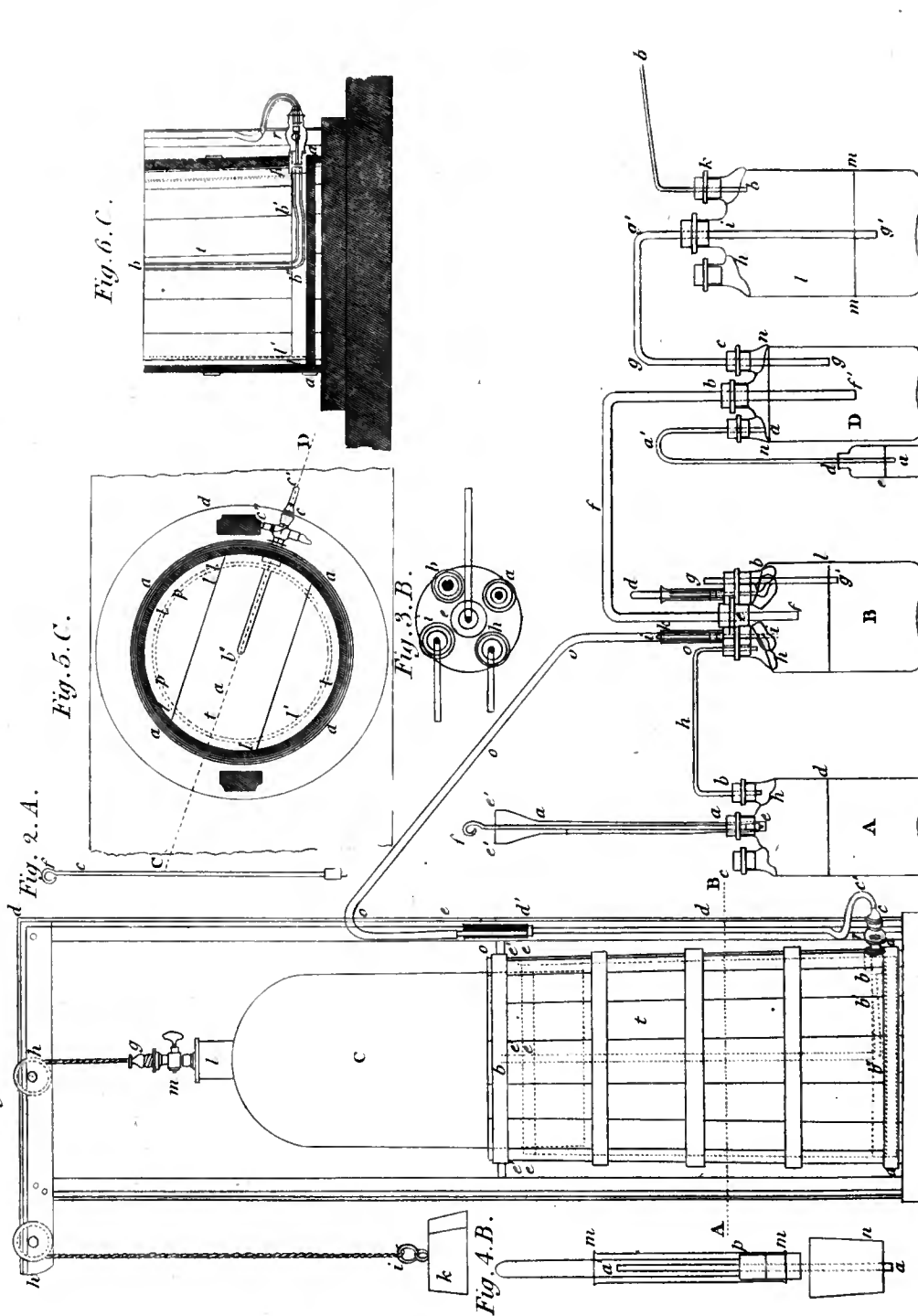
*Barrow snail.*



Fig. 1.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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DECEMBER 1798.

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ARTICLE I.

*Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M.R.I.A. Corresponding Member of the Royal Society of Edinburgh, &c.\**

IT is generally supposed that the seasons in our island have suffered a considerable change, almost within the memory of the present generation. The winters of our climate are said to have laid aside their ancient horrors, and frequently to have assumed the mildness and vegetative powers of spring; while summer is represented as less favourable than heretofore, less genial in promoting vegetation, and less vigorous in forwarding the fruits of the earth to maturity.

It is indeed true, that in this instance popular opinion does not stand supported by the concurrent testimony of meteorological observations: there is no clear evidence derivable from them, that the present seasons are materially different from former ones; and therefore philosophers and meteorologists naturally ascribe to the querulous disposition of the farmer, the chill sensations of old age, or the predilection which every one feels for the cheerful days of childhood, the adoption of an opinion that seems so easily to flow from these sources.

But let it be remembered, that the instruments of atmospherical observations do not extend to all the circumstances which influence the crops of the farmer, or the sensations of the man. The thermometer may mark the general temperature of our climate as unchangeable; and the pluviometer may ascertain its usual moisture; whilst a clouded atmosphere or a tempestuous wind shall mar the progressive maturity of harvest, and shatter the languid frame of declining age.

Heat and cold and rains are, indeed, principals in the economy of seasons; but winds, clouds, vapours, and other circumstances rarely registered, often unperceived, are to be deemed at least ancillary in the extensive system; and may give plausibility to popular sensations and opinions, even without the aid of meteorological testimony.

It is the purpose of this paper to offer to the Academy some observations relating to this interesting subject; and to mark a few prominent events in the phenomena of our climate, which may add credibility to general report.

### *Of the Winds, and their Effects.*

THE winds which most usually prevail in our latitudes blow from the westward, for reasons unnecessary to be detailed here. These winds are commonly mild in their temperature, and moist in their nature. They are from these properties extremely friendly to animal and vegetable life; and to them the great population of Ireland, and the uncommon fertility of its soil, may among natural causes be ascribed.

But from whatever circumstances it has arisen, these winds have of late years swept with uncommon violence over the surface of our island; frustrating the usual effects of their genial properties by the overbearing fury of their course; and, like Saturn, sometimes devouring the offspring to which themselves had given birth.

Why these westerly winds have ceased to bear the character of zephyrs may admit of much curious and interesting investigation: at present I shall be satisfied with endeavouring to establish the fact itself, by suggesting to the Academy some circumstances that seem to determine the matter with a very great degree of probability.

The effects of these winds are marked in visible characters over the whole surface of the kingdom; but they are peculiarly distinguishable in the northern province of Ulster; and chiefly in the extreme countries of that province, where a northerly latitude, joined to an exposed situation on the coasts of the ocean, forms an apt station for observations, and exhibits as it were on a magnified scale the degrees of the phenomena themselves.

Three natural registers of these effects have come within my observation; the trees of the country, the sands of the sea-coast, and the tides of the ocean: of each of these I shall make mention in its order.

### *Of the Trees.*

IT is a fact extremely well established, that the pine-tree, peculiarly that species vulgarly denominated the Scotch fir, formerly grew on many of the mountains of this kingdom, and on parts of the northern and western coasts, exceedingly bare and open to storms. Vast roots and noble trunks of this species of pine have been seen and examined by me with attention, in situations where human industry cannot now rear a twig of the hardiest tree. On the highest lands of the general surface of the kingdom, in the county of Westmeath, amid the mountains of the county of Antrim, and on the naked coasts of Enishowen and Rosses, in the county of Donegal, pine-trees have formerly arrived at an age of an hundred and twenty years, have grown to the size of a yard in diameter, and surpassed fifty feet in height.

There is great reason to think that two centuries have hardly elapsed since many of these trees grew in those situations; and probable reasons might be adduced to limit the great pe-



riod of their destruction to the age of James the first of England. In those reigns, rewards were held out for settling the kingdom, and clearing its surface of forests, which under favour of incessant wars and neglected tillage, during a period of eight centuries, had overspread the face of the country\*.

The harsh and furrowed bark of this pine has occurred to me in such a perfect state of preservation as almost alone to determine its species†.

The cones have been found by me at a depth of many feet from the surface of the earth, in such condition as almost to give hope of raising plants from their seed‡. Marks of the woodman's hatchet on their trunks; vestiges of fire applied for their destruction; and pieces of charcoal into which many of them have been burnt§; paleings and small enclosures found at the level where they have formerly grown||.

Leathern shoes, wooden vessels filled with butter and other light substances found at considerable depths in turf bogs\*\*, and not likely to have descended through the matted texture of that substance, give additional testimony to the opinion that the existence of these bogs, and of course that of the trees which they contain, is not of an extremely ancient date.

It is needless to recall the attention of the Academy to the difficulty of raising trees, at present, in many of those situations where the ancient pine and oak of Ireland have within the period of human existence flourished with luxuriance.

The labours of the farmer, the resources of wealth and information, the rewards of patriotic societies, and even the liberal encouragement of the legislature itself, have in vain struggled against the western storms during the latter part of the present century; and the planters of our age, wearied with combating the tempest, have generally found it necessary to fly from all elevated and exposed situations, and to abandon the pleasing idea of covering the nakedness of mountains, the sterility of rocks, and the bleak uniformity of bogs, with the luxuriant foliage of the oak and the pine.

Of all the forest trees which in later times have been cultivated for general use, there is none higher in the estimation of our farmers than the ash. It is a tree which buds late, but finally issues forth strong and succulent shoots: secure by its deciduous nature from wintry blasts, it is nevertheless extremely sensible to the efforts of summer storms; and becomes

\* "In this reign ipe-staves was one of the ordinary exports of Ireland; so that a mighty trade was driven with them, and thousands of trees were felled every year for this purpose. A multitude of iron-mills were erected; and it is incredible how much charcoal a single iron-mill will consume in one year. So that all the great woods which the maps shew us, on the mountains between Dundalk and Newry, are quite vanished, except one tree close to the highway, at the very top of the mountain, which, as it may be seen a great way off, therefore serveth travellers for a mark."——

"Yet there are still great woods remaining in Dunnagall, in Tyrone, in Antrim, &c." See Nat. Hist. of Ireland, by Boates, Molleneux, and others, written about the middle of the last century.

† In Bracknaghlin bog, county of Westmeath.

‡ In Lackbeg bog, near Rutland, county of Donegal.

§ Found in a bog in the liberties of the city of Londonderry.

|| In a bog near Surock, county of Westmeath; near Kilrush, county of Clare; at Carnisk, near Ramelton, county of Donegal.

\*\* Omitting other instances, two wooden vessels containing butter were very lately found deep in a turf bog, in the Fews Mountains, near Ballymoire, the seat of Sir Walter Synnott. The vessels were extremely inartificial, being little better than the hollow trunks of some large species of willow: the butter was insipid, inodorous, colourless, somewhat resembling unctuous white scatites in its touch and appearance; but its inflammability remained so perfect, as to admit of its being made into candles, to which use much of it was applied.

a faithful register of the winds of our climate, marking their most prevalent direction by the inclination of its boughs, and their violence, by the degree wherein its tender shoots or more mature branches are withered or blasted.

Half a century has scarce elapsed since this useful species of tree was generally planted, and grew luxuriantly in every part of Ireland: yet are there hardly any exposed places in the kingdom where its top branches do not now in one part or another exhibit the withered vestiges of commencing decay. Through many parts of Ulster it is a blasted tree; and in all unsheltered situations in the three northern counties of Antrim, Derry, and Donegal, the species seems fast verging toward annihilation\*.

Attentive to each prognostic of the seasons, alive to every circumstance whereon the future subsistence of his numerous family in a populous country may depend, the farmer of the north sees these vestiges of the tempest with solicitude; and often taught of late to dread the effects of summer storms on his luxuriant crops of potatoes, he has become querulous from experience, and thinks he has some reason to complain that the seasons are less favourable to his hopes than formerly; that the pine-tree has not forsaken the mountains, nor the ash been blasted in his fields, without the influence of increasing tempests.

#### *Of the Sands.*

THE waste and dreary solitude of extended barren sands which almost every sea-coast exhibits, is generally passed over with rapidity, as useless to the philosopher from the incalculable fluctuation of its surface, and wearisome to the traveller by its disgusting uniformity.

Yet, in the midst of such a sterile scene, where nothing occurs to divert the path or distract the pensive mind of the traveller, if traces of human existence should suddenly become visible; if enclosures should appear, to mark the cheerful fire-side of some former villager, or the circuit of his little garden; if embattled walls or marble piers should start up amid the sands, suggesting ideas of ancient elegance and festivity; he must have a Stoic's mind, indeed, who will not venture to calculate causes, and feel interested in events, that come home so closely to the human heart:

On many parts of the coast of Ireland scenes such as these may be discovered.

At the entrance of the river Bannow, in the celebrated barony of Forth in the county of Wexford, vestiges of ruins, traced with difficulty amid the heaps of barren sand, serve to ascertain the site of a town, whose ancient respectability was marked by the royal charter, which endowed it with the privilege of sending representatives to the parliament of the nation, and whose opulence may be inferred from the streets which are registered in the revenue records of the last century †.

\* In Lancashire, in England, between Garstang and Preston, the trees are also cut off by the westerly winds. See Newte's Tour, year 1791.

† "So late as the year 1626, Bannow is registered in the custom-house books of Wexford, as having four streets which paid quit-rent to the crown, viz. Lady-street, High-street, Weavers-street, Little-street, and some buildings surrounding the church. The only remains of Bannow which stand visible at this day (1786) are the walls of its church. There is not on or near the town but one poor solitary hut. The election for the representatives of the town is held on the walls of an old chimney adjoining to the church, which tumbled down piece-meal, and forms the council-table of that ancient and loyal corporation."

*Private Letter of a Friend in the County of Wexford.*

*Amid*

Amid the sands between Portrush and Dunluce, in the county of Antrim, in the year 1783, the ruins of a village might be seen deserted by its inhabitants, who had been obliged to move further into the country.

In the year 1787, the peninsula of Hornhead, in the county of Donegal, contained vestiges of enclosures so small and so numerous as to mark the residence of a considerable number of human families, in a spot which exhibited nothing but

—— “ a desert, salt and bare,

“ The haunt of seals and orcs, and sea-mews clang.” MILTON.

Somewhat about a century ago, the peninsula of Ross-gull, lying between the harbours of Sheep-haven and Mulroy, in the county of Donegal, was selected as the residence of one of the noble families of Hamilton, titled Boyne. It is to be presumed there was then but little apprehension, that the elegant edifice of that age should, after the short interval of an hundred years, stand, like Tadmor of the East, the solitary wonder of a surrounding desert.

For the age wherein it was built, and the style of architecture of that day, the mansion of Roseapenna may be called elegant. The approach was from a level green on the shore, through a succession of embattled courts and hanging terraces, rising in order one above the other, and adorned with marble piers of no mean design and workmanship.

The rear was ornamented with gardens laid out and planted in the fashion of the last century; and the parks and fields of the demesne seem to have been well divided and enclosed.

At present, every object in this place presents to view peculiar characters of desolation. The gardens are totally denuded of trees and shrubs, by the fury of the western winds: their walls, unable to sustain the mass of overbearing sands, have bent before the accumulated pressure, and, overthrown in numberless places, have given free passage to this restless enemy to all fertility. The courts, the flights of steps, the terraces, are all involved in equal ruin, and their limits only discoverable by tops of embattled walls visible amid hills of sand.

The mansion itself, yielding to the unconquerable fury of the tempest, approaches fast to destruction: the freighted whirlwind, howling through every avenue and crevice, bears incessantly along its drifted burthen, which has already filled the lower apartments of the building, and begins now to rise above the once elevated thresholds. Fields, fences, villages, involved in common desolation, are reduced to one undistinguishable scene of sterile uniformity; and twelve hundred acres of land are said thus to have been buried within a short period in irrecoverable ruin\*.

Hence

\* It would be teasing to dwell on a repetition of similar examples. I shall just mention two others:—In a summer excursion from College in the year 1787, passing from Dunfanaghy to Rutland, along the western coast of Donegal, I had great difficulty to discover a house situated between the river Guidore and the Rosses islands, whither I had been directed to enquire for a guide; and after much search at length perceived its roof just emerging from the sands. The owner told me that his house was not long built, and had at first a considerable tract of pasture ground between it and the sea-shore; but that of late he was every year obliged with great labour to dig it out of the encroaching sands, and purposed shortly to remove it to the opposite shore of a lake, called Mullochdearg, which lay behind the house, in despair of being able to maintain his present situation.

A disputed.

Hence it appears, that interesting natural events, the consequence of storms, have occurred on our coasts in the latter part of the present century, which were not foreseen, nor even suspected, at its commencement.

*Of the Tides.*

IT cannot be wondered at if these tempests should have had considerable effect on the tides of the ocean. The waters of that element, as long as they are unimpeded by extraneous causes, obey the influence of the heavenly luminaries, and ebb and flow with degrees of quantity and regularity of periods which come within the reach of human calculation. But, when they are agitated by storms or other violent convulsions, their quantities and periods become altogether uncertain and incalculable.

Generally speaking, agitation of the ocean, from whatever cause it may arise, produces increase in the influx and reflux of its tides, as well as deviation from their calculated times; and where the movement of this extraneous influence coincides with the natural direction of the waters, the effects are visibly distinguishable by the traces of inundation which attend their unusual progress.

As the tides of our coast raised in the Atlantic ocean flow in upon us from the westward, a storm from that quarter invariably gives them an uncommon elevation in our harbours; and this accumulation of waters sometimes anticipates the tempest itself, becoming the forerunner and prognostic of its distant commencement and approaching impetuosity.

Of late years these extraordinary influxes of the ocean have been much greater and more frequent than formerly. Every person on our coasts, whose situation has made the construction or preservation of embankments against them necessary, knows, by painful experience, how much his labours have of late years increased, and how impotent works formerly effectual are now found to be in repelling the increasing tides of the present day. Public roads encroached on; walls beaten down; strands less passable than heretofore; meadow and tillage land oftener and more deeply inundated; all concur to prove increasing tides and frequency of storms on our coasts.

These phenomena faithfully registered, extensively and distinctly delineated in natural characters, independent of every bias from human system or prejudice, free from the uncertainty of casual records or the locality of peculiar stations for observation, seem directly to demonstrate an unusual and increasing violence in the winds of our climate during the present century. That these tempests have chiefly borne upon us from the westward is plain, from the same general appearances: for, where local circumstances have not directly interfered in opposition, the trees, shrinking before the pressure from the ocean, have universally yielded to the western blast. The sands have drifted, and the tides rushed upon us from the same quarter, evidently demonstrating the more frequent recurrence and superior potency of the Atlantic storms.

[To be continued.]

A disputed boundary in Favet, on the northern coast of the same country, between the tenants of one of my own glebes and the neighbouring peasants, is ascertained by a heap of iron scoræ in the midst of loose and shifting sands. Thirty or forty years ago, there stood here the forge of the village; but no remains of it, or its smith, are now discoverable, except the cinders of the forge, and the rank weeds that spring from a rich stratum of earth, once the soil of his garden, and still visible in a hillock of sand,

II. *Analysis*

## II.

*Analysis of the Red Lead of Siberia; with Experiments on the new Metal it contains. By Citizen VAUQUELIN, Inspector of Ores\*, and Conservator of Chemical Products at the Mineralogical School †.*

In arctum coacta rerum naturæ majestas. PLIN.

SECTION I.—*Historical Facts.*

THE fossil known by the name of red lead was discovered in 1770, by M. Pallas, in the gold-mine of Berefok, near Ecatherineburg in Siberia, in the form of four-sided prisms, with or without pyramidal terminations, of a beautiful orange red, commonly fixed in a quartzose matrix, to which they so strongly adhere as not to be detached without difficulty.

All the specimens of this substance which are to be found in the several mineralogical cabinets in Europe were obtained from this gold-mine; which indicates, that it was formerly abundant; but it is said, that for some years past it has become very scarce, and that at present it is bought for its weight in gold, especially if pure and regularly formed. The specimens which do not possess the regular figure, or are broken into fragments, are appropriated to painting, in which art this substance is of high value for its beautiful orange yellow colour, its unchangeableness in the air, and the facility with which it can be levigated with oil. The following are the expressions of M. Pallas, who speaks of this mineral, in his Travels in the year 1770, under the article of the Gold-mine of Pischminskoi, tom. ii. page 235:—"A very remarkable red ore of lead is likewise worked at this place, which has not yet been found in any other mine of the empire, nor elsewhere. This ore of lead is weighty, of various colours; sometimes of a cinnabar red, and semi-transparent. It is fixed in long or short crystals in the clefts of quartz, and also in the bed of the mine, which is a sand-stone. It very frequently possesses, and every where when the space permits, the same thickness and prismatic form, with four plane faces and two extremities irregularly truncated. It is likewise found in small irregular contorted pyramids attached to quartz, and resembling small rubies. By reducing it to powder it affords a beautiful guhr of a deep yellow colour, which may be used in miniature painting. In all the essays of this lead ore in the laboratories of Ecatherineburg a small portion of silver has been obtained. It produces more than half its weight (valeur) in lead. Mr. Lehman could not ascertain whether this bley-spath did or did not contain silver, because his experiments were made on too small a scale to render that metal perceptible. It is difficult at present to procure the necessary quantity for trials in great, because the miners do not often work in the place where this ore is found, for want of air. In the mixed gangues of quartz in which this rare and curious mineral is formed, there are found small crystals pointed at each end, and of the colour of sulphur. They resemble native sulphur, and are considered as such by the miners; but they do not burn in the fire, nor decrepitate before the flame, like the ore of lead. This may, perhaps, be a metallic spar; but it is difficult to procure the quantity ne-

\* Des mines.

† L'Ecole des Mines.—This Memoir is translated from the Journal des Mines, No. xxxiv. page 737.

cessary for examination. These small crystals are found on the sand-stone as well as on the quartz.

"I can give no other detail respecting these minerals, which will not speedily be exhausted in this country, unless the veins should extend themselves to a great depth."

## SECTION II.

### *Account of the Experiments formerly made on the Siberian Red Lead.*

THE beautiful red colour, transparency, and crystalline figure, of the Siberian red lead soon induced mineralogists and chemists to make enquiries into its nature. The place of its discovery, its specific gravity, and the lead ore which accompanies it, produced an immediate suspicion of the presence of that metal; but, as lead had never been found in possession of the characteristic properties of this Siberian ore, they thought, with justice, that it was mineralized by some other substance; and Lehman, who first subjected it to chemical analysis, asserted, in a Latin dissertation printed at Petersburg in 1766 (I suppose 1786), that the mineralisers were arsenic and sulphur.

In 1789 Citizen Maquart undertook a long course of experiments, in which I had the advantage to participate, as he has been pleased to mention in the introduction to his work entitled *Essais de Mineralogie du Nord*. The object of these experiments was to determine the nature of the mineraliser of red lead. We sought in vain for the presence of arsenic; but, by an error, arising from the state of chemical knowledge at that time, we considered the red lead ore as a combination of super-oxygenated lead, iron and alumine.

Since that time Bindheim affirmed, that he had found it to contain molybdic acid, iron, nickel, cobalt, and copper.

From the consideration of these results, so diametrically opposite to each other, and under the encouraging consideration of the immense progress of chemical science since the renovation of its language and the rectification of its theory, and venturing likewise to place some dependance on the slight experience I have acquired in the art of assaying since I had the advantage of belonging to the establishment of mines, I thought proper to submit this substance to a new examination. My labours have not been without their recompense; and I hope to prove, in the following paragraphs, that all which has hitherto been asserted with regard to the mineraliser of the Siberian red lead is entirely destitute of foundation; that it contains neither arsenic, as Lehman pretended; nor the molybdic acid, and the three or four metals, announced by Bindheim; nor iron nor clay, as Maquart and myself imagined; but a new metal, possessing properties entirely unlike those of any other metal.

## SECTION III.

### *The new Methods of Analysis applied to the Red Lead of Siberia.*

THE science of chemistry is at present founded upon principles so certain, that, when the operator has reason to form a presumption respecting the nature of a compound body, it is possible to discover, by mere induction, the means which are most likely to separate its elements. Thus, from the suspicion that an acid might exist in the composition of the red lead, I concluded that I might decompose it in the way of double affinity; and the result of my experiments confirms the indications of theory.

*The*

*The first Method.*

*Experiment 1.*—100 parts of this mineral reduced to a fine powder were mixed with 300 parts of saturated carbonate of potash; and this mixture, together with about 4000 parts of water, was boiled for an hour. I observed that, 1. as soon as the re-action of the principles began, a strong effervescence was produced, which lasted for a long time; 2. the orange colour of the lead became a brickdust red; 3. at a certain period the whole appeared to be in solution; 4. in proportion as the effervescence proceeded, a coarse powder of a dirty yellow colour was thrown down; 5. and lastly, the fluid assumed a very fine golden yellow colour.

As soon as the effervescence had entirely ceased, and the re-action appeared to have terminated, the fluid was filtered, and the metallic powder collected. After washing and drying, it was found to weigh only 78 parts; the potash had therefore taken up 22 parts.

*Experiment 2.*—Upon the 78 parts last mentioned I poured nitric acid diluted with 12 parts of water. A strong effervescence followed; the greatest part of the matter was dissolved; the fluid did not acquire any colour; and the undissolved residue consisted of a small quantity of lemon-coloured powder. I separated the fluid part from the residue by means of a syphon, washed the remaining powder several times, and added the waters to the former solvent. The residue, when dried, weighed only 14 parts; whence it follows, that the nitric acid had dissolved 64 parts.

*Experiment 3.*—I again mixed these 14 parts with 42 parts of carbonate of potash, and the requisite quantity of water. These being treated as before, afforded the same phenomena. After filtration of the liquid, it was added to the first solution. The residue, washed and dried, weighed 2 parts. It was red lead, and was neglected.

*Experiment 4.*—The two nitric solutions put together and evaporated afforded 92 parts of nitrate of lead, crystallised in octahedrons, perfectly white and transparent.

These 92 parts of nitrate of lead dissolved in water were precipitated by a solution of sulphate of soda. The product was 81 parts of sulphate of lead, which answer to 56,68 of metallic lead.

*Experiment 5.*—The alkaline solutions, which were of an orange-yellow colour, were put together. In the course of several days they deposited two parts of a yellow powder which did not contain lead. These solutions, evaporated till a pellicle was formed on the surface, afforded yellow crystals by cooling, among which was carbonate of potash not decomposed.

These crystals were dissolved in water, and the solution, together with the mother water, was mixed with the weak nitric acid till the carbonate of potash was saturated. The fluid had then a very deep orange-red colour. When mixed with a solution of muriate of tin recently prepared, it first assumed a brown colour, which afterwards became greenish. When mixed with a solution of the nitrate of lead, there was an immediate regeneration of the red lead. And, lastly, by spontaneous evaporation, it afforded crystals of a ruby red colour, mixed with crystals of nitrate of potash.

Ninety-eight parts of this mineral, decomposed as has been here related, having afforded 81 parts of sulphate of lead, 100 parts would have afforded 82,65, which are equivalent to 57,1 of metallic lead. Now, admitting, as is proved by experiment, that 100 parts of lead

absorb, in order to combine with acids, 12 parts of oxygen, the 57,1 of metallic lead must contain in the red lead 6,86 of this principle; which leaves the weight of the mineralising acid 36,4.

*Experiment 6.*—To verify by synthesis the proportion of principles found by analysis in the red lead, I dissolved 50 grains, or about 2,654 grammes, of metallic lead in nitric acid, which was divided into two equal parts. One of these was completely precipitated by the requisite quantity of the combination of the acid of red lead with potash; and I obtained 43 grains, or about 2,282 grammes, of red lead as fine as the native mineral.

The other portion of the nitrate of lead, precipitated by caustic potash, afforded 28 grains of the white oxide of lead. So that by this synthesis 100 parts of red lead would be composed of 65,12 of oxide of lead, and 34,88 of acid. It gives, therefore, a difference of 1,72 less of acid than was deduced by the analysis, in the mineralising acid of the red lead; a difference which shews as great a degree of accuracy as the chemical methods can afford.

#### SECTION IV.

##### *The second Process for decomposing the Red Lead Ore.*

ANOTHER method, of which the execution is no less easy than the foregoing, consists in pouring 100 parts of muriatic acid diluted with the same quantity of water upon 100 parts of the red lead ore in powder, and occasionally agitating the mixture.

In this case the muriatic acid combines with the oxide of lead, and forms an insoluble salt, which falls down, while the acid of the red lead remains dissolved in the water which was before mixed with the muriatic acid. The fluid assumes a colour perfectly resembling that of the red lead before it was pulverised. As soon as it is perceived that the muriatic acid exerts no further action on the red lead, and the precipitated muriate of lead still contains some red particles, the supernatant liquor is decanted, and a new quantity of muriatic acid, one third or one fourth of the original quantity, and diluted in the same manner, is poured on the residue.

The decomposition being complete, the second solution is decanted, and added to the first; the residue is washed with a small quantity of cold water, which is also added to the solutions before decanted off.

In this process the operator finds himself under the necessity of obtaining his acid of red lead mixed with a certain quantity of muriatic acid, or of leaving a portion of the red lead undecomposed.

In fact, if, according to the known proportions of the component parts, no more of the muriatic acid were to be added than is necessary for the saturation of the oxide, part of the red lead would escape decomposition; for it appears that the acid of the red lead, at the time of its separation, unites with a portion of the muriatic acid, which seems necessary for its more ready solution in the water.

Of these two inevitable inconveniences I have therefore preferred that which leaves a small portion of the muriatic acid in the acid of red lead, because it is easy to separate it.

For this purpose the acid of the red lead, contaminated with muriatic acid, is to be diluted with a small quantity of water, and left at rest in a cool place for several days, in order that the small portion of muriate of lead which it may contain should crystallize and fall down. The fluid is then to be filtered or drawn off by a syphon; and to this must be added,



added; a little at a time, the oxide of silver precipitated from its solution by lime water or by a caustic alkali, and well washed.

The oxide of silver unites by preference with the muriatic acid, and forms with it a white insoluble salt, which falls to the bottom of the fluid. But great care must be taken not to add too great a quantity of the oxide of silver, because it will unite likewise with the acid of the red lead, and form an insoluble combination, which will mix with the muriate of silver. As soon, therefore, as it is observed that the oxide of silver assumes a purple red colour, it is a sign that the muriatic acid is entirely saturated, and no more of the oxide must be added. It is better, however, to add too much than too little; for, in this case, the only danger is that of losing a small quantity of the acid of red lead; whereas, in the other, the inconvenience will be, that a portion of the muriatic acid will be left in combination with the metallic acid. When the acid of red lead, mixed with muriatic acid, is evaporated to dryness, a lilac-coloured powder is obtained, which becomes green by the contact of the air, and is a combination of the oxide of the new metal with the muriatic acid.

## SECTION V.

### *The Nature and Properties of the Acid of Red Lead.*

THE acid of red lead, prepared according to the foregoing instructions, is of an orange-red colour, with a sharp metallic taste. It is very soluble in water, and its solution evaporated by a gentle heat, or spontaneously by exposure to the air, crystallizes in small long prisms of a ruby-red colour.

*Experiment 1.*—Paper wetted with this acid, and exposed for several days to the sun's light, assumes a green colour, which does not change in obscurity.

*Experiment 2.*—A plate of iron, of tin, or most of the other metals, immersed in the solution of this acid, causes it to assume the same colour.

*Experiment 3.*—Ether or alcohol, boiled for a few instants with this substance, produces the same effect.

*Experiment 4.*—The muriatic acid, heated in a retort with this acid, whether solid or in solution, produces a lively effervescence. Much oxygenated muriatic acid is afforded, and the fluid assumes a beautiful deep green colour.

*Experiment 5.*—These phenomena, which also take place when the ore of red lead is dissolved in the muriatic acid by means of heat, having led me to presume that the acid of red lead, by virtue of the great quantity of oxygen it contains, and the slight adherence it contracts with that principle, might favour the solution of gold in the muriatic acid, I put some leaves of that metal in a mixture of these two acids; and, by a slight ebullition, I actually obtained a complete solution of the gold.

The colour of this solution was green; it tinged the skin purple; and the solution of tin recently prepared occasioned a very abundant precipitate of the same colour.

*Experiment 6.*—This acid, mixed with a solution of the hydro-sulphuret of pot-ash, is precipitated in greenish brown flocks.

*Experiment 7.*—The aqueous solution of the tanning principle precipitates it in flocks of a yellowish brown colour.

*Experiment 8.*—Heated by the blow-pipe upon charcoal, it boils, and leaves a green infusible substance.

*Experiment 9.*—When fused with the phosphoric glass, or with borax, it communicates to the vitreous globules a very fine emerald-green colour.

*Experiment 10.*—Lastly, this acid combines with alkalis and earths, from which it disengages the carbonic acid with effervescence, and forms, with those substances, salts more or less coloured, of which the properties are described in the following section.

From the preceding results it evidently follows, that the mineraliser of the red lead ore is a true acid; that the radical of this acid is a peculiar metallic substance; for no other metallic acid yet known exhibits properties similar to the present. In fact, What metallic acid possesses a ruby colour; communicates to all its combinations red or yellow colours, more or less deep, or yields to the muriatic acid part of its oxygen, converting it into the oxygenated muriatic acid, while itself passes to the state of a green oxide, soluble in the muriatic acid? Lastly, What metallic acid is there, which forms with mercury a combination of a cinnamon red; with silver, a carmine red compound; with lead, an orange-yellow mineral; with the hydro-sulphuret of potash, an olive-green, &c.? If I am not deceived, there is none such. Therefore, notwithstanding the repugnance which I have to admit new simple bodies, a repugnance grounded on the numberless modifications which nature may give to bodies already known, and render them apparently new, I am nevertheless forced, by the great number of new characters possessed by this and by no other substance, to regard it as a metal naturally acidified, which resembles no other we are acquainted with.

This opinion will be still more confirmed by the experiments in the following paragraphs.

## SECTION VI.

### *Combinations of the Acid of Red Lead with Alkalis, Earths, and Metallic Oxides.*

THE small quantity of native red lead which I have hitherto possessed, has not allowed me to prepare any large masses of the salts which this acid is capable of forming with the alkaline, earthy, and metallic substances; in order to examine their properties with all the precision necessary to render them perfectly known. I shall confine myself, therefore, in this place, to exhibit their principal characters, such as colour, solubility in water, taste, their habitudes in the fire, &c. reserving myself to return to this object when circumstances shall be more favourable. It is well known, likewise, that a great interval of time frequently takes place between the period when the discovery of an uncommon substance has been made, and that in which all its properties are perfectly known, and that researches of this kind are usually indebted to time and opportunity for their ultimate degree of perfection.

### *Combination of the Acid of Red Lead with Barytes.*

THE acid of red lead ore readily unites with barytes, with which it forms a salt of very sparing solubility; for, by pouring an aqueous solution of this acid into a solution of the earth, a precipitate is formed of a pale lemon-yellow colour. This salt, however, is not entirely insoluble; for the supernatant fluid still retains a slight yellow colour, though the two principles of the salt are mutually saturated. This pulverulent salt has no perceptible taste;

taste; it is decomposed by the mineral acids; it gives out oxygen gas by heat, and the residue is an earthy mass of a green colour.

*Combination of the Acid of Red Lead with Lime.*

LIME combines with the acid of red lead, and affords a salt which does not appear to be more soluble than the foregoing: for, by mixing lime-water with a solution of this acid, a deposition of an orange-yellow colour is made, which is less abundant than with barytes; a consequence which naturally follows, from the barytes being about twenty times more soluble in cold water than lime; in consequence of which the precipitate formed with this last earth must be twenty times less in quantity. A large quantity of salt, of the same colour as the former, is also obtained by evaporation of the fluid. The salt formed by the combination of lime and the acid of red lead does not appear to differ from that of barytes, except in its being less soluble, and possessing different affinities and proportions of component parts.

Its habitudes with acids, and with heat, are the same as those of the combination of barytes with the same acid.

(To be concluded in our next.)

III.

*Information respecting the Earth of the Beryl; in Continuation of the first Memoir on the same Subject. By Citizen VAUQUELIN\*.*

I HAVE announced in my Memoir on the Earth of the Beryl, that this fossil contains about 8 per cent. of that principle; but at the same time I observed, that I did not consider this proportion as being very accurate, because I presumed that a certain quantity had been dissolved by the potash employed to separate the alumine.

I also announced that I had begun some trials, to ascertain whether the alumine contained in the beryl was in fact contaminated by a mixture of this new earth. The result of these experiments, related in a few words, will form the subject of the present communication to the Institute, together with an account of certain properties of this substance, of which I have since extracted a larger quantity.

*Experiment 1.* I put together the alumine which had been obtained from three docimastic quintals of beryl, which had been analysed in the same number of separate operations. The quantity was 63 grammes. I dissolved them in sulphuric acid, and, after having (bréveté) the solution, I submitted it to evaporation to obtain the alum. The evaporation was continued until crystals were no longer afforded. The remaining mother water was very saccharine and thick.

*Experiment 2.* I mixed this mother water with a solution of carbonate of ammoniac more than sufficient to saturate the acid. The mixture was repeatedly shaken during twenty-four hours. I perceived that the bulk of the precipitate thrown down by the first portions of the

\* The first Memoir is inserted in the present volume of our Journal, page 358. This continuation is translated from the *Annales de Chimie*, xxvi. 170.

carbonate

carbonate had very perceptibly decreased. At the expiration of that time I filtered the fluid, to separate the undissolved part: this last, after washing and ignition, weighed 5 grammes, and had all the properties of alumine.

*Experiment 3.*—I exposed the ammoniacal solution to the action of heat in a capsule of porcelain: as soon as the temperature was sufficiently elevated to drive off the carbonate of ammoniac, a large quantity of a white, granulated, and very voluminous earth was deposited. I continued the evaporation until all the ammoniacal salt was dissipated. I then threw the residue upon a filter, and washed it with much water. This residue, after drying by a gentle heat, was perfectly white, pulverulent, and soluble in acids, with a strong effervescence. It weighed 42 grammes, which by a red heat were reduced to 25 grammes; whence it follows, that these 42 grammes contained 17 grammes of carbonic acid and water.

Consequently, the three quintals of beryl having afforded 24 grammes of the new earth which was not dissolved by potash, and the alumine which these three quintals afforded having also, as has been just observed, 25 grammes of the same substance, it is evident that each quintal of beryl contains 16,33. One hundred parts of beryl are therefore composed of

69 parts silex,  
16 earth of beryl,  
13 alumine,  
1 oxide of iron,  
0,5 lime.

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99,5

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*Experiment 4.*—I have observed in my first Memoir, that alumine dissolved in the nitric acid was precipitated by the earth of beryl. I was desirous of knowing whether the same phenomenon would likewise take place with alum. I therefore dissolved one hundred parts of this salt in about six hundred parts of hot water. The solution afforded crystals of alum. Into this solution I put a certain quantity of the earth of beryl, recently precipitated from a solution by ammoniac, and well washed. The mixture was boiled for an hour. I soon perceived that the earth of beryl was taken up; and in proportion as the excess of acid in the alum was saturated, there fell down a great quantity of earthy matter, in white flocks, in a very divided state. As soon as the decomposition appeared to me to be complete, I filtered the liquor, and collected the precipitated earth, which, when washed and dissolved in the sulphuric acid, afforded, with a sufficient quantity of sulphate of pot-ash, crystals of alum perfectly octahedral.

The fluid from which the alumine had been separated possessed a very saccharine taste; and, when subjected to evaporation, it did not afford alum. Hence we find, that the earth of beryl has a greater affinity than alumine with respect to the sulphuric acid, as well as the nitric.

*Experiment 5.*—Being desirous of ascertaining still more effectually the difference which exists between alumine and the earth of beryl; I dissolved ten parts of the former earth in the sulphuric acid; and after having added the necessary quantity of sulphate of pot-ash,

ash, I obtained, by several successive crystallizations, 90 parts of alum. I also dissolved 10 parts of the earth of beryl in the same acid, and added the same quantity of sulphate of pot-ash. I obtained only 50 parts of salt in small crystalline grains, of which I could not determine the figure. This salt is soluble in 7 or 8 parts of cold water, a quantity very insufficient to dissolve alum at the same temperature.

There cannot, therefore, remain any doubt concerning the particular nature of the earth contained in the beryl; which must henceforward be reckoned in the number of substances of this order, of which it will compose the eighth species.

It almost always happens in the sciences of observation, and even in the speculative sciences, that a body, a principle, or a property, formerly unknown, though it may often have been used, or even held in the hands, and referred to other simple species, may, when once discovered, be afterwards found in a great variety of situations, and be applied to many useful purposes.

Chemistry affords many recent examples of this truth. Klaproth had no sooner discovered the different substances with which he has enriched the science, but they were found in various other bodies; and if I may refer to my own processes, it will be seen, that after I had determined the characters of chrome, first found in the native red lead, I easily recognised it in the emerald and the ruby. The same has happened with regard to the earth of the beryl. I have likewise detected it in the emerald; in which, nevertheless, it was overlooked both by Klaproth and myself in our first analysis: so difficult it is to be aware of the presence of a new substance, particularly when it possesses some properties resembling those already known!

Though I have not yet determined with much accuracy the proportion in which this earth exists in the emerald, I think, nevertheless, that it is nearly the same as in the beryl; but I shall ascertain this point with more certainty in my second analysis. The emerald and the beryl are therefore two stones of the same nature, excepting the colouring part; and the sciences of crystallography and chemistry are here also perfectly consistent in their results.

*A Table of the general Properties of the Earth of the Beryl.*

1. It is white.
2. Insipid.
3. Insoluble in water.
4. Adhesive to the tongue.
5. Infusible.
6. Soluble in the fixed alkalis.
7. Insoluble in ammoniac.
8. Soluble in the carbonate of ammoniac.
9. Soluble in almost every one of the acids (except the carbonic and phosphoric acids), and forming salts of a saccharine taste.
10. Fusible with borax into a transparent glass.
11. Absorbs one-fourth of its weight of carbonic acid.
12. Decomposes the aluminous salts.
13. Is not precipitable by well-saturated hydro-sulphurets.

*A Table of the specific Characters of the Earth of Beryl.*

1. Its salts are saccharine, and slightly astringent.
2. It is very soluble in the sulphuric acid by excess.
3. It decomposes the aluminous salts.
4. It is soluble in the carbonate of ammoniac.
5. Is completely precipitated from its solutions by ammoniac.
6. Its affinity for the acids is intermediate between magnesia and alumine.

None of the known earths unite the six properties announced in this table.

I present to the Instituté a certain quantity of this earth, and shall produce at one of its future sittings a series of combinations formed with this earth, extracted from a considerable quantity of beryl given to me by Citizen Patrin, whose zeal for the advancement of the sciences is well known to every one of their cultivators.

## IV.

*Observations on Electricity, Light, and Caloric, chiefly directed to the Results of Dr. PEARSON'S Experiments on Electric Discharges through Water. By a Correspondent.*

TO MR. NICHOLSON.

S I R,

WHEN I received the 6th, 7th, and 8th Numbers of your Journal for 1797, in succession, I read Dr. Pearson's Experiments and Observations on the Gaz produced by passing Electric Discharges through Water \*, with attention.

His experiments appeared to me to be well devised and conducted, and his conclusions fair and satisfactory: but his explanations of the manner in which those gazes were produced from water, and then re-converted into water by the same agent, the electric fluid, were by no means satisfactory.

That I might not be premature in my decisions against Dr. Pearson's principles or reasonings, I diverted my attention from the subject till now; but, upon the re-perusal of his papers, I still think his principles lax and indeterminate, and his reasonings, in part, unphilosophical, and to me apparently indefensible. I take the liberty, therefore, of laying the few following animadversions and objections before you:

The electric fluid, common fire, and light, those universal and general agents of nature, so far from being understood, appear to me to be not only confounded, but also to be so imperfectly considered, as to be the cause of endless confusion in every department of philosophy.

Oxygen gaz is said to be formed of oxygen and caloric:—what idea then are we to form of caloric?—Is it a simple, homogeneous principle, or is it a compound?—The general idea, or at least the common expression, seems to imply the idea of its being simply the calorific principle, and consequently uncompounded.

Oxygen may be converted into oxygen gaz by the electric fluid, or by light, as well as by fire. The electric fluid, then, imparts caloric to oxygen, and so does light.—What

\* Philos. Journal I. 241. 299. 349.

ideas then are we to form of the electric fluid and of light?—Are they merely modifications of simple caloric, or are they compounds in which caloric forms a part?

If the electric fluid be a mode of existence of simple, homogeneous caloric, how are we to explain the fact \*, that certain electric atmospheres repel each other, which atmospheres will attract other electric atmospheres that are also repulsive to each other?

It is incontrovertibly evident that electric atmospheres are of two distinct kinds; equally extensive, equally powerful, and mutually attractive to each other; although each is invariably repulsive to every atmosphere of the same kind as itself; and it is equally certain that they only produce fire, or take that form when they rush together, and cease to be electric. Instead, then, of supposing the electric fluid to be caloric, or partly formed of caloric, it appears to me to be more philosophical to suppose that it consists of two principles † in states of separation, which form caloric when they quit their electric states, and combine.

Is light a simple fluid ‡, or a modification of caloric?—If so, what ingenuity can explain how one homogeneous fluid, or simple fire, can, by passing through a prism, be separated into such variety of parts so permanently dissimilar to each other? and how happens it that light never produces fire but by evidently changing its mode of existence, as it ceases to be light when it takes the state and properties of fire?—This consideration again seems to lead to the conclusion that light consists of distinct and dissimilar principles, which, combining together, lose their properties as light, and constitute a fluid of very different properties and character, which we call fire, or caloric.

Nay, even if we advert to caloric itself, is it an uncompounded principle, or can any simple principle possibly perform the various parts assigned to it?—According to the present system of chemistry, caloric, homogeneous, simple caloric, destroys combinations which itself had formed; it attaches itself to particles of matter, and forms itself into repulsive spheres around them: and yet certain spheres of caloric in this state of repulsion will rapidly attract other spheres of the same caloric in similar states of repulsion! In short, caloric is hot or cold, attractive or repulsive, visible or invisible, just as occasion may serve; and, Proteus-like, it takes all shapes and forms:—we dread to meet it in Jove's thunderbolt, and court its influence in the cooling breeze!

\* The facts are simply, that *bodies* attract and repel each other in certain states of electrization; but the existence of electric atmospheres, of one simple electric fluid, or of two, &c. are mere hypotheses which yet remain without proof. M. de Luc (*Idées sur la Meteorologie*) supposes electricity to consist of the matter of heat combined with another matter incapable of passing through glass, &c. and that the charge is produced by the condensation of this last with the transmission of heat, in the same manner as steam (water and heat) might be condensed on a pane of glass, and cause the evaporation of water, supposed to be placed on the opposite surface, by virtue of the transmitted heat. N.

† That light of every kind is emitted by the electric spark is easily seen with the prism. N.

‡ The great Leonard Euler, and others, have maintained that the sensations and effects of light are merely consequences of the vibrations of a rare and very elastic fluid. To this doctrine have been opposed the rectilinear motion of light, which does not flow into the lateral spaces, together with its reflection, refraction, and colours. Much complexity of vibration would indeed be required to account for these phenomena: but not more, perhaps, than necessarily results from the consideration of sound; its echo, which deviates little from the angle of reflection; and the harmony, melody, tone, &c. of its distinct contemporaneous and successive impressions. N.



Having thus, in a general way, pointed out the confusion which obscures and depreciates philosophy in consequence of employing words without ideas, and of admitting certain agents as simple principles which probably are compounds, without deigning to consider whether they are so or not, even though the most palpable absurdities attend the admission; permit me to make a few critical remarks upon the paper in question.

Dr. Pearson explains the production of the gazes from water, by supposing that the dense electric fire, at the moment of diffusion, interposes betwixt the constituent elements of water, and places them beyond the sphere of attraction for each other, when each ultimate particle of the oxygen and hydrogen unites with a determinate quantity of fire, and they form hydrogen gaz and oxygen gaz.

It appears, then, that electric fire forcibly destroys the chemical union between oxygen and hydrogen; and that then a portion of it assumes the state of caloric, attaches itself to the particles of oxygen and hydrogen, and counteracts their chemical union, by keeping them distant from each other in the state of gaz.

Dr. Pearson then proceeds to explain in what manner these two gazes are made to recombine, and form water, by means of the electric fire or caloric. He first points out the manner in which he supposes the caloric acts in these words:—"I conceive its agency to be merely diminishing or destroying the powers which counteract chemical union;" and then proceeds to apply his theory to the explanation of the production of water from hydrogen gaz, and oxygen gaz, by the agency of caloric, in these words:—"Accordingly, when an electric spark, or the smallest particle of flame, or of an ignited substance, is applied to the gaz produced in the above process, or to the mixture of hydrogen and oxygen gaz, the ultimate particles of these gazes nearest to the flame are driven from it in all directions, as from a centre, by the interposition of fire, or of caloric and light; so that they are brought within the sphere of their chemical attraction for the ultimate particles of the gases at a certain distance from the centre of application of fire, which therefore unites," &c. Now, I must take the liberty to say, that I think this no explanation whatever. The conclusion appears to me to be drawn from premises which neither warrant such conclusion, nor make it either probable or conceivable.

According to Dr. Pearson's own theory and explanation, caloric counteracts the chemical union betwixt oxygen and hydrogen, and holds them in the state of gas; and, according to his own principles, he ought to have explained in what manner the caloric of an ignited body diminishes, or destroys, the spheres of caloric which counteract their chemical union; instead of which he only tells us, that caloric drives the ultimate particles of those gazes, nearest to it, in all directions, which, therefore, combine with other particles at a distance; but gives no idea whatsoever of the manner in which that flame, or that caloric, flowing off from an ignited body, diminishes or destroys the caloric which is already attached to the particles of oxygen and hydrogen, and counteracts their chemical affinity or union. In short, if caloric actually is attached to the disunited particles of oxygen and hydrogen, and prevents their chemical union, nothing that Dr. Pearson has said tends, in the least, to show how the accession of still more caloric can either diminish, or destroy, the caloric already attached to them, in sufficient quantity to prevent their union.

Much might be said upon a subject like this; but, as I wish not to take up too much of your time, I shall not proceed further with my remarks at present. What I have already  
advanced



advanced is, I think, sufficient to shew that Dr. Pearson's explanation of the subject in question is neither philosophical nor admissible; and, for the credit of the doctrine which he espouses, some more satisfactory explanation should be given. With respect to Dr. Parr's theory, that light and fire repel each other, when disengaged from matter, it appears to me too fanciful to be solid, and too inconsistent to be supported. If light and fire repel each other when disengaged from matter, by its decomposition, it is not an easy matter to shew how they, when in states of freedom and consequent repulsion, were brought together on the same substance at its formation: and, if oxygen and light repel each other, as he contends, why does light combine at once with the oxygen of nitric acid, and form oxygen gas, by merely placing the acid in its way? But this is not the hypothesis I have undertaken to combat.

I offer this letter for your perusal, and confess that I wish to see the subject impartially attended to. It is painful and humiliating to see how readily the most glaring absurdities are overlooked, nay, even adopted as principles, and employed as indisputable facts; and no further proof is necessary to point out the confusion and absurdity to which such erroneous principles tend, than the result of Count Rumford's experiments and reasonings on the production of heat by friction\*; which is, that as heat thus produced by friction cannot be accounted for upon the adopted principles of chemistry, he seems inclinable to conclude, that it cannot be a material substance, and is most probably nothing but motion!!! Surely this is sacrificing too much to hypothesis, to give up the conviction of reason, and the testimony of the senses, rather than suppose it possible that a favourite theory may be false. The production of heat by friction, is, no doubt, inexplicable upon the principles of M. Lavoisier, which ought to excite the suspicion of those who have embraced them; for, if so simple a fact cannot be explained by those principles, it is, at least, probable, that they are not deserving of that confidence which is so generally and implicitly placed in them. The production of heat, *ad libitum*, by friction or percussion between solid bodies, does, however, admit of an easy explanation, without being driven to the necessity of making caloric a non-entity, or heat a particular kind of motion†, propagated in such a particular

\* Philos. Journal, II. 106.

† The prodigious quantity of heat which follows from an apparently minute action, has always been considered as a strong argument against the hypothesis, that heat is mere motion; but, upon close examination, it is found to apply to both theories. If a very minute portion of a large mass of oxygen gas and carbon be *put into a state of vibration* (or heated), and the combination of this first portion be succeeded by vibrations and combinations of the remaining parts of the mass, we shall be led to investigate the attractive and repulsive powers concerned in the phenomena, and shall in all probability find the task by no means easy. But will our difficulties be alleviated or removed by supposing the presence of a third substance (caloric) in previous combination with the oxygen, which will certainly demand an exhibition of similar powers to account for its transference? Whatever may be the supposed play of the affinities or powers *with caloric*, it seems probable that the mere theory might be constructed as well (I do not say better) *without it*.

It has been said that latent motion (or heat) is an absurdity, as in terms indeed it is; but this might be explained in a variety of ways. Caloric is absorbed, or motion is accumulated, in certain processes. Slight incidents, not commensurate to the effect, set them free. He who sets the catch of the pile-engine at liberty, or discharges a cross-bow, or breaks the rail of Prince Rupert's drop, or communicates a spark to a powder-magazine, may be said to give effect to latent motion, or rather to destroy the equilibrium of forces generated by former efforts, far superior to that employed to set them at liberty.

particular kind of manner, that the motion of a single spark of fire, directed into a powder-magazine, will not only give motion to the building itself, and its contents, but will also shake a county!

Should this letter be thought unworthy of attention, or unfit for the public eye, I could wish, however, that you would oblige me so far, if convenient, as to transmit it to Dr. Pearson. I have the highest opinion of his chemical knowledge, as well as of the liberality of his principles; and though I have plainly pointed out some parts of his writings which I think erroneous, I still hold him no less in estimation. My only motive in making the objections and remarks which I have, is, if possible, to arrive at truth.

I am, with grateful esteem for the valuable information which I have repeatedly received from your labours in the fields of science, Sir,

Your most obedient servant,

Nov. 16, 1798.

*A constant Reader of your valuable Publications.*

Though I have stated a few facts and observations, I by no means wish to be thought a maintainer of any of the theories alluded to in the text or the notes.—None of them appear to me to be established upon indubitable facts.

## V.

*An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, Count of Rumford, F. R. S. M. R. I. A.\**

IN the second part of my seventh essay, (on the propagation of heat in fluids,) I have mentioned the reasons which had induced me to doubt of the existence of those chemical properties in light that have been attributed to it, and to conclude, that all those visible changes produced in bodies by exposure to the action of the sun's rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the heat which is generated, or excited, by the light that is absorbed by them.

As the decision of this question is a matter of great importance to the advancement of science, and particularly to chemistry, and as the subject is in many respects curious and interesting, it has often employed my thoughts in my leisure hours; and I have spent much time in endeavouring to contrive experiments, from the unequivocal results of which the truth might be made to appear. Though I have not been so successful in these investigations as I could wish, yet I cannot help flattering myself, that an account of the results of some of my late experiments will be thought sufficiently interesting to merit the attention of the Royal Society.

Having found that gold, or silver, might be melted by the heat (invisible to the sight) which exists in the air, at the distance of more than an inch above the point of the flame of a wax-candle, (see my seventh essay, part II. page 350.†) I was curious to know what effect this heat would produce on the oxides of those metals.

*Experiment No. I.* Having evaporated to dryness a solution of fine gold in aqua regia, I dissolved the residuum, in just as much distilled water as was necessary in order that the so-

\* Philof. Transf. 1798, p. 449.

† Or Philof. Journal, II. 165.

lution (which was of a beautiful yellow colour) might not be disposed to crystallize; and, wetting the middle of a piece of white taffeta riband,  $1\frac{1}{2}$  inch wide, and about eight inches long, in this solution, I held the riband, with both my hands, stretched horizontally over the clear bright flame of a wax candle; the under side of the riband being kept at the distance of about  $1\frac{1}{2}$  inch above the point of the flame. The result of this experiment was very striking. That part of the riband which was directly over the point of the flame, began almost immediately to emit steam in dense clouds; and, in about 10 seconds, a circular spot, about  $\frac{3}{4}$  of an inch in diameter, having become nearly dry, a spot of a very fine purple colour, approaching to crimson, suddenly made its appearance in the middle of it, and, spreading rapidly on all sides, became, in one or two seconds more, nearly an inch in diameter.

By moving the riband, so as to bring, in their turns, all the parts of it which had been wetted with the solution to be exposed to the action of the current of hot vapour that arose from the burning candle, all those parts which had been so wetted, were tinged with the same beautiful purple colour.

This colour, which was uncommonly brilliant, passed quite through the riband, and I found the stain to be perfectly indelible. I endeavoured to wash it out; but nothing I applied to it, and among other things I tried super-oxygenated marine acid, appeared in the smallest degree to diminish its lustre. The hue was not uniform, but varied from a light crimson to a very deep purple, approaching to a reddish brown.

I searched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the most beautiful purple enamel, which, in the sun, had a degree of brilliancy that was sometimes quite dazzling.

*Experiment No. 2.* A piece of the riband which had been wetted with the aqueous solution of the oxide, was carefully dried in a dark closet, and was then exposed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the solution (and which on drying had acquired a faint yellow colour) was tinged of the same bright purple colour as was produced in the last-mentioned experiment, when the riband was exposed wet to the action of the heat\*.

*Experiment No. 3.* A piece of the riband which had been wetted with the solution, and dried in the dark, was now wetted with distilled water, and exposed wet to the action of the ascending current of hot vapour which arose from the burning candle: the purple stain was produced as before, which extended as far as the riband had been wetted with the solution, but no farther.

I afterwards varied this experiment in several ways, sometimes using paper, sometimes fine linen, and sometimes fine cotton cloths, instead of the silk riband; but nearly the same tinge was produced, whatever the substance was that was made to imbibe the aqueous solution of the metallic oxide.

Similar experiments, and with similar results, were likewise made with pieces of riband,

\* We shall hereafter find reason to conclude, that the success of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which existed in the hot current that ascended from the flame of the candle. R.

fine linen, cotton, paper, &c. wetted in an aqueous solution of nitrate of silver; with this difference, however, that the tinge produced by this metallic oxide, instead of being of a deep purple, inclining to crimson, was of a very dark orange colour, or rather of a yellowish brown.

In order to discover whether the purple tinge, in the experiments with the oxide of gold, was occasioned by the *heat* communicated by the ascending current of hot vapour, or by the *light* of the candle, I made the following experiment, the result of which I conceive to have been decisive.

*Experiment No. 4.* A piece of riband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame.

The riband was dried, but its colour was not in the smallest degree changed.

When it was held a few seconds within about  $\frac{1}{8}$  of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced.

The heat which existed at that distance from the flame, *on the side of it*, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flattened, such as is used in making silver lace.

The objects I had in view in the following experiments are too evident to require any particular explanation.

*Experiment No. 5.* Two like pieces of riband were wetted at the same time in the solution, and suspended, while wet, in two thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed naked to the direct rays of a bright sun; while B was inclosed in a cylinder of pasteboard, painted black within and without, and closed with a fit cover, and consequently remained in perfect darkness.

In a very few minutes, the riband in the phial A began sensibly to change its colour, and to take a purple hue; and, at the end of five hours, it had acquired a deep crimson tint throughout.

The phial B was exposed in the window, in its dark cylindrical cover, three days; but there was not the smallest appearance of any change of colour in the silk.

*Experiment No. 6.* Two small parcels of *magnesia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a shilling,) were placed in heaps, in two china plates, A and B, and thoroughly moistened with the before-mentioned aqueous solution of the oxide of gold. Both plates were placed in the same window; the moistened earth in the plate A being exposed naked to the sun's rays; while that in the plate B was exactly covered with a tea-cup, turned upside down, which excluded all light.

The *magnesia alba* in the plate A, which was exposed to the strong light of the sun, began almost immediately to change colour, taking a faint violet hue, which by degrees became more and more intense, and in a few hours ended in a deep purple; while that in the plate B, which was kept in the dark, retained the yellowish cast it had acquired from the solution, without the smallest appearance of change.

*Experiment No. 7.* A small parcel of *magnesia alba*, placed on a china plate, having been  
moistened

moistened with the aqueous solution of the oxide of gold, and thoroughly dried in a dark closet, was now exposed, *in this dry state*, to the action of the direct rays of a very bright sun.

It had been exposed to this strong light above half an hour, before its colour began to be *sensibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with distilled water, it changed colour very rapidly, and soon came to be of a deep purple tint, approaching to crimson.

*Experiment No. 8.* A piece of white taffeta riband, which had been wetted with the solution, and thoroughly dried in the dark, was suspended in a clean dry phial of very fine transparent glass; and the phial, being well stopped with a dry cork, was exposed to the strong light of a bright sun.

After the riband had been exposed, in this manner, to the action of the sun's direct rays about half an hour, there were here and there some faint appearances of a change of its colour; but it showed no disposition to take that deep purple hue which the riband had always acquired, when exposed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with distilled water, and exposing it again, *while thus wetted*, to the sun's rays, it almost instantly began to change colour, and soon became of a deep purple tint; but, though I examined the surface of the riband with the utmost care, and with a good lens, both during the experiment and after it, I could not perceive the smallest particle of *revived gold*, nor did I see any vestige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated several times, and always with results which led me to conclude, (what indeed was reasonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a state of crystallization*.

The heat which is generated by the absorption of the rays of light must necessarily, *at the moment of its generation* at least, exist in almost infinitely small spaces; and consequently, it is only in bodies that are *inconceivably small* that it can produce durable effects, in any degree indicative of its extreme intensity.

Perhaps the particles of the oxide of gold dissolved in water are of such dimensions; and it is very remarkable, that the colours produced, in some of my experiments on white ribands, by means of an aqueous solution of the oxide of gold, are precisely the same as are produced from the oxide of that metal, by enamellers, in the intense heat of their furnaces.

As the colouring substance is the same, and as the colours produced are the same, why should we not conclude that the effects are produced in both these cases by the same means, that is to say, by the agency of heat? or, in other words, and to be more explicit, by exposing the oxide in a certain temperature, at which it becomes disposed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the results of the following experiments afford still more satisfactory information, respecting the intensity of the heat generated in all cases where light is absorbed, and the striking effects which, under certain circumstances, it is capable of producing.

The facility with which most of the metallic oxides are reduced, in the dry way, by  
means

means of charcoal, shows that, at a certain (high) temperature, oxygen is disposed to quit those metals, in order to form a chemical union with the charcoal, or at least with some one of its constituent principles, if it be a compound substance; and hence I concluded, that gold might be revived, *in the moist way*, by means of charcoal, from a solution of its oxide in water, were it possible, under such circumstances, to communicate to the charcoal, and to the oxide, *at the same time*, a degree of heat sufficient for that purpose.

To see if this might not be done by means of light, I made, or rather repeated, the following very interesting experiment:

*Experiment No. 9.* Into a thin tube of very fine colourless glass, 10 inches long, and  $\frac{5}{16}$  of an inch in diameter, closed hermetically at its lower end, I put as many pieces of charcoal, about the size of large peas, as filled the tube to the height of two inches; and, having poured on them as much of the aqueous solution of nitro-muriate of gold as nearly covered them, exposed the tube, with its contents, to the action of the direct rays of a very bright sun.

In less than half an hour, small specks of revived gold, in all its metallic splendour, began to make their appearance here and there on the surface of the charcoal; and, in six hours, the solution, which at first was of a bright yellow colour, became perfectly colourless, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The surface of the charcoal was, in several places, nearly covered with small particles of revived gold; and the inside of the glass tube, in that part where it was in contact with the upper surface of the contained liquid, was most beautifully gilded.

This gilding of the tube was very splendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greenish blue colour, without the smallest appearance of any metallic splendour.

From the colour, and apparent density of this cloud, I was induced to conclude, that the gilding on the glass was less than *one millionth part of an inch* in thickness.

This interesting experiment was repeated six times, and always with nearly the same result. The gold was completely revived in each of them, and the solution left perfectly colourless: in most of the experiments, however, the sides of the glass were not gilded, all the revived gold remaining attached to the surface of the charcoal.

In two of these experiments, I made use of pieces of charcoal which had been previously boiled several hours in a large quantity of distilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the solution, to prevent them from imbibing any air; and, in different experiments, the solution was used of different degrees of strength.

I plainly perceived that the experiment succeeded best, that is to say, that the gold was *soonest revived*, in those cases in which the solution was *most diluted*: one of the experiments, however, and which succeeded perfectly, was made with the solution so much condensed, that it was nearly at the point at which it became disposed to crystallize\*.

\* This agrees perfectly with the results of similar experiments made by the ingenious and lively Mrs. FULHAME. (See her Essay on Combustion, page 124.)

It was on reading her book that I was induced to engage in these investigations; and it was by her experiments that most of the foregoing experiments were suggested. R.

On examining, with a good microscope, the particles of revived gold which remained attached to the surface of the charcoal, after it had been dried, I found them to consist of an infinite number of small scales, separated from each other; not very highly polished, but possessing the true metallic splendour, and a very deep and rich gold colour.

The gold which attached itself to the inside of the glass tube, was in the form of a ring, about  $\frac{1}{10}$  of an inch wide, (badly defined however below,) and adhered to the glass with so much obstinacy, as not to be removed by rinsing out the tube a great number of times with water; it had, as has already been observed, a very high polish, when seen by reflected light.

Those who enter into the spirit of these investigations, will easily imagine how impatient I must have been, after seeing the results of these experiments, to find out whether gold could be revived from this aqueous solution of its oxide by means of charcoal, *without the assistance of light*, and merely by such a degree of equal heat as could be given to it in the dark. To determine that important question, the following experiment was made.

*Experiment No. 10.*—A cylindrical glass tube,  $\frac{6}{10}$  of an inch in diameter, and 10 inches long, closed hermetically at its lower end, and containing a quantity of a diluted aqueous solution of the oxide of gold, mixed with charcoal in broken pieces, about the size of large peas, was put into a fit cylindrical tin case, which was nicely closed with a fit cover; and the glass tube, with its contents, so shut up in the dark, was exposed two hours, in the temperature of  $210^{\circ}$  of Fahrenheit's scale.

On taking the glass tube out of its tin case, I found the solution *perfectly colourless*, and the revived gold adhering to the surface of the charcoal.

On repeating the experiment, and using the solution nearly saturated with the oxide, the result was precisely the same; the solution being found *perfectly colourless*, and the revived gold adhering to the surface of the charcoal.

I own fairly, that the results of these experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothesis, respecting the causes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypothesis of mine, I hope and trust that I never shall be so weak as to feel pain at the discovery of truth, however contrary it may be to my expectations; and still less, to feel a secret wish to suppress experiments, merely because their results militate against my speculative opinions.

It is proper I should observe, that the charcoal used in this last-mentioned experiment had been boiled two hours in distilled water; by which means its pores had been so completely filled with that fluid, that the pieces of it that were used were specifically heavier than water, and sunk in it to the bottom of the containing vessel.

(To be continued.)

## VI.

*Experiments and Observations on the Nature of Sugar, and of Vegetable Mucilage. By Mr. WILLIAM CRUICKSHANK, Chemist to the Ordnance, &c.\**

FROM the failure of these trials to convert mucilages into something resembling sugar, we began to suspect that they were not so simple as had been generally supposed. With a view to throw some light on this subject, the following comparative experiments were made:

One ounce of powdered gum arabic was introduced into a coated glass retort, to which a receiver with the pneumato-chemical apparatus was adapted: heat being gradually applied, there came over into the receiver 3 dr. 30 gr. of pyromucous acid, mixed with a little heavy empyreumatic oil. After the retort had been red-hot for some time, it was removed, and the charry residuum which it contained was found to weigh 1 dr. 46 gr. This had a greyish colour, and burned very slowly; but when exposed to a strong heat in an open crucible, it left a whitish powder, amounting to 10 grains, which was found to be lime mixed with a very small proportion of calcareous phosphate.

The pyromucous acid being super-saturated with lime, a strong smell of ammonia was instantly perceived; and a piece of paper dipped in muriatic acid being held over the vessel, copious white fumes were immediately produced. This circumstance shews, that azote forms a constituent part of the gum.

There were collected in the pneumato-chemical apparatus 273 oz. measures of gas; of this 93 were carbonic acid, and the remaining 180, that species of hydro-carbonate which is obtained by heat from moistened charcoal.

Two measures of this gas, well freed from carbonic acid, were mixed with  $1\frac{1}{2}$  of very pure oxygen gas, and introduced in a strong glass jar, filled with and inverted over mercury: when fired by the electric shock, they occupied the space of one measure only: lime water being admitted, the whole was absorbed, except a very small particle, which was found, from the nitrous test, to be pure air. From a number of experiments we have found, that twelve measures of oxygen gas, when united with carbon, produce ten of carbonic acid gas. Hence it follows, that the quantity of oxygen gas necessary to the formation of carbonic acid gas must, in this case, have been 1.1 measures, or a little better; the remaining 4 must therefore have been consumed in the production of water, and would be sufficient to saturate .8 of hydrogen, equal in weight to .048 of a grain nearly.

\* These Experiments, &c. are a continuation of the Research communicated in the first volume of this Journal, page 337. In the second edition of Dr. Rollo's Treatise on Diabetes Mellitus in which that article is reprinted, and whence the above is taken, there are a few emendations in the second and third paragraphs, as follow: The sugar afforded  $8\frac{1}{2}$  drams of acid, which required 150 grains of the alkaline solution to saturate it; the charry residue was 5 drams; and the gas which escaped  $2\frac{1}{2}$  drams. The gum arabic afforded 7 dr. 40 gr. of acid, which required 118 grains of the solution of pot-ash to saturate it; the charry residue was 3 dr. 45 gr.; and the gas which escaped 5 drams. Whence the sugar yielded more pyromucous acid than the gum, in the proportion of 150 to 118.

The concluding paragraph on page 341, beginning, "*Indeed, when we reflect,*" &c. is cancelled, and the investigation is continued as in the text. The present article must therefore be read as a continuation from vol. i. p. 341, of this Journal. N.

Now,



Now, an ounce measure of carbonic acid gas weighs .864 of a grain, and this contains .24 of pure charcoal: hence the quantity of charcoal to hydrogen in this inflammable gas must be as .24 to .048, or 5 to 1.

But one measure of pure hydro-carbonate, such as may be obtained from the decomposition of camphor, by making its vapour pass through a red-hot earthen tube, or from the distillation of animal substances, opium, &c. requires two of pure air to saturate it; and the quantity of carbonic acid amounts to 1.45, which makes the proportion of carbon to hydrogen as 12 or 13 to 1. Hence these gases differ materially, and ought not to be confounded: indeed, the difference between them is manifest, from the manner in which they burn when mixed with common air, and brought into contact with an ignited body. Pure hydro-carbonate burns slowly, with a perfectly white flame, and never detonates: on the contrary, the compound inflammable gas just-mentioned burns rapidly, with a reddish blue flame, and more or less of a detonation. We thought it necessary to mention this circumstance, as much confusion might be produced by applying the same name to substances considerably different. It is remarkable that æther, decomposed by heat, affords pure hydro-carbonate, whilst alcohol yields the mixed species.

But to return:—An ounce of gum tragacanth was submitted in a coated glass retort to a similar process, and the products collected were as follows:

	dr.	gr.
Charcoal remaining in the retort,	-	-
Pyromucous acid,	-	-
Carbonic acid gas,	-	-
Hydro-carbonate,	-	-

dr. gr.

1 45

4 5

78 oz. meas.

91 ditto.

The charcoal burned slowly with a peculiar phosphorescent flame, and left a white residuum of 12 grains, which was found to consist of lime mixed with a little calcareous phosphate.

The pyromucous acid being super-saturated with lime, the quantity of ammonia disengaged appeared to be considerably greater than from that afforded by the gum arabic.

Having in this way discovered lime in both species of gum, we were anxious to know if this earth could be detected by reagents, without having recourse to decomposition by heat. Accordingly, a quantity of sulphuric acid was dropped into a solution of gum arabic: after standing for some hours, a number of needle-like crystals were slowly deposited: these being separated were dissolved in distilled water; to this solution the oxalate of ammonia was added, when a copious precipitate of oxalate of lime immediately took place.

From these experiments, therefore, it is manifest, that gums consist of oxygen, hydrogen, carbon, azote, and lime, with a little phosphoric acid.

An ounce of refined sugar was next introduced into a coated retort, and the pneumatological apparatus applied, as in the former experiments. The products obtained were,

	oz.	dr.	gr.
Pyromucous acid, with a drop or two of empyreumatic oil,	0	4	30
Charcoal,	-	-	-
Hydro-carbonate,	-	-	-
Carbonic acid,	-	-	-

oz. dr. gr.

0 4 30

0 2 0

119 oz. meas.

41 ditto.

The pyromucous acid being supersaturated with lime, not the least vestige of ammonia could be perceived. Sugar, therefore, does not contain azote; neither does it contain lime;

for the charcoal, which was of a beautifully black colour, burned out completely when exposed to a strong red heat.

Being desirous of ascertaining more completely the difference between common sugar and the saccharine matter secreted by the breasts of animals, an ounce of the crystallized sugar of milk was distilled in an apparatus similar to that already described, and the products were found to be,

	oz.	dr.	gr.
Pyromucous acid, mixed with a very little empyreumatic oil,	0	6	0
Charcoal, with a little phosphate of lime,	0	1	0
Carbonic acid gas,	31	oz.	meas.
Hydro-carbonate, of the same nature with that obtained in the former experiments,	103	ditto.	

The charcoal being burned in an open crucible, there remained about one grain, which appeared to be phosphate of lime chiefly.

The pyromucous acid was next supersaturated with lime; but the quantity of ammonia disengaged was so small that it could with difficulty be detected: there appeared, however, to be a very little.

Hence, then, it would seem that this animal sugar, contrary to what might be expected, contains hardly any azote.

It would appear also, that it contains less charcoal, and more oxygen, than common sugar.

In order to investigate still further the nature of these substances, we endeavoured to ascertain the quantity of oxalic basis or radical contained in each, or how much oxalic acid they would afford when treated with an equal proportion of the nitrous acid.

An ounce of sugar was added to six ounces of the concentrated nitrous acid, diluted with an equal bulk of water. When the action had in a great measure ceased, heat was applied, and the evaporation continued until the liquor was reduced to about an ounce by measure: after this had cooled, the crystals were separated by filtration, and the remaining fluid again evaporated, until the whole, when cold, shot into a mass of crystals, leaving only a few drops which refused to crystallize. These crystals being collected, and well dried on blotting paper, amounted to 4 dr. 20 gr. or a little better than half the weight of the sugar employed.

An ounce of gum arabic was next treated with an equal proportion of nitrous acid properly diluted: the quantity of crystals collected amounted to 3 dr. 36 gr.; but in this instance, the last crystals obtained were mixed with an insoluble white powder, which, being separated from the oxalic acid by the addition of distilled water, amounted to 6 gr. and was found to be oxalate of lime. The pure acid, therefore, did not exceed 3 dr. and a half. An equal quantity of gum tragacanth afforded, by the same process, 3 dr. 10 gr. of oxalic acid, and 10 gr. oxalate of lime.

An ounce of the sugar of milk was also treated in a similar manner, with six ounces of the concentrated nitrous acid properly diluted, and there were obtained in all 3 dr. 48 gr. These crystals, however, were mixed with a white powder, which, being but a little soluble in water, was readily separated: it amounted to 30 gr. and appeared to be the saccholactic acid of Scheele; hence the pure oxalic acid did not exceed 3 dr. 18 gr.

An ounce of honey yielded, by a similar process, 4 dr. 4 gr. of pure oxalic acid, but exhibited no signs of saccholarctic acid.

The following Table will shew, at one view, the different products resulting from destructive distillation, with their relative proportions, and likewise those obtained by the nitrous acid.

Substances employed, 1 oz. of each.	Products obtained by Heat.				Products obtained by Nitrous Acid.	
	Charcoal, &c.	Pyromucous Acid, &c.	Carbonic Acid Gas.	Hydrocarbon- ate.	Oxalic Acid.	Other Substances.
Sugar, - - -	oz. dr. gr. 0 2 0	oz. dr. gr. 0 4 30	oz. measures. 41	oz. measures. 119	oz. dr. gr. 0 4 20	None.
Honey, - -					0 4 4	None.
Sugar of Milk,	0 1 0	0 6 0	31	103	0 3 18	30 grains of saccholarctic acid.
Gum Arabic, -	0 1 36 and lime 10 0 1 46	0 3 30 with some ammonia.	93	180	0 3 30	Oxalate of lime, 6 grains.
Gum Tragacanth,	0 1 33 and lime 12 0 1 45	0 4 5 with some ammonia.	78	91	0 3 10	Oxalate of lime, 10 grains.

From these experiments it would appear, that sugar consists merely of carbon, hydrogen, and oxygen; and that gum differs from sugar, not only in containing a less proportion of oxygen, but also by its combination with lime and azote; and that the sugar of milk differs from both, as it contains the radical of the saccholarctic acid: in other respects, however, it approaches very nearly to the nature of vegetable sugar. Does the milk of carnivorous and graminivorous animals yield the same proportion of this acid? and is this sugar itself always of the same nature?

From the well known facts respecting vinous fermentation, there is now reason to suppose, that no substances, but those which consist simply of carbon, hydrogen, and oxygen, are susceptible of it, and that an union with a fourth changes the nature of the compound so much as to render this process impracticable.

With a view to throw some light on this obscure subject, the following experiments were made:

August 4, 1798.—An ounce of sugar dissolved in 5 ounces of water was digested with a little fresh flaked quicklime, in a moderate heat, for about 15 minutes; the solution was then filtered, and about 2 drachms, by measure, of good yeast added; the vessel and mixture, weighing 17 oz. 2 dr. were introduced below a large glass bell.

There was placed close to it, as a standard, another vessel, containing an ounce of sugar dissolved in 5 ounces of warm water, and to which an equal quantity of yeast was added. This vessel and mixture, which weighed together 17 oz. 1 drachm, 20 grains, were likewise placed

placed under a glass bell. The temperature of the room during the whole of this experiment was rather high, and varied from  $68^{\circ}$  to  $75^{\circ}$  of Fahrenheit.

The last mixture began to ferment in 12 hours, and in 24 the process seemed very brisk, much water now condensing on the sides of the bell. In five days the process began to subside; in seven, the yeast fell to the bottom, and the liquor became clear. The mixture, at this time, smelled strongly of beer turned a little sour, although it still tasted sweetish. It was suffered to remain until the 28th, when it was removed from the bell and weighed, and the loss was found to exceed three drachms: it now tasted much like a mixture of strong vinegar and honey. During the whole of this period, amounting to 24 days, the mixture, containing the sugar digested with the lime, never shewed the least appearance of fermentation, nor was there any moisture condensed on the sides of the bell. The vessel and mixture being now weighed, the loss amounted to two drachms nearly; the liquor smelled very musty, and had a rough astringent taste, mixed with sweetness, but was not in the least acid. In this instance the yeast fell to the bottom very soon, the liquor continued more or less mouldy, and became, at last, a little mouldy at the top.

We next digested an ounce of sugar, dissolved in five ounces of water, with a little pot-ash, and to the filtered solution added about two drachms of very good yeast. This mixture was exposed in an open vessel to a temperature of about  $65^{\circ}$ ; and another vessel, containing a solution of an equal quantity of sugar, mixed with yeast, placed by it as a standard.

The solution with the yeast alone began to ferment in 12 hours; but the other, containing the pot-ash, shewed no symptoms of fermentation at the end of 12 days.

Being now in a great measure satisfied, that any fourth substance combined, although in small quantity, with the three which form sugar, would prevent fermentation, we next wished to know if every compound, consisting of carbon, hydrogen, and oxygen, however differently these substances might be proportioned, were susceptible of its process. The sugar of milk, from what has already been observed, evidently consists of these three simple substances; but, from a number of its sensible properties, and the result of its analysis by heat, as well as its containing the radical of the saccholaric acid, it is manifest that their combinations and proportions must be very different from those in common sugar.

We therefore mixed a solution of its saccharine matter with the usual quantity of yeast, and exposed it to a temperature ranging from  $65^{\circ}$  to  $70^{\circ}$ : in four days some degree of fermentation was perceptible, and in three days more, became brisk: at the end of sixteen, when its process had apparently ceased, the liquor was examined: it now had the smell of cyder, but rather more of the flavour of apples; to the taste it was very sour, and, when added to the tincture of litmus, strongly reddened it.

The acid thus procured either contained or consisted of common vinegar; for with the oxide of lead it formed a sweet saline mass, composed evidently of slender prismatic crystals which were not deliquescent.\*

Hence it is manifest, that the sugar of milk is at least in some degree susceptible of the vinous fermentation.

Having observed, in our attempts to convert gum into sugar, that it seemed to run easily into the acid state, we were anxious to know if any thing like fermentation preceded this

\* See Scheele's Essays, page 274.

state, or if it was possible to convert it into an acid, by mere exposure to the air, without the addition of some substance containing much oxygen, as the nitrous or oxygenated muriatic acids. Accordingly a solution of gum arabic, mixed with a proper proportion of good yeast, was introduced into an open vessel, and kept at a temperature ranging from  $68^{\circ}$  to  $75^{\circ}$  for twenty-six days, but during this period nothing like fermentation was perceived: the mixture at last emitted a very peculiar and offensive smell: the gum, however, still retained its natural taste, and was not in the least sour. In this case it should appear, that the azote, and lime, which in the gum are combined with carbon, hydrogen, and oxygen, prevented the vinous fermentation, and consequently the formation of any thing like vinegar.

It has been supposed, that a decoction of purely animal matter might undergo certain spontaneous changes, and at last become acid. In order to determine this point, about sixteen ounces of a strong decoction of beef were introduced into an open vessel, and kept at the temperature of about  $68^{\circ}$  or  $70^{\circ}$ . A few days after, an equal quantity of a similar decoction, mixed with an ounce of yeast, was likewise exposed in an open vessel to air of the same temperature.

At the end of five days, the decoction, without any mixture, began to shew evident marks of putrefaction, but did not in the least taste acid, nor had it ever shewn any signs of vinous fermentation; in two days more it became extremely offensive, accompanied with the production of ammonia.

The decoction with the yeast did not shew any evident signs of putrefaction until the seventh day, but there was nothing like fermentation perceived; in two days more it became extremely putrid and offensive, and was thrown away.

Hence it should appear, that neither vegetable nor animal mucilages are, when pure, in any degree susceptible of the vinous fermentation: indeed, these are facts which have been so generally admitted, that, had not a contrary opinion been lately advanced, we should have conceived the three last experiments unnecessary.

The mistake has no doubt arisen from observing the facility with which a decoction of a mixture of animal and vegetable matter runs into the acid state: but in this case the animal substance performs the part of yeast only; and it is in this way that diabetic urine so readily ferments spontaneously, and becomes vinegar.

From the preceding experiments we may draw the following conclusions:

1st. That sugar consists of carbon, hydrogen, and oxygen; and may be considered as a pure vegetable oxyde.

2d. That sugar of milk is composed of the same principles, but contains more oxygen and considerably less charcoal.

3d. That gum differs from sugar in containing, besides carbon, hydrogen, and oxygen, both lime and azote.

4th. That vegetable farina cannot be converted into saccharine matter, without the joint action of oxygen and water; the first of which appears to be absorbed, and the last decomposed, during this process.

5th. That when sugar is deprived of its oxygen, or combined with other substances, it loses its characteristic properties, and is no longer susceptible of the vinous fermentation.

6th. That neither vegetable nor animal mucilages, in their pure state, are susceptible of this process.

## VII.

*On the Art of covering Wire Cloth with a transparent Varnish, as a Substitute for Horn; and on other Objects of public Utility. By ALEXIS ROCHON, of the National Institute of France, &c.\**

IN the progress of the present war, the marine store-houses of France were totally without the essential article of horns for lanthorns. It was impossible to substitute glass in the place of this article, on account of its brittleness, and the obvious danger which might result from that quality. In this situation of distress, the agents of the French government consulted Citizen Rochon, and directed him to make every experiment he could think of to discover a proper substitute. His attention was first directed to a memoir of the celebrated Poivre on the fabrication of lanthorns of horn by the Chinese. It is known that this industrious nation prefer horn to glass on account of its cheapness and toughness, and that they possess the art of welding this substance together with so much delicacy, that they make lanthorns of two feet diameter of astonishing transparency, and to all appearance of one single piece. It is also known that the Chinese use the horns of goats and sheep only, which they soften and split into laminæ by processes supposed to be unknown in Europe; or, perhaps, by employing a proportion of human labour and patience for that purpose which the European demand might be inadequate to repay. Citizen Rochon, who does not appear to be perfectly aware of the degree of accuracy with which the same art of splitting horn is practised in Europe, proposed, that the horns of beeves should be sawed into laminæ, and then scraped and polished; or, to which he gives the preference, that they should be laminated in boiling water.

While this active philosopher was employed at Brest in establishing a manufactory for laminating the horns of beeves, which he purposed to reduce into the state of a paste by means of pure alkali in the digester of Papin, it occurred to him, that he might supply the pressing wants of the navy by another expedient, which consisted in the application of a coating of glue upon wire cloth.

In this process, he at first tinned the iron wires of the sieve cloth he made use of, but afterwards found it more convenient, in every respect, to give it a slight coating of oil paint to preserve it from rust. The glue he made use of was afforded by boiling the clippings of parchment with the air-bladders and membranes of sea-fish; materials which he used, not from any notion that they were preferable to isinglass, but because they were the cheapest he could procure. He added the juice of garlick and cyder to his composition, in proportions which, I suppose, he did not measure, but which he found to communicate great tenacity and somewhat more of transparency than it would have possessed without them. Into this transparent and very pure glue or size he plunged his wire cloth, which came out with its interstices filled with the compound. It is requisite that the size should possess a determinate heat and consistence, concerning which, experience alone must guide the operator.

\* Extracted from a memoir read to the National Institute of France the 21st Ventose, in the year VI. (March 11th, 1798), and inserted in the *Journal de Physique* for April 1798. The memoir contains various political and economical observations more particularly applicable to France, with general observations, which I have not thought it necessary either to transcribe or abridge; neither have I been solicitous to take the very words of my author in the parts I have abstracted. N.

When this prepared wire cloth is fixed in the lanthorn, it must be defended from moisture by a coating of pure drying linseed oil; but even in this state it is not fit to be exposed to the weather. The case with which these lanthorns are repaired in case of accident, by a slight coating of glue, is pointed out as a great advantage by the inventor, who likewise informs us that they were used in the expedition to Ireland as signal lanthorns, though contrary to his wishes. For this use he recommends the large plates of mica, which were then imported from Boston. With the latter substance, enclosed between two pieces of very open wire cloth, he made certain squares, 26 inches in length and 18 in width, for the light-house at Ushant, which had been damaged by a flock of wild ducks, that flew through the windows and dashed out the lights.

Citizen Rochon affirms, that lanthorns of wire cloth, prepared in his method, are much cheaper than those made of tin and horn; that they are very cheaply repaired, and afford a stronger light.

He applied coarse iron wire cloth to another use, which, he thinks, may be worth attention in future. He made the roof of one of his workshops of this wire-cloth in order to avoid the danger of fire, and covered it with a slight coating of plaister. He thinks that a composition of lime and pounded scales of iron would have been preferable. This coating ought not to be thicker than a slate; and he recommends that it should be penetrated with boiling whale oil, and painted with tar and ochre. Such a roof would afford no hold for the wind, and might, as he apprehends, be of considerable use to defend buildings and sheds which require particular defence against fire.

In the course of experiments made for the discovery of a varnish proper to defend his new lanthorns from moisture, Citizen Rochon did not employ resins or copal, which are always somewhat friable, but a perfect solution of elastic gum in drying linseed oil. This varnish or unguent having fixed the attention of Genouin, that learned chemist demanded whether it might not be practicable to use it in making bougies and other medical instruments, which were also at that time very much wanted. From this suggestion, Rochon employed the English machine for weaving whips to make the more consistent part of the instrument. He plunged this woven piece in a mixture of melted wax with a little ochre, then drew it through a wire plate to take off the superfluous wax, and render it perfectly smooth; after which he applied the varnish of elastic gum, which completed the instrument.

Sartori, ornamental painter at Brest, pointed out to our operator that fish glue is preferable to parchment size upon open wire cloth, because it is more transparent and stronger.

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#### VIII.

*On the Production of Nitric Acid by the Contact of Oxygen very much heated and the Air of the Atmosphere\*.*

**P**AUL, a celebrated artist at Geneva, having constructed and improved the apparatus of Watt for the production of the gases, was employed in obtaining by means of this apparatus the oxygen gas from the black oxide of manganese, taking care not to close the apparatus till the manganese was red-hot, in order to permit the escape of the water and carbonic acid

\* Communicated to Dr. Delametherie by J. L. Odier. *Journal de Physique*, iii. New Series, p. 464.

which this oxide always contains in its native state. While the gas was extricated in great abundance, one of the cocks of the tube through which the gas passed was accidentally opened, so that a portion of the gas, very hot and pure, was emitted into the atmosphere. At the same instant all the assistants were surprised by a manifest smell of nitric acid, and a slight fume was seen to rise from the place whence the gas had issued.

Professor Pictet, who was present, first took notice of the singularity and importance of this fact. It appears to prove, that when hot and very pure oxygen is brought into contact with the atmospheric air at the ordinary temperature, nitric acid is formed by the chemical combination of the two constituent principles of that acid; so that by causing pure and hot oxygen gas on the one hand, and atmospheric air on the other, to pass into a glass globe or any other close vessel, a great quantity of nitric acid gas would be obtained, which might be condensed and absorbed by water previously put into the vessel. Again, it is known that manganese has the property of absorbing the oxygen of the air, or of water, when deprived of its original portion by means of fire. It might therefore be practicable, by means of a determinate quantity of the black oxide of manganese, to obtain successively from the atmospheric air itself an unlimited quantity of nitric acid, which would besides have the advantage of being perfectly pure, if care was taken to purify the atmospheric air made use of in the operation\*.

## IX.

*Analysis of the Chrysolite of the Jewellers, proving it to be Phosphate of Lime.*

*By Citizen VAUQUELIN†.*

WHEN I frequently heard from the mouth of Fourcroy, in his lectures during the last ten years, and repeated in my memoir on the new metal contained in native red lead, that, if the art of the chemist could be exercised upon the objects of natural history preserved in collections, discoveries would often be made of much utility to the advancement of that science; I did not expect that I should have so speedy an opportunity of evincing the truth of that observation to the Institute.

Naturalists have hitherto considered the chrysolite as a precious stone of the second order, and all writers have arranged it at the end of the gems properly so called. Citizen Lametherie, in his *Manuel du Minéralogiste*, placed it immediately after the emerald and the aqua marine; Citizen Sage, in his *Chemical Analysis*, has ranged it with the saphir;

\* This fact appears to deserve the notice which the Author and the worthy Genevan Professor have bestowed upon it, and is certainly entitled to farther investigation. In order that the speculations at the end of the paper may be admitted or refuted, it appears necessary to determine, whether the elastic fluid which escaped was pure oxygen, or was contaminated with nitric acid. This might have been ascertained by examining the water over which the gas was collected. As the native oxide of manganese contains azote, which, as Fourcroy informs us, is driven over, for the most part, before the ignition; and as Milner and Cavendish have shewn that nitrous acid is formed by the combination of oxygen and azote at a red heat, there seems to be some ground for suspicion, that the nitrous acid in the case before us was formed by a remaining portion of azote in the manganese after the ignition took place. N.

† Read to the first class of the National Institute of France, in Brumaire, in the year VI. (Nov. 1797.) It is inserted in the *Annales de Chimie*, xxvi. 123. I have added the words in the title expressing the component parts of this stone. N.



and Wallerius has placed it between the emerald and the garnet. The account of this author is copied in the note below\*.

Kirwan uses the term chrysolite to denote merely the peridot, which nevertheless differs from it greatly in the nature of its principles.

Achard of Berlin analysed a species of chrysolite in which he affirms that he found flex 0.15; alumine 0.64; lime 0.17; iron 0.1.

But the results of this analysis are so different from mine, that I strongly suspect he must have operated on a different stone from the true chrysolite. This suspicion is so much the more probable, as the name of chrysolite has been given to many different stones, particularly the peridot, the chryso-beryl, the olivine, and in general all stones which have a greenish yellow colour.

Citizen Launoy, in a journey he is at present performing in Spain to collect the objects of natural history, found in the hands of a dealer a considerable quantity of chrysolites, which he sent to Paris; and the Council of Mines having purchased part of them, directed me to submit them to analysis.

It was not long before I discovered that this fossil, which has all the external appearances of a stone, is not truly of that class; but that, on the contrary, it is a salt composed of an acid and a base which are both well known; namely, the phosphoric acid and lime.

As soon as I had obtained this first result I enquired of Citizen Haüy, whether he had compared the masses of chrysolite with those of the apatite or crystallised phosphate of lime. He answered, that he had not, but that he had among his papers the relative results of the primitive forms of both, and would immediately compare them. He found with pleasure that they did not differ in the least appreciable quantity; and this conclusion may even be drawn from the results of which he has given an abstract in his treatise published in the Journal des Mines. Citizen Haüy, therefore, discovered by geometry what I confirmed by chemical analysis; and this satisfactory agreement, between two sciences apparently so remote from each other, affords a proof of the truth and certainty of their respective principles.

I shall now proceed to relate the experiments by means of which I ascertained the nature of the principles of the chrysolite, and determined their proportions.

*Experiment 1.* Two hundred parts of chrysolite in crystals being subjected to the action of a strong heat for one hour, lost their yellow colour without undergoing any change of form or transparency. They then resembled rock crystal, and had lost only one two-hundredth part of their weight.

*Experiment 2.* One hundred parts of pulverised chrysolite were mixed with the same quantity of concentrated sulphuric acid, and about four hundred parts of distilled water; the mixture immediately became hot, and assumed the consistence of thick soup. Nearly the same quantity of water as before was added, and the whole was boiled for several hours in a matras with a long neck. The mixture being then diluted with much water

\* "Colore hæc gemma gramineo viridi flavo, seu aurantiorum, omnibus gemmis imo crystallo montano mollior, chalybe rasilis, calcinata colorum transparentiam perdit, albescens, pondere et parum diminuitur; certis circumstantiis per se liquabilis in vitrum opacum album; in eo etiam à reliquis gemmis distincta, quòd in momento fusionis eodem modo phosphorescat ut terra aluminaris, vel spathum gypsofum. Cum borace instar smaragdi in fusionibus se habet, quam tamen gravitate specificâ superat in proportionem ad aquam ut 3.600, vel 3.700 : 1.000. Figura dicitur esse polygonâ seu quadrangularis; occurrit etiam filiciformis, rotundata, in Brasiliâ."

was filtered; and the solid matter being collected, washed, and ignited in a silver crucible, weighed 116 parts. This matter, subjected to various proofs, exhibited all the characters of sulphate of lime.

The filtered liquor was evaporated to dryness in order to expel the sulphuric acid. The residue was ductile, and might be drawn out into threads as long as it preserved its heat; but it hardened by cooling like a kind of glass slightly opaque; it weighed 46 parts. These forty-six parts, dissolved in water and mixed with carbonate of ammoniac, formed an abundant precipitate which became still more abundant by heat. This precipitate separated by the filter, washed, and ignited in a silver crucible, weighed 11 parts. It was phosphate of lime not decomposed.

The fluid thus cleared of the phosphate of lime was evaporated to the consistence of a syrup. At the expiration of several days it afforded crystals in the form of a four-sided prism terminating in quadrangular pyramids, whose sides corresponded with those of the prism, and of a penetrating urinous taste. Heat decomposed this salt, ammoniac was driven off, and the acid remained in the state of a perfectly transparent glass. The glass, mixed with the powder of charcoal and strongly heated in a retort, very speedily afforded phosphorus.

It cannot be doubted, therefore, but that this pretended stone is a combination of lime and phosphoric acid; a true native phosphate of lime in crystals. Nevertheless, though I am convinced by experience of the accuracy of the result I have announced, I was desirous of making some other experiments in order that there might not be the slightest doubt upon the subject.

*Experiment 3.* One hundred parts of the same substance in powder were digested in muriatic acid diluted with two or three parts of water. The solution took place speedily without the least effervescence; it was clear and colourless.

This solution, evaporated nearly to dryness in order to dispel the excess of muriatic acid, was again diluted with water, and mixed with a solution of oxalic acid, till the precipitate which immediately followed was no longer produced. The filtered liquor left upon the paper a precipitate, which after washing and drying weighed 118 parts. These 118 parts of precipitate, strongly calcined in a crucible, assumed at first a black colour arising from the coal of the oxalic acid decomposed by the fire. This coal being burned off, there remained 54.28 parts of a white pulverulent substance, which was acrid, soluble in water, turned vegetable blue colours to a green, and, in a word, presented all the characters of pure lime. The fluid from which the oxalate of lime had been separated was evaporated to dryness, and assumed a black colour on account of an excess of oxalic acid which the heat had decomposed.

When the whole of this last acid appeared to have been entirely converted into carbon, the residue was dissolved in water, and the solution filtered in order to separate the carbon. The fluid part, saturated with carbonate of ammoniac, let fall a few light flocks of phosphate of lime which weighed one part. This fluid, subjected to evaporation, afforded a salt perfectly resembling that of the former experiment. It was decomposed by lime water, and was fused with the blow-pipe, with inflation, and emitted a smell of ammoniac and a yellow greenish light. The residue was a transparent glass.

Since, therefore, this experiment perfectly agrees with the former, not only with regard to the nature of the principles of the chrysolite, but also the proportions, as will immediately

diately be shewn; it would be useless to add to the number of experiments, which would convey no other information than is already known concerning the phosphate of lime.

In order to establish the proportions of phosphoric acid and lime in the chrysolite, it must be recollected, 1. that 100 parts of this substance afforded in the second experiment 116 parts of calcined sulphate of lime, which, according to Bergman, contained 48,84 of pure lime: 2. that there remained 11 parts of phosphate of lime not decomposed, which were capable of forming 14,33 of sulphate of lime, which added to the 116 give 130,33. Now if 116 contain 48,84 of lime, it is evident that 130,33 must contain 53,32. According to this experiment, therefore, 100 parts of chrysolite contain 53,32 of lime; and subtracting this from the 100, there will remain for the phosphoric acid 46,68. In the third experiment it is also seen, that 100 parts of chrysolite, dissolved in the muriatic acid, afforded by the oxalic acid 118 parts of oxalate of lime; and that these 118 parts left after calcination 54,28 parts of pure lime, which subtracted from 100, give 45,72 for the phosphoric acid.

We see, therefore, that the results of these two experiments do not differ so much as one hundredth part from each other, and that they perfectly agree with the component parts obtained by Klaproth in his analysis of the apatite, from which he obtained 55 parts of lime and 45 of phosphoric acid.

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X.

*Account of a singular Instance of Atmospheric Refraction. In a Letter from WILLIAM LATHAM, Esq. F.R.S. and A.S. to the Rev. H. WHITFIELD, D.D. F.R.S. and A.S.\**

DEAR SIR,

Hastings, August 1, 1797.

ON Wednesday last, July 26, about five o'clock in the afternoon, whilst I was sitting in my dining-room at this place, which is situated upon the parade close to the sea-shore, nearly fronting the south, my attention was excited by a great number of people running down to the sea-side. Upon enquiring the reason, I was informed that the coast of France was plainly to be distinguished with the naked eye. I immediately went down to the shore, and was surprised to find that, even without the assistance of a telescope, I could very plainly see the cliffs on the opposite coast; which, at the nearest part, are between forty and fifty miles distant, and are not to be discerned, from that low situation, by the aid of the best glasses. They appeared to be only a few miles off, and seemed to extend for some leagues along the coast. I pursued my walk along the shore to the eastward, close to the water's edge, conversing with the sailors and fishermen upon the subject. They, at first, could not be persuaded of the reality of the appearance; but they soon became so thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it were, that they pointed out, and named to me, the different places they had been accustomed to visit; such as, the Bay, the Old Head or Man, the Windmill, &c. at Boulogne; St. Vallery, and other places on the coast of Picardy; which they afterwards confirmed, when they viewed them through their telescopes. Their observations were, that the places appeared as near as if they were sailing, at a small distance, into the harbours.

\* *Philos. Trans.* 1798, page 357.

Having indulged my curiosity upon the shore for near an hour, during which the cliffs appeared to be at some times more bright and near, at others more faint and at a greater distance, but never out of sight, I went upon the eastern cliff or hill, which is of a very considerable height, when a most beautiful scene presented itself to my view; for I could at once see Dengenefs, Dover cliffs, and the French coast, all along from Calais, Boulogne, &c. to St. Vallery; and, as some of the fishermen affirmed, as far to the westward even as Dieppe. By the telescope, the French fishing-boats were plainly to be seen at anchor; and the different colours of the land upon the heights, together with the buildings, were perfectly discernible. This curious phenomenon continued in the highest splendour till past eight o'clock (although a black cloud totally obscured the face of the sun for some time), when it gradually vanished.

Now, Sir, as I was assured, from every enquiry I could possibly make, that so remarkable an instance of atmospherical refraction had never been witnessed by the oldest inhabitant of Hastings, nor by any of the numerous visitors (it happening to be the day of the great annual fair, called Rock fair, which always attracts multitudes from the neighbouring places), I thought an account of it, however trifling, would be gratifying to you.

I should observe, the day was extremely hot, as you will perceive by the subjoined rough journal of a small thermometer, which was kept in the dining-room above mentioned. I had no barometer with me, but suppose the mercury must have been high, as that and the three preceding days were remarkably fine and clear. To the best of my recollection, it was high water at Hastings about two o'clock P.M. Not a breath of wind was stirring the whole of the day; but the small pennons at the mast-heads of the fishing-boats in the harbour were, in the morning, at all points of the compass.

I am, &c.

WILLIAM LATHAM.

P. S. I forgot to mention, that I was a few days afterwards at Winchelsea, and at several places along the coast; where I was informed the above phenomenon had been equally visible. I should also have observed, that when I was upon the eastern hill, the cape of land called Dengenefs, which extends nearly two miles into the sea, and is about sixteen miles distant from Hastings, in a right line, appeared as if quite close to it; as did the fishing-boats, and other vessels, which were sailing between the two places: they were likewise magnified to a great degree\*.

\* On this interesting subject see Mr. Huddart's Observations on Horizontal Refractions, *Philos. Journal*, I. 145. Ellicot, on the Phenomenon of Looming, I. 152. The *Fata Morgana*, or Appearance of Figures in the Sea and Air, before Reggio, described, I. 225. Mudge, in the *Philos. Transf.* 1795, p. 586, 587. Smeaton's Account of Eddystone Light-House, p. 191. Hutton's Dictionary, II. 352. These appearances are much more frequent and general than has usually been supposed. I have been credibly informed, that the *Fata Morgana* has been seen from Broad Stairs, in Kent; and that the elevation and inversion of terrestrial objects is commonly or frequently observable (in summer, I suppose), through a telescope, over level ground, if the eye of the observer be not much elevated. In the curious instance related above by Mr. Latham, it does not seem probable that the French coast was enlarged, but that it was only elevated by the refraction. At all events, the objects could scarcely have suffered any alteration of the horizontal angles or bearings. N.

STATE of the THERMOMETER at *HASTINGS*, during the Month of JULY 1797.

1797.	Therm.	Time.	Wind.	Weather.
July 1.	64	10 A. M.	SW	Windy. Fair.
2.	64	10	SW	Windy. Fair.
3.	62	10	SW	Rain. Windy.
4.	62	10	SW	Fair. Windy.
5.	61	10	SW	Rain. Windy.
6.	60	10	SW	Rain. Windy.
7.	61	10	W	Rain. Windy.
8.	62	10	NW	Fine.
	66	5 P. M.	NW	Fine.
9.	66	10 A. M.	SW	Fine.
10.	67	10	N afterw. SW	Fine.
11.	65	10	SW	Foggy all day.
12.	63	10	SW	Fine.
13.	72	10	SW	Fine.
14.	76	10	W	Fine.
	68	12	W	Fine.
15.	72	10	W	Fine.
16.	72	10	N	Fine.
	78	7 P. M.	E	Storm of Wind. Lightning.
17.	73	10 A. M.	W	Fine.
18.	70	10	W	Fine. Showers in the Night.
19.	67	10	WSW	Fine. Windy.
20.	67	10	SW	Rain. Windy.
21.	65	10	SW	Fine. Windy.
22.	61	10	S	Rain.
23.	65	10	S	Fine.
24.	66	10	S	Fine.
25.	66	10	SW	Fine.
26.	68	10	SW	Fine. Dead calm all day.
	76	5 P. M.	SW	Fine.
27.	72	10 A. M.	SW	Fine.
28.	70	10	S	Fine.
29.	72	10	E	Fine.
30.	70	10	SW	Rain.
31.	69	10	S	Fine. Windy.

## XI.

*An Account of Electrical Machines of considerable Power, in which Silk is used instead of Glass. W. N.*

**E**LECTRICITY has been, in most practical cases, excited or collected by the friction of various substances against each other. In this process, which has not yet been explained by reference to simpler phenomena, it is a known condition, that one at least of the substances rubbed must be a non-conductor. Resin, lack, silk, baked wood, and above all glass, are the bodies which have hitherto been used. The durability and unchangeable nature of glass\*, and its being very little if at all affected by the atmospherical variations, are undoubtedly the causes why it has obtained the preference. Its brittleness, and the great expence of large plates or cylinders, are certainly among those reasons why philosophical operators should be desirous of a substitute of less cost and danger.

Dr. Ingenhoufs, the inventor of the plate machine, made a variety of experiments for this purpose. Pasteboard thoroughly dried and heated, and then soaked and varnished with a solution of amber in linseed oil, formed plates which were strongly electrified when rubbed with a cat's skin or hare's skin. He tried baked wood boiled in linseed oil, but with less success. A cylinder of strong silk velvet, formed by stretching that substance upon two circular wooden disks, was found to afford considerable electrical force when caused to revolve against a cushion covered with hare's skin†. And lastly, the same philosopher contrived a portable apparatus for charging a jar by means of a varnished silk ribband, exposed to the friction of a rubber attached to the external coating, while the opposite electricity of the silk was taken off by a metallic part communicating with the inside‡.

It was at the beginning of 1784 that M. Walckiers de St. Amand undertook to construct a machine, in which a piece of silk was made to revolve incessantly, and pass between two pair of rubbers. He made one of small dimensions, and afterwards a larger in which the silk was twenty-five feet in length, and five feet broad. In the following year a machine of the same kind was constructed by M. Rouland, professor and lecturer in natural philosophy in the university of Paris §. As no accounts of either of these have been published in this country ||, and the advantages and effects described by the authors appear

\* Mr. C. Cuypers of Delft affirms that glass becomes harder, and fitter for electrical purposes, by long exposure to the warm air of a room; and Mr. Birch, of Essex-street, in a very extensive electrical practice, found that glass cylinders lose their power by long use, so as to become of no value; but he ascribes this change to the use of the aurum musivum on the cushion.

† Bakerian Lecture, Phil. Trans. 1779.

‡ Nouvelles Expériences, &c. sur la Physique, par J. Ingenhoufs, F.R.S. &c. Paris, 1785.

§ Description des Machines électriques à taffetas, par M. Rouland, &c. Amsterdam and Paris 1785, octavo, 35 pages, with one plate, of which Pl. XVIII. is a copy. — The Report of the Parisian Academy on the Machine of M. Walckiers, dated 25th March 1784, was separately printed in 29 pages octavo, with a coloured plate. The Report is copied in M. Rouland's pamphlet.

|| Some time after the construction of these machines, Mr. Edward Nairne, of Cornhill, whose researches in this and other practical departments of science are well known, received an order to construct one of the same form, but found it impossible by any adjustment to prevent the silk from running totally to one or the other end of

appear to be considerable, I have thought it of advantage to insert the description of the latter in this place.

A, B, Pl. XVIII. is a wooden table four feet and a half long, two feet nine inches wide, and somewhat more than an inch and a half thick: its feet are 18 inches long. Upon this table are fastened by strong wooden screws, *a b c d*, two cross pieces, each nine inches broad, which carry the uprights C, D, E, F, which last are 27 inches in height. At about two-thirds or more of the height of these uprights, there are cut notches of an inch square each, in which the axes of the two cylinders G and H turn freely. These axes are parallel to the table and to each other, and are kept in their place by clamps of wood screwed over them. The cylinders G and H are formed of light wood glued together, and covered at the ends by a circular piece, whose rounded edges arise half an inch above the surface of the cylinders themselves. Their diameter is eight inches; the axes are of box wood, and are less than an inch in diameter, having a shoulder which prevents the ends of the cylinders from touching the uprights when turned round; and lastly, the cylinders are covered with serge.

The handle is copper, its radius being six inches long.

K, L, is a piece of taffety covered with oily and resinous matter, of the same kind as is used in France in the construction of air-balloons, which, M. Rouland says, renders the silk very electrical: the breadth of the silk is nearly one inch less than the length of the cylinders, and it is wrapped round them with its ends sewed together.

The whole breadth of the silk is taken hold of or pinched between two flattened tin tubes opposite each other at M, and two of the same kind at N: these are the rubbers, and may be made to press against each other, more or less strongly, by means of screws. They are retained by strings of silk fastened to the four uprights of the machine. *vv* are two brass chains hooked upon the rubbers, and communicating with the earth; *op* and *qr* are four pieces of taffety, prepared in the same manner as the principal piece, sewed in the direction of their length to the rubbers, and fastened to each other by their corresponding corners by means of threads of silk. The metallic tubes or rubbers are covered with cat's skin.

S represents the conductor. It is a cylinder of brass three inches in diameter, 36 inches in length, including the balls at the end, whose diameters are four inches: one of these balls has a ring, *t*, above it, which serves to form a communication between the conductor S and any other conductor.

The upper and lower parts of this cylindrical prime conductor are armed with two plates of brass *yy*, whose length is equal and correspondent to the breadth of the taffety, which is 26 inches, and 132 inches or 11 feet long: the edges of the plates are about half an inch distant from the silk, and serve instead of the metallic points that were used by M. Wafckiers, but rejected by M. Rouland, because they were apt to stick into the silk and damage it.

of the cylinders. The inventors have given no instructions to obviate this defect. I have no doubt but that they made their rollers gradually largest in the middle. I have seen a machine for folding woollens, invented and made by Mr. Rehe, of Shoe-lane, in which this difficulty was removed in some leading rollers, by making them in the form of two very acute frustums of cones, joined at the middle by the larger base.—See Phil. Journal, I. 23.

The conductor S is suspended by silk strings, fastened to the uprights of the machine by the hooks and rings *ii*: its situation is parallel to the cylinders G, H, and equidistant from each. The action of this machine is as follows: The cylinder H is moved rapidly on its axis by means of the handle, and the cylinder G moves of course in the same direction on the two extremities of its axis, provided the taffety K, L, be properly stretched. This tension is easily obtained; because the cross pieces to which the uprights C, D, and E, F, are fixed, may be moved nearer or further from each other, and fastened by means of the screws *a b* and *c d*, which pass through holes cut in the direction of the table.

The rotation of the cylinders necessarily producing a circulation of the taffety, it must consequently be rubbed in its passage between the tin tubes covered with cat's skin at M and N; and by this friction it obtains what is called the negative electricity, which is communicated from both parts of the silk to the common conductor S. But it may be made to electrify positively, by removing the rubbers to the middle of the silk, so that the prime conductor may communicate with them: or, if the two cushions be removed to half the distance between the revolving cylinders and the prime conductor, positive and negative electricity may be had at the same time, the rubbers being in a negative state, and the prime conductor in a positive state.

The advantages of a machine of this construction beyond those of glass are stated by the inventor to be, 1. It is not brittle in any part. 2. Its excitation is more steady, because it requires no amalgam. 3. Its dimensions have no limit.

The power of excitation in this way appears to have been very considerable. The facts are not related with so much detail as could be wished in the Report of the Academy; but it appears that the negative sparks from the conductor of Walckiers, which was five feet long, were from 15 to 17 inches in length, very loud and dense, and very painful to the hand; that pointed bodies emitted very sensible sparks to the conductor; and that a battery of 50 square feet was charged by 30 turns of the machine, which gives 19 feet of silk rubbed to charge one foot of glass\*. In another instance, however, it is said, that a square foot was charged by one turn of the machine, which answered to  $31\frac{1}{2}$  square feet of silk. It is not said whether the labour of turning was considerable or not.

M. Rouland made several trials to substitute plain silk instead of that which was varnished; and he also tried woollens and mixed cloth containing goat's hair; but none of these answered to his satisfaction.

## XII.

*Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.*

(Continued from page 276, vol. ii.)

### Proposition VI.

**T**HE expence of fluid is less through cylindrical tubes than through conical tubes, which diverge from the commencement of the contracted vein, and have the same external diameter.

\* See Philos. Journ. I. 87.



The general theory is the same for both these forms of tubes; but the loss of living force is greater in the cylinder, and the effect of the communication of motion in these tubes cannot approach its maximum as in the cone. Let the tube  $ACNM$ , fig. 5. Pl. VIII. of the present volume, have the form of the contracted vein in  $ACFD$ ; the cylindrical part  $GINM$  has its diameter  $MN$ , greater than  $DF$ . By the reasoning made use of in the preceding proposition, it is proved, that the lateral communication of motion tends to produce a vacuum in the solid zone  $ROY SXQTZ$ . If the communication of motion in this tube were completely made, it would follow, that the pressure of the atmosphere would increase the velocity of the contracted vein in the ratio of  $DF^2$  to  $MN^2$ .

But the form itself of the cylindrical pipe always destroys a notable part of the effect: for the fluid filaments  $AD$ , in turning through the curve  $DR$ , proceed briskly to strike the sides of the tube  $GM$  at  $R$ , where they lose part of their motion. In the space  $DGR$  eddies, or circular whirls, are produced, as in a basin which receives water by a channel. These eddies are, to a certain extent, a failure of the effect, and retard the efflux of the stream. A much less increase of the expenditure takes place in the cylindrical tube than would answer to the ratio of  $DF^2$  to  $MN^2$ .

*Experiment XVIII.*—A notion may be formed of these internal shocks and eddies in the cylindric tube, and their effects on the efflux of the fluid, if attention be paid to the following table of the expenditure through the different additional tubes in the horizontal position. All these tubes have the diameter of their two extremities = 18 lines; they were all provided with the conical tube of the form of the contracted vein at their inner extremity, excepting that of fig. 6. The charge was always 32.5 inches above the centre of the orifice.

*Table of the Times employed in discharging Four Cubic Feet of Water through the different Adjutages.*

Through the orifice in a thin plate,	-	-	-	41".
Through the simple tube of fig. 6.	-	-	-	31".
Through the tube of the form of fig. 5.	-	-	-	31".
After having amended (adouci) the conical divergent part, $DFIG$ , of the same tube,	-	-	-	30".
Through the tube fig. 9.	-	-	-	32".5
Through the conical tube of the form fig. 10.	-	-	-	27".5
Through the tube fig. 5. the portion $GINM$ being 23.5 lines in diameter, and 84 in length, the rest as before,	-	-	-	27".

It may perhaps be demanded, whether, in the internal part of the simple cylindric tube  $KL$  of fig. 6. there be the same augmentation of velocity, and the same contraction of the stream, as in the compound tube of fig. 5.? By reasoning according to the principles we have established, I think, 1. That in the section  $KL$  of fig. 6. there is the same increase of velocity as we have seen (Prop. II.) take place in the section  $AC$  of fig. 5. The direction of the fluid particles which pass through these two sections must be the same in both cases, because this direction can depend only on the impulse received within the reservoir, which is the same in both. 2. In fig. 6. the fluid particles, after having passed through the section  $KL$ , begin immediately to experience the effect of the lateral communication of motion. They must therefore deviate laterally through the curve  $Lxz$ , before they arrive

rive at the place of contraction which they assume at D F, fig. 5. and which they likewise assume when the orifice is made in a thin plate. If we imagine a tube of glass y K, one extremity of which is applied at K, fig. 6. and the other extremity open in the interior part of the reservoir, it will be seen that the pressure of the atmosphere, which is exerted upon the coloured fluid T, must likewise act on the surface of the reservoir, and join the pressure of the fluid in the reservoir to press the water into the tube y K, as it presses the coloured liquor into T S. The pressure of the atmosphere must, in the same manner, augment the impulse of all the fluid particles which arrive at K L, and consequently must increase the expenditure.

Since the checks and eddies in an additional cylindric tube must always destroy a part of the active force of the fluid, it follows, that the fluid column issuing out of the tube can never acquire the whole velocity which is due to the actual charge, and is observed nearly entire in the orifices through a thin plate; and the diminution of velocity corresponds with the increase of the time beyond that indicated by the theory, as may be seen in the following

*Experiment XIX.* The orifice P fig. 1. being made through a thin plate, and the vertical height P M, being 54 inches, the distance M N of the jet was 81,5 inches. Having applied to the same orifice the cylindrical tube of fig. 5. and the perpendicular P M being let fall from the external orifice of the tube, the distance M N was found to be 69 inches. According to the theory, the expenditure of four cubical feet through this tube ought to have taken place in 26",24, but it really employed 31". And the proportions  $31'' : 26'',24 = 81,5 : 69$  nearly.

The same observation may be made on an experiment of Michelotti (tom. ii. pages 22 and 23). P M being 19,33 feet, and the water issuing through an orifice in a thin plate M N was 23,2 feet; it was no more than 20 when an additional cylindric tube was applied which had not even the proper length.

It is evident, that the theory of the lateral communication of motion must likewise apply in the same manner to descending and ascending tubes, whenever their form admits of this lateral communication. In descending tubes, we must add the increase of expenditure occasioned by this cause to that which is produced by the acceleration of gravity, and which we have estimated in Proposition IV. In ascending tubes, gravity acts in a contrary direction, and consequently its effect must be deducted from that of the lateral communication. Experiment VII. relates to ascending tubes. The following relate to other positions.

*Experiment XX.* The tube A B E E of fig. 11, Experiment XV. was applied in the place of the tube B C Q O, in fig. 7. The height of the water in the reservoir above the lower extremity of the tube was 41,5 inches. The four cubical feet of water were emitted in 22".

I applied the same conical tube A B F E, fig. 11, to the orifice R, fig. 8, to form an ascending jet a little inclined from the perpendicular. The height of the water of the reservoir above the upper extremity of the tube was 23 inches. The expenditure of four cubical feet was made in 30".

The time of the expenditure in Experiment XV. was 25". And by comparing it with the present, we find nearly  $\sqrt{41,5} : \sqrt{32,5} = 25'' : 22''$ . And  $\sqrt{23} : \sqrt{32,5} = 25'' : 30''$ .

*Expe-*

*Experiment XXI.* The orifice R, fig. 8, was circular, and 4,5 lines in diameter; the charge was 31,7 inches, and the jet declined a little from the perpendicular. The orifice being through a thin plate, afforded a cubical foot of water in 161". With an additional cylindrical tube of the same diameter, and ten lines in length, the cubical foot of water was emitted in 121".

Under a charge of 56 inches, the same orifice afforded, through the vertical jet, a cubical foot in 123" through the thin plate, and in 91" with the same additional tube.

These two results being combined, give for the expenditure of vertical jets a mean ratio, between the thin plate and the cylindrical adjutage, of 100 to 134, which is also the ratio between the horizontal jets.

*Experiment XXII.* I applied the glass tube QRT, fig. 6, to the point S fig. 5. of the compound tube ACMN, the distance BS being 24 lines. In this situation the fluid T no longer rises in the tube. This proves that the lateral translation of the fluid in the cylindrical tube is made very near the place where the vein is contracted, and that consequently DR must briskly strike the side GM.

By this experiment we see that the distance BR to which the oblique filaments strike the sides of the tube, does not amount to 24 lines. Supposing DO = 20 lines, the time which the particle D employs to pass through the space DO in my experiments is less than  $\frac{1}{10}$  of a second. Let us decompose the curve-lined motion DR according to the lines DO, OR. Let us suppose the acceleration through OR to be uniform, and it will be found that this acceleration is at least five times as great as that of heavy bodies. If the lateral force through OR were simply the mutual attraction of the particles of the water, this attraction in the particle D must not only overcome the inertia of the particle itself, but likewise that of the other particles nearer the axis, which follow D in its deviation through DR, and impress upon them a much greater sum of acceleration than that of gravity. Now the force of attraction of one particle of water is not greater than the natural gravity of a thread of water of the length of one line at most. The lateral communication of motion, which is the cause of the acceleration through OR, is therefore much greater than could have been produced by the mutual attraction of the particles of water.

*Proposition VII.* By means of proper adjutages applied to a given cylindric tube, it is possible to increase the expenditure of water through that tube in the proportion of 24 to 10, the charge or height of the reservoir remaining the same.

I shall here give an account of the different precautions necessary to be taken when the expenditure of water through a cylindrical tube of a given length is required to be the greatest possible.

1. The inner extremity of the tube AD (fig. 13.) must be fitted at AB with a conical piece of the form of the contracted vein\*; this increases the expenditure as 12,1 to 10. Every other form will afford less. If the diameter at A be too great, the contraction will be made beyond B, and the section of the vein will be smaller than the section of the tube.

2. At the other extremity of the pipe BC apply a truncated conical tube CD, of which let the length be nearly nine times the diameter C, and its external diameter D must be 1,8 C. This additional piece will increase the expenditure as 24 to 12,1. (Experiment XVI.) By this means the quantity of water will be increased by the two adjutages ABCD, in the proportion of 24 to 10.

\* Boffut, Art. 509.

At Rome, the inhabitants purchase the right of conveying water from the public reservoirs into their houses. The law prohibits them from making the pipe of conveyance larger than the aperture granted them at the reservoir, as far as the distance of fifty feet \*. The legislature was therefore aware, that an additional pipe of greater diameter than the orifice would increase the expenditure; but it was not perceived that the law might be equally evaded by applying the conical frustum C D beyond the fifty feet. From this second rule we learn, that it is not proper to make the flues of chimneys too large in the apartments; but that it will be sufficient if they be enlarged at their upper terminations, according to the form C D, fig. 13. This divergency of the upper part will carry off the smoke very well, even when it is not practicable to afford chimneys of sufficient length to the upper apartments. The same observation is applicable to chemical furnaces for strong fire.

3. The pipe B C ought to be straight, without elbows or curvatures. To the experiments which Boffut has made on this head† I shall add the following.

[To be continued.]

### ACCOUNTS OF BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1798. Part II. Quarto. 593 pages, with a List of Presents and Index, occupying 12 pages more, and 17 plates. Sold by Elmsly, London.

**T**HIS Part contains the following papers: 10. A Disquisition of the Stability of Ships, by George Atwood, Esq.—11. Quelques Remarques d'Optique, principalement relatives à la Reflexibilité des Rayons de la Lumière. Par P. Prevost, Professeur de Philosophie à Geneve, de l'Académie de Berlin, de la Société des Curieux de la Nature, et de la Société Royale d'Edimbourg. Communicated by Sir Charles Blagden, Knt. F. R. S.—12. An Account of the Orifice in the Retina of the Human Eye, discovered by Professor Sæmmering: to which are added, Proofs of this Appearance being extended to the Eyes of other Animals. By Everard Home, Esq. F. R. S.—13. A Description of a very unusual Formation of the Human Heart. By Mr. James Wilson, Surgeon. Communicated by Matthew Baillie, M. D. F. R. S.—14. Account of a singular Instance of Atmospheric Refraction. In a Letter from William Latham, Esq. F. R. S. and A. S. to the Rev. Henry Whitfield, D. D. F. R. S. and A. S. (See *Philos. Journal*, II. 417.)—15. Account of a Tumour found in the Substance of the Human Placenta. By John Clarke, M. D. Communicated by the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—16. On the Roots of Equations. By James Wood, B. D. Fellow of St. John's College, Cambridge. Communicated by the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.—17. General Theorems, chiefly Porisms, in the higher Geometry. By Henry Brougham, jun. Esq. Communicated by Sir Charles Blagden, Knt. F. R. S.—18. Observations of the Diurnal Variation of the Magnetic Needle in the Island of St. Helena; with a Continuation of the Observations at Fort Marlborough in the Island of Sumatra. By John Macdonald, Esq. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—19. On the Corundum Stone, from Asia. By the Right Hon. Charles Greville, F. R. S.—20. An Enquiry concerning the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F. R. S. M. R. I. A. (See *Philos. Journal* II. 400.)—

\* Fontin. de aquæduct. art. 203. 106 et 112.

† Art. 631 et seq.

21. Experiments to determine the Density of the Earth. By H. Cavendish, Esq. F. R. S. and A. S.—22. An improved Solution of a Problem in Physical Astronomy; by which swiftly converging Series are obtained, which are useful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction: To which is added an Appendix, containing an easy Method of obtaining the Sums of many slowly converging Series which arise in taking the Fluents of Binomial Surds, &c. By the Rev. John Hellins, F. R. S. Vicar of Potter's Pury, in Northamptonshire. In a Letter to the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.—23. Account of a Substance found in a Clay-pit; and of the Effect of the Mere of Difs upon various Substances immersed in it By Mr. Benjamin Wiseman, of Difs, in Norfolk. Communicated by John Frere, Esq. F. R. S. With an Analysis of the Water of the said Mere. By Charles Hatchett, Esq. F. R. S. In a Letter to the Rt. Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—24. A Catalogue of Sanscrita Manuscripts, presented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Esq. F. R. S.—Presents received by the Royal Society from November 1797 to June 1798.—Index.

Cases of the Diabetes Mellitus, with the Results of the Trials of certain Acids and other Substances in the Cure of the Lues Venerea. By John Rollo, M. D. Surgeon General, Royal Artillery. Second edition, with large additions. Octavo, 628 pages. London, Dilly, 1798.

An account of the first edition of this excellent treatise was given in the first volume of this Journal, page 285. The present edition is greatly enlarged; but the time of receiving it, and other circumstances, prevent me from giving an abstract of its contents at present. The chemical reader will see with pleasure, in one of the articles of the present Number, that Mr. Cruickshank has extended his researches into the nature of Sugar and Vegetable Mucilage. See page 406.

*L'Art du Blanchement des Toiles, Fils et Cotons de tout Genre, &c. In English.* The Art of Bleaching Piece Goods, Thread and Cottons, of every Description; rendered more easy and general by means of the Oxygenated Muriatic Acid; with the Method of rendering Painted or Printed Goods perfectly White and Colourless. To which are added, the most certain Methods of Bleaching Silk and Wool; and the Discoveries made by the Author in the Art of Bleaching Paper. Illustrated with 9 large Plates in 4to. representing all the Utensils and different Manipulations of the Bleaching Process: An elementary Work, composed for the Use of Manufacturers, Bleachers, Dyers, Calico Printers, and Paper-Makers. By Pajot des Charmes, formerly Inspector of Manufactures. 8vo. 202 pages. Sold by Dugour and Durand, Rue et Hôtel Serpente, à Paris.

This work appears to be of such great practical utility that I have begun a translation, which will appear in the course of the winter, with additional information, which I hope to procure, respecting the practice of the intelligent manufacturers of our own country.

*Memoires et Observations de Chimie de Bertrand Pelletier, Docteur de Medecine, &c.; Or, Memoirs and Observations in Chemistry, by Bertrand Pelletier, Doctor of Medicine, &c. Collected and arranged by Charles Pelletier, and Sedellot the younger, M.D. &c. 2 vols. octavo, with 5 plates, and a portrait of the Author. Sold at Paris by Croulebois, Fuchs, Barrois, and the principal booksellers.*

The works of Pelletier are known and valued by every scientific chemist. Most of them have

have already appeared in the *Annales de Chimie* and *Journal de Physique*; but this collection contains, as Lametherie informs us, some new articles and annotations of the author. The utility of such collections is obvious, even if their sole object were to form into one body the scattered productions of a man of genius. The works of many eminent writers of *Mémoires* have lost half their utility, for want of this friendly office to science and to posthumous fame.

An English Translation of the late Dr. Stewart's "*Propositiones Geometricæ More Veterum demonstratæ*" is announced for publication by Mr. Leybourn.

It consists of a series of geometrical theorems, mostly new, investigated first by analysis, and afterwards synthetically demonstrated by an inversion of the same analysis. The utility of such a work is evident; and the celebrity of the original, which is now scarce, will render the present translation more valuable.

From a passage in the *Eloge de Leibnitz*, in the volume of the French Academy for 1716, and in the first of the two volumes of *Eloges par Fontenelle*, I conclude that the George Dalgarno, mentioned in page 345, is so named by an error of the press, which but too frequently occurs in copying proper names from obscure manuscript. His name appears to have been *Dalgarme*. I shall give the passage in English, on account of its curiosity and value. (See page 493 of the last-mentioned work.)

"It might now seem as if we had exhausted the subject of the labours of Leibnitz. But this is not the case; not because we have passed over in silence a very great number of individual facts, sufficient to have established the fame of any other man, but because what remains to be narrated is of a very different kind from what we have already given. It is the project of a Philosophical and Universal Language, which he had conceived. Wilkins Bishop of Chester, and Dalgarme, had laboured at this enterprise: but when Leibnitz was in England, he observed to Boyle and Oldenbourg, that he did not think those great men had followed the true method. They might cause nations whose languages were different, to communicate together with ease; but they had not seized the true real characters, which were the most delicate instrument the human understanding could avail itself of, and were calculated extremely to facilitate the processes of reason, the powers of memory, and the invention of things. These characters were to resemble as much as possible the characters of Algebra, which, in fact, are very simple and very expressive, which never present any redundancy or equivocal expression, and of which all the varieties are demonstrative. He has somewhere spoken of an *alphabet of human thoughts*, which he meditated. According to every appearance, this alphabet bore relation to his universal language. After the discovery of this, it would have been also necessary to have discovered the art of persuading the different nations to use it; and this would not have been the smallest difficulty of the task." With regard to this last art, it appears to me to be very simple, and capable of being pointed out in a few words. On this subject the reader may consult the remarks at pages 189 and 191 of our present volume.



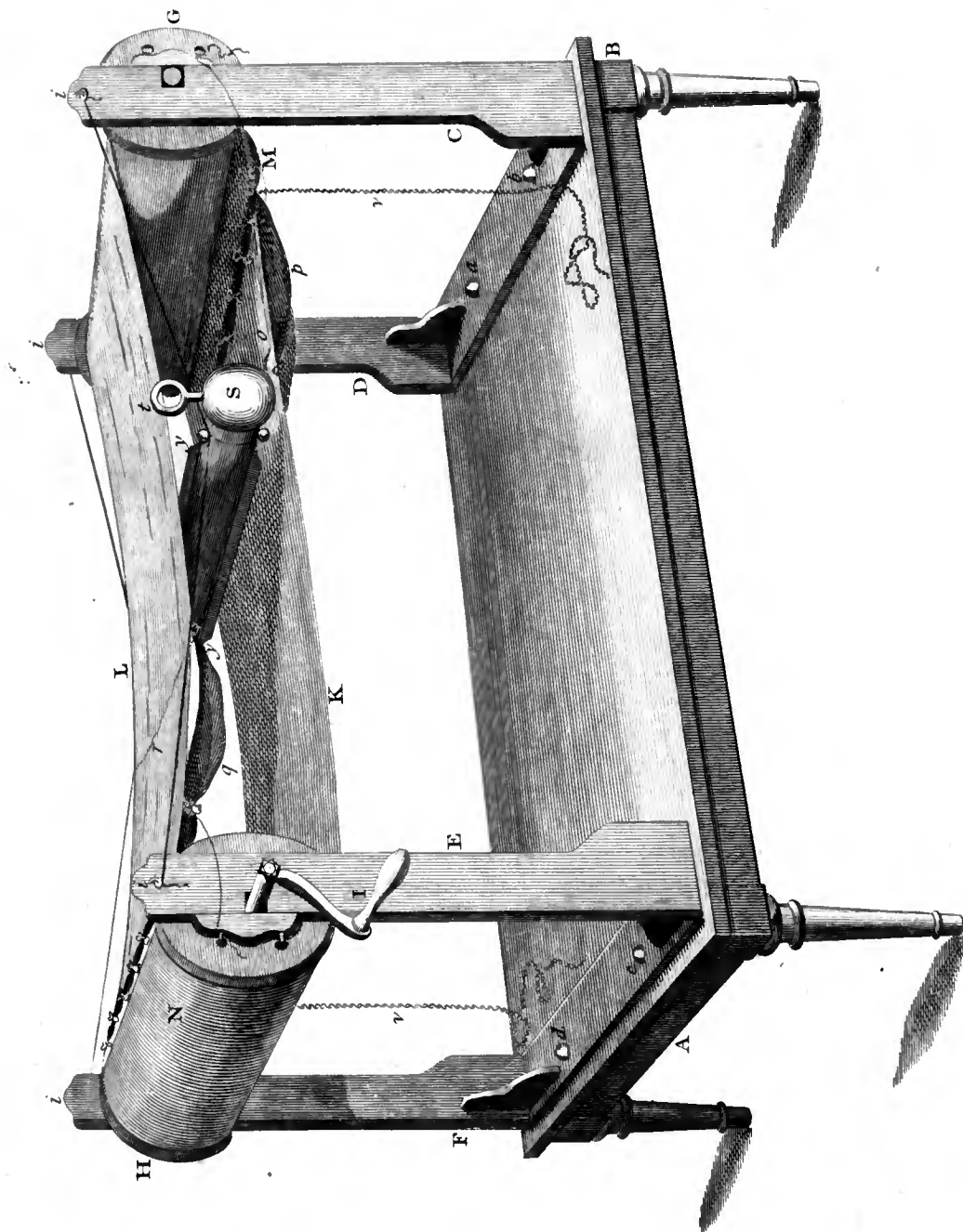


Mansion of Rosafennia in Ireland, destroyed by the change of Climate during the present Century.





*Powerful Electrical Machine of Walckiers & Rouland.*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JANUARY 1799.

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ARTICLE I.

*Description of a new Instrument for drawing equidistant and other parallel Lines with great accuracy and expedition; intended principally for the use of Engravers. With Specimens of its Performance. By W. N.*

SOME months ago I was informed by the Engraver who executes the plates for this Journal, that an ingenious artist had constructed a machine for ruling the shades and grounds of copper-plate engravings, which he not only used with great success in his own business, but had sold to others under an engagement of secrecy. How far this information might be strictly accurate, was of less consequence at the time this conversation took place, than the consideration that it would be easy to make such a tool, and render its advantages more general by publication. I then promised to make one; and having now finished it, I take the earliest opportunity of communicating it to the world. From the preceding historical statement, the reader will see that I have no claim to the original thought of substituting mechanical operation instead of hand work in this department of engraving; and those who have seen the screw gear of Ramsden's great dividing engine will also perceive, that I have done little more than distribute the parts of this tool in what appeared to me to be the most simple and convenient manner.

Fig. 1. Plate XX. represents the instrument of the size of that which I have made. The outline section, fig. 2, represents the same viewed in a direction parallel to the edge of the moveable ruler. The letters denote the same things in both. A A A A is a frame fixed to the drawing-board. It resembles that of a sliding rule, and serves to guide a sliding piece, which, lying in fig. 1, immediately under the screw, could not with convenience be denoted by any letter. B C is a screw of exactly forty threads in the inch, but might conveniently be made of a coarser thread. G H and E D are two cocks, the former of which, G H, is fixed to the frame A, and bears a clip or pair of nuts, which open and shut with a joint like a pair of compasses, and either embrace the screw by a re-

gularly tapped part when shut, or leave it at liberty when open. Fig. 3. shews the face of this cock with the clip, of which P is the joint, QQ tapped part, R a pin to insure their coming up fairly, and S a claw, which, when drawn upwards, serves to close them; or, if thrown back, leaves them at liberty to open by the action of a small spiral spring, let into opposite holes near the joint. ED, fig. 1, represents the other cock, which is fixed to the sliding piece. It carries the steel ruler EF, which, though sufficiently strong, is thin enough to adapt itself to slight variations of thickness of the plate beneath it. In my instrument I have made it adjustable to much greater variations of thickness, by means of an horizontal axis; but as this contrivance adds to the expence, and diminishes the simplicity of the instrument, I would rather recommend that great variations should be allowed for by putting paper or thin slips of metal underneath the plate AA, as may be required. The end C of the screw is turned down, and fixed in the cock ED by means of a nut and washer; or in any other of the methods which are familiar to instrument-makers. The upper part of the cock ED is filed round, and cut into teeth, of which fifty would complete the whole circle. The centre of this external circular part corresponds with the axis of the screw (see fig. 4.). L, M represent two short cylindrical pieces which are hollow, and apply to each other so as to form a kind of box. Within, and fixed to the part M, which is fixed to the screw itself, there is a ratchet wheel divided into fifty teeth; and within the part L, which is merely supported by the cylindrical part or stem of the screw, there is a ratchet, which holds when the part L is moved by its handle N from right to left, but escapes when that handle is moved in the contrary direction. O is a lever, or arm, likewise supported by the stem of the screw, and occupying the remaining space between the handle N and the cock D. At the outer extremity of this lever there is a small steel blade, which, by means of a back spring exactly resembling that of a pocket knife, may be made to form a continuation of the lever itself, or, by being placed at right angles to the lever, may be made to rest in any of the divisions between the teeth of the circumference of the cock; and consequently will, by that means, confine the lever to the position in which it is placed. The handle N cannot pass the lever O, because this last is too thick, and there is a stud or pin T, fig. 4, upon the face of the cock D, which prevents the handle from being moved beyond a certain determinate station to the left hand. And, lastly, the ruler IK, which is represented as broken off at K, but (like EF) may be of any required length, serves, by means of a thumb screw at I, and another at the opposite end, to secure the copper-plate against the drawing-board in the usual manner.

After this description the use of the apparatus may be easily understood. By drawing back the claw S, fig. 3. the screw is set at liberty, and the ruler EF may be brought to any required distance from IK by hand. The plate may then be duly placed and secured to the board, and the clip drawn gently together by the claw S. In this situation, suppose the lever O to be placed at a considerable distance from the handle N, that handle may be moved to the right, during which the click will gather upon the ratchet wheel; and then being returned to the left, it will carry the screw round. The gentle pressure, exerted by means of the claw, will tend to close the clip upon the screw, as soon as it comes into a fair position by its rotation; at which instant the claw will suddenly fall into its place, and the machine is ready for work, excepting that the adjustment for the fineness or coarseness of the

the

the stroke must first be made. This is done by the lever O. If the steel blade be dropped into the first notch beginning on the left hand, the handle N will be confined; if at the second notch, the handle, upon being moved backwards and forwards between the pin T and the lever O, will move the screw through one tooth, or one-fiftieth of a turn, each time, and consequently will carry the ruler F through one 2000th part of an inch. If the blade of the lever O be placed in any other of the notches (which are numbered by filing the corners of every fifth notch), the quantity passed over, at each return of the handle, will be greater or less according to the number. As there are but twenty-six notches, the greatest single shift of this instrument will be one-eightieth part of an inch; but as the shift is so readily made, it is easy, even with this fine screw, to reach greater intervals, by moving the handle once, twice, or even three times, between stroke and stroke. Thus for one-fiftieth of an inch, or  $\frac{4}{5000}$ ths, the number of intervals cannot be passed over at one stroke; but, if the blade be set at the twentieth notch, the ruler will be shifted exactly that quantity by two movements of the handle. It is needless to multiply directions for this part of the operation; and the more so, as the artist is much more likely to determine the fineness of his shade by previous trials, which will render him acquainted with the instrument, than by any numerical computation.

An instrument like the present may also be usefully employed in subdividing lines into small parts. If curved lines be required to be drawn at equal distances from each other, the simplest method appears to be that of varying the figure of a temporary edge of horn or metal, which may be fixed to EF: and if lines be required converging to a point, the ruler EF may be made to occupy the radius of a circle of any magnitude whatever, by giving an adjustable angular figure to the slider, and causing it to move against two points, upon principles deduced from the thirty-first proposition of Euclid's third book.

As this instrument was made at intervals of leisure, without any account being kept of time or materials, I cannot distinctly state the cost: but I estimate that the whole being made of brass and steel, and well finished, with a small drawing-board and the wooden holding piece IK, might be afforded by a shopkeeper in the mathematical-instrument business for ten guineas. If the frame and slider were of box-wood, which I have no doubt would answer very well, and the ornamental covering LM of the ratchet-wheel were omitted, I suppose the instrument could be afforded for considerably less than half that sum.

The engraver has used this apparatus in the plates of the present number.

## II.

*Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M. R. I. A. Corresponding Member of the Royal Society of Edinburgh, &c.*

[Concluded from Page 386, Vol. II.]

*Of the Effects of these Western Tempests on the General Temperature of the Climate.*

**I**N a paper, honoured with a place in the second volume of the Transactions of the Academy, I recorded the result of some experiments made in the year 1788, for the purpose of ascertaining the temperature of the earth in our latitudes.

From these experiments it appears, that the medium temperature of the kingdom of Ireland, from its southern to its northern extremity, nearly coincides with the standard assigned to corresponding degrees of latitude in Mr. Kirwan's ingenious tables\*.

It is further discoverable, that the general warmth of our island is in no respect inferior to that of other countries in its parallel: from whence we are naturally led to conclude, that the annual quantity of heat received here is not less in our day than it was in former ages.

If these things are so, it may be denied that there is any substantial foundation for the frequent complaint of change in our seasons. The ocean may be agitated by storms; sands be drifted along our coasts, and trees blasted in mountainous and exposed situations; but why should the farmer lament the diminished ardour and shortened period of his summers, as if winter possessed more than its natural portion of the revolving year? Whence should arise the gardener's complaints, that his peaches, however sheltered from storms, rarely now arrive at maturity; and that his May-duke cherries, even in the most favoured aspects, no longer merit that distinguished title?

May I be permitted to assume, as true, the fact which it has hitherto been the purpose of this paper to demonstrate, and in support of which the trees, the sands, and the tides of our island seem to bear constant and unequivocal testimony; namely that, of late years, the western winds have blown with increased violence? From that single principle an answer may be deduced to all these interesting questions.

It is a matter well known, and easily to be explained, that the surface of the ocean varies less from the mean annual temperature of its latitude, than land in the same parallel†: that is, the surface of the sea is colder in summer, and warmer in winter, than the surface of the ground in that latitude.

If then the prevalent winds of any country blow over an ocean situated in its parallel, that country will relatively be denominated temperate; it will be free from all extremes; the heats of summer and the colds of winter will be checked by sea breezes of a contrary property; and the land, influenced by the neighbouring element, must, more or less, partake in its equability of temperature.

Such is the case in almost all the islands of the world: and such from immemorial ages has been the peculiar character of Ireland. Hither come the western winds, modified by the temperature of the broad Atlantic ocean, which they traverse in their career: hither fly the clouds, teeming with moisture collected in a course of three thousand miles along its surface; and here uniformity of temperature, and redundant humidity, have always been marked as the distinguished character of our soil.

Since then the trees, the sands, and the tides demonstrate that these winds have, of late years, blown with unusual violence; since they bear testimony that a large quantity of air thus directed, thus tempered, and surcharged, has passed over our lands; it plainly follows that the climate must have felt the change; that it must have experienced colder summers and milder winters than heretofore, approaching towards that equability of heat and redundancy of moisture which the farmer and the gardener at present so heavily lament.

But it is not from increase of quantity alone that these winds have produced their effects.

\* See Kirwan's Estimate of Temperatures.

† Ibid.

They have altered the temperature of the ocean itself; and thus have, as it were, multiplied their changeful influence on the land; acting there at once with new properties, as well as with increased quantity.

The surface of water in a state of tranquillity admits of greater variations of temperature than in a state of agitation. It may become much hotter in summer and colder in winter when calm than when disturbed; for the particles at the surface, when heated or cooled, do not immediately give place to others nearer the bottom; the process of commixture, in a tranquil state, is gradual, and the transmission of change somewhat resembles the slow and retarded progress that takes place on the land. Agitation always counteracts this gradual process; a rapid commixture of the particles produces a quick assimilation of temperature throughout the whole mass, and, thus taking away all partial excesses, reduces the whole toward a medium state of uniformity.

Thus, summer tempests always tend to diminish the superficial heat of an ocean over which they rush; while winter blasts, agitating the waters at considerable depths, resist the natural cold of the season by a supply of relatively warm particles, which arise from the bottom toward the surface.

That the Atlantic ocean, the vast and potent arbiter of our seasons, has suffered unusual agitation of late years, seems evident from the natural phenomena recorded in the beginning of this Memoir. The trees and sands of our island bear testimony to the storms which sweep along its surface; and the tides come to us as frequent and unerring messengers of the tumultuous and agitated state of its billows attendant on their furious career.

From this perturbed abyss of waters has arisen an unceasing influence, equally potent to check the ardour of the summer solar beams, or to relax the shackles of a northern winter. By this prevailing influence, summer has been rendered impotent to raise and ripen many of our crops; and the farmer, taught by necessity, learns now to hand them over ready grown, and prepared for the maturity of summer under the mild temperature of an Atlantic winter. Hence too the gardener has, of late years, been compelled to call in the aid of artificial heat to forward the peach, the nectarine, the grape, and every other species of delicate fruit to perfection.

Hence premiums now vainly attempt the growth of hemp, which formerly peasants, in the most northern counties of Ireland, cultivated without reward around their cabins for domestic use. And hence the ancient apiaries of our island, once so celebrated, and guarded with such special and minute attention by the Brehon laws \*, are now extinct; and honey, from being a common article of popular consumption, has become a rare luxury, or an expensive medicine.

Winter has likewise felt the general influence of this Atlantic temperature; our grasses scarcely droop beneath the frosts; wheat and oats vegetate in the open fields during the very solstice itself; myrtles and laurels, in sheltered situations, brave the severity of winter; the Foyle, and other large rivers of the northern province, frequently subject to the icy chains of former ages, now run in uninterrupted freedom †.

\* See *Collectanea de Reb. Hib. Transl. Brehon Laws.*

† Fifty years have elapsed since the river Foyle has been effectually frozen over at Derry. It is also observed that the river Thames, in Britain, is less frequently frozen of late years than formerly. See *Archæologia Britannica*, vol. iii. p. 55.

Facts such as these are to be considered as the basis of general opinion concerning the alteration of our climate; while old age, connecting these appearances with the fragility of declining life, and a decayed constitution, has become querulous in proportion to its feelings, and, judging of external phenomena by the exaggerated test of its own acute sensations, emphatically pronounces that the seasons are now less favourable than formerly.

*On the Consequences of this Equability of Temperature to Animal and Vegetable Life.*

FROM the transient perusal of this Memoir, a rapid and impatient mind would probably draw numberless false conclusions. The facts here recorded seem to wear a gloomy aspect; to mark a gradual deterioration of our seasons; to indicate a climate, harsh, ungenial, and of consequence sterile in its nature; clouded, humid, tempestuous, cheerless, and unfriendly to animal and vegetable life.

All these conclusions contradict experience, and may be overturned by a calm consideration of the phenomena themselves.

Experience teaches us that dry seasons and easterly gales are, in our island, invariable sources of feeble vegetation and numerous diseases\*; and the history of the world informs us that winds, whether hot or cold, are in their nature deleterious to animal and vegetable life, in proportion as they become deprived of humidity. The warm air of the African desert breathes desolation over the parched land of Egypt and Syria, before it is yet felt as a tempest†. Even our own westerly winds, the Atlantic messengers of health and fertility to our island, after traversing the cold and dry tract of the vast Siberian continent, bear nothing but sterility, and almost perpetual winter, to the unhappy climate of Kamtschatka‡.

Heat or cold in extremes, dry air in rapid motion, and moist air in a stagnant state, seem to be the principal external sources of human diseases; and climates are generally found favourable to health and longevity in proportion as they are exempt from these natural causes of disorder and decay.

It ought therefore to be inferred, *a priori*, that Ireland, celebrated for the singular equability of its temperature, and the ceaseless motion of an atmosphere always influenced by the moisture of the Atlantic, should be likewise characteristically free from natural disease; and experience proves that this conclusion is true.

The exhausting agues of North America, or the fens of England§; the fatal fluxes which prevail in the low countries of the continent of Europe; the dreadful bilious distempers of both the Indies; the pestilence which desolates the African and Asiatic cli-

\* Of this the influenza, attendant on the easterly winds of spring in the present year (1795), affords a strong instance.

† See Volney's account of Egypt and Syria.

‡ The latitude of Kamtschatka corresponds with that of Ireland; the westerly winds are prevalent in each; yet the former experiences a rigorous winter of nine months, and the latter rarely of as many days. See Cook's Voyage in 1779, vol. iii. ch. 6.

§ The ague is so rare in several parts of Ireland, that many persons are totally unacquainted with it. In the northern province the author has never met with the disease, and its existence there is generally denied.



mates, are all either entirely unknown, or but feebly felt in Ireland. There is here no characteristic disease to mark the natural source of unhealthfulness. There are few disorders which cannot be directly traced up to some artificial cause; to some intemperance, to some neglect, to some excess, either of luxury or penury, in the suffering subject. Manufactures in their kind unwholesome, industry exerted beyond its proper limits, irregularity of food or fermented liquors, illicit amours, colds, the consequence of folly or inattention, excess or deficiency of proper exercise, anxiety and fretfulness of mind, together with the copious list of casualties, afford to the physician of this country the causes and explanation of almost all its disorders.

From its peculiar salubrity, the natives of this island are celebrated through Europe for just symmetry of proportion and an athletic frame; because, from earliest infancy to manhood, a check is rarely given to the progressive increase of animal strength, or the natural and appropriate forms of an undiseased body. From the same hygeian source flow those ardent passions, those exuberant streams of animal spirits, which render our natives always cheerful, oftentimes turbulent and boisterous, the usual consequences of uninterrupted health and a vigorous constitution. Hence wild adventure, personal courage, impetuosity of pursuit, inattention to consequences, and improvidence of disposition, become the characteristic features of minds seldom under the influence of that anxiety, which, flying from the present moment and its enjoyments, watches for futurity, and pants after remote felicity.

The general temperature of our climate, in the vicinity of the capital, is somewhat lower than the 50th degree of Fahrenheit's thermometer\*; and a mean of the hottest or coldest months of our year rarely varies more than ten degrees from this standard heat †. Winter therefore with us is usually accompanied with a temperature of forty degrees, spring and autumn of fifty degrees, the summer of sixty degrees of the thermometer: and the general heat of any single month of these several seasons seldom varies much from the corresponding temperature of that particular season to which it belongs.

Of these limits the lowest is not sufficiently cold to check the growth of any of the natural herbage of our island, nor the highest powerful enough to parch the surface of a moist soil, or to scorch its luxuriant grasses. Hence it comes to pass that our fields maintain a perpetual verdure, unimpaired by either solstice. Hence too the farmer is enabled to lay his lands under grass almost at any season, even at the commencement of winter ‡: and hence the grazier never loses the benefit of his rich pastures at any period of the year, unless during the transient passage of a temporary drift of snow; so that horses, cattle, and sheep, arrive here, with little care, at a degree of perfection unattainable in other countries without infinite expence and trouble.

For the growth of flax, the staple commodity of the kingdom; of potatoes, the general food of its inhabitants; and of barley, from whence the ardent spirits and other fermented liquors of the country are derived; the climate seems to possess a peculiar felicity of tem-

\* See Hamilton's paper on the temperature of Ireland, in the second volume of these Transactions.

† See State of the Weather in Dublin, from June 1791 to June 1793, by Mr. Kirwan, Vol. V. of these Transactions.

‡ See Young's Tour in Ireland, Vol. I. p. 130.

perature, and moist in all its seasons; and for their necessary degree of ripeness the heat of summer is amply sufficient.

It is for the more delicate species of garden-fruits, for ill conducted crops of oats in bleak and mountainous situations, and for the complete and perfect maturity of wheat, that uneasy apprehensions can ever be entertained by the gardener or the farmer.

The former are in themselves of little importance; and, where such luxuries are required, glass coverings and artificial heat afford an ample supply, and remove all pretence for discontent. Human art and industry, attentive to circumstances, and accommodated to times, has stolen from the mild temperatures of our winters as much warmth as serves to compensate for the deficiency of summer heats in forwarding and perfecting the crops of wheat; and a little will teach the farmer in our mountainous provinces to use the same artifice with respect to his oats, whenever necessity shall demand his increased attention.

To sum up matters then with truth and brevity—A density of population, surpassing that of the vaunted millions of undepopulated France\*; a copious export-trade in provisions of various kinds, unequalled by any kingdom whose inhabitants are proportionably numerous†; and a staple manufacture, unrivalled in general use, in certainty of produce, and intrinsic value‡; are circumstances which have not fallen to the lot of other nations, and bring with them clear and irrefragable evidence to demonstrate a salubrious country, a genial climate, and a fertile soil in Ireland.

\* The inhabitants of Ireland amount to more than four millions. (See Mr. Bushe's Memoir, Irish Transf. Vol. III.)

Hence the density of its population is at the rate of 182 persons to a square geographical mile, supposing the island to contain in round numbers 22,000 superficial miles.

Mr. Zimmerman, in his Political Survey of Europe, erroneously supposing the inhabitants of this country less than two millions and a half, deduces a density of population only to 117 persons on each mile. The following table marks the number of individuals on a geographical square mile in the northern countries of Europe, as taken from Zimmerman's tables (Ireland being corrected); from whence it will appear that this kingdom rates extremely in the density of its population.

	Persons.			
Denmark	-	-	-	12
Sweden	-	-	-	14
Russia	-	-	-	20
Scotland	-	-	-	51
Poland	-	-	-	53
Prussia	-	-	-	67
England	-	-	-	129
Germany	-	-	-	135
France	-	-	-	152
Ireland	-	-	-	182
Holland	-	-	-	236

† Two hundred and twenty thousand barrels of beef and pork, independent of the various matters connected therewith, and of every other species of provision, were exported in the year ending 25th March 1793.

‡ After a progressive increase of ninety years the exports of linen-cloth alone, in the year 1792, amounted to forty-three million yards, from one million only at the commencement of the present century.

CONCLUSION.

## CONCLUSION.

IN this Memoir I have endeavoured to prove, from natural and almost incontrovertible registers of the phenomena of later years, that the winds, and particularly the western storms, have swept with increased violence over Ireland.

From this fact I have deduced a necessary change in its climate; a more general equability of temperature through the year; summers less warm, and winters more mild and open: and lastly, I have endeavoured to support this conclusion by general observation, and the enumeration of particular instances where the defect of summer heat, and winter ice, seem to be the most strongly marked.

One question still remains curious in its principle, and interesting in its solution—Why have these western winds blown with unusual and increased violence?

The limits of a Memoir such as this do not permit me to enlarge on this subject, and even appear to include a reproof for the length of the present interruption. I shall therefore trespass no longer on the moments of the Academy than to suggest a few queries, which may stand over for future consideration; leaving it to time, and the ingenuity or better directed observations of others, to verify, to disprove, or to condemn, the hints which may be contained in them.

1st. Have not our winds become more violent, and the temperature of our seasons more equable, since the forests of Ireland were cleared, and the country cultivated? And have not these winds, and that equability of temperature, been nearly proportioned to these as to their causes?

2d. Have not similar changes occurred under analogous circumstances in North America; even in Canada, that country of extremes in heat and cold? And did not the island of Bermudas, though situated so much to the southward of us, become barren of fruit in consequence of the destruction of its timber-trees?

3d. Has it not appeared from observations on the ascent of balloons, and the motion of clouds, that the lower mass of air often pursues a different course from the upper stratum? May not then the limits of our stormy currents of air be often confined within a few hundred yards of the surface of the earth? And if so, is it not possible, and even probable, that the frequent interruption of forests and groves, and hedge-row trees, might have formerly very much retarded, and finally checked, the progress of a tempest?

4th. Have not all the countries of Europe, Asia, and America, within the parallel of Ireland, been very much denuded of their forests within the present century? And has not the increased velocity of the westerly winds been proportioned to this destruction of the forests and trees, as to their natural causes?

5th. Is it not probable, since the prevalent winds of our parallel have a westerly tendency, that circumstances which have removed impediments to their career round the entire globe would give increased velocity to their course?

6th. Should not Ireland, launched as it is into the Atlantic beyond the other coasts of Europe, and denuded as it has been of forests and hedge-row trees, be most sensibly affected by increasing tempests from the west, and the first to experience their influence on its climate?

Whether these queries have any foundation in nature, or are merely to be esteemed the result of an unrestrained imagination; whether so diminutive an animal as man, so tem-

porary in duration, so impotent in strength, acting through the lengthened period and persevering efforts of a large portion of his species, can reasonably be deemed equal to the involuntary production of such vast effects; to a change even of the elements and climates of the earth, may admit of doubt, of opposition, and even of denial. For these reasons I have taken the liberty simply to propose them as matters of enquiry; and only beg the indulgence of the Academy for troubling them with matters that may not be attended with sufficient evidence to bring conviction, or even to escape censure.

### POSTSCRIPT.

October 20, 1795.

THE present year (1795) has been comparatively cold in its commencement, and warm in its summer: it has been calm, dry, and in many respects a contrast to the long series of preceding years, whose effects are registered in this Memoir.

Yet the extremes of its temperature have not been equal to those of former years, as far as can be inferred from the recorded effects of heat and cold on natural objects. The large rivers of the northern province have not been bound firmly in the icy chains of winter, nor have the grapes ripened in favourable aspects in any part of the kingdom during summer, as formerly occurred in seasons whose temperature was denominated extreme.

### III.

*Experiments and Observations on Electricity—Excitation—the two States—Points of Difference between the Action of weak and strong Electricities compared together. (W. N.)*

THE excitation of electrics by friction still remains a mystery. We possess nothing in the least resembling a theory of the changes of electrical capacity which the rubber and cylinder undergo during their change of relative position in contact. In the year 1789 I communicated several facts to the Royal Society\* concerning the mutual action of the silk flap and the electrical cylinder upon each other, and shewed that something like compensation takes place while they remain together. The transition of the surface of the cylinder from the compensated to the uncompensated state is strikingly seen when a hole is cut in the silk, and the cylinder turned under circumstances of considerable excitation. A cascade of fire issues from that edge of the hole which is nearest the cushion; but instead of being dissipated into the air, it bends down again, and unites with the cylinder at the opposite edge of the hole, whence it proceeds as usual to the receiving part of the prime conductor.

The phenomena of the two electricities called plus and minus are singularly distinct in almost every experiment which can be made with the exhibition of electric light. Paper is a good substance for observing the visible passage of electricity. If a strong plus electric stream be let fall on the flat side of an uninsulated sheet of paper, it forms a beautiful star about four inches in diameter, consisting of very distinct radii not ramified. The minus electricity, in circumstances perfectly similar, throws many pointed brushes to the paper,

\* Philosophical Transactions, No. lxxix.

but forms no star upon it. In this experiment I used a machine with a cylinder of seven inches diameter.

Hence it seems to follow, that a hollow ball of paper, or a glass globe covered with paper, might form an amusing part of the electrical apparatus for experiments in the dark.

The laws of action with regard to weak electricities and those of considerable intensity appear to differ in various particulars, which require further investigation. When a number of jars are charged in succession, that is to say, by insulating the whole series, and causing the outer coating of the first to communicate with the inner of the second, and the outer coating of the second with the inner of the third, and so forth, it is well known that the whole may be charged by communicating electricity to the inside of the first, provided the last have a communication with the earth; but with this circumstance, that the charge will restore itself by explosion when the quantity of electricity is much less than the first jar would singly have received and retained. From this result it has with some probability been concluded, that glass resists the communication of electric energy, and that the jars are successively less and less charged. Whether this last conclusion be true, has not I believe been ascertained by experiment.

Under all the uncertainties concerning the place occupied by the electric charge of coated glass, though it may seem unfair to make any inference respecting glass which is uncoated, yet, upon the whole, there appears to be a probability that the interposition of naked glass may impede the action of electrified bodies. This question more immediately points at the tube in which the gold-leaf electrometer of Bennet is inclosed. To determine whether the tube of the electrometer does affect the electric state of the included leaf, either by compensation or otherwise, I took a piece of window-glass eighteen inches long, two inches wide, and one-twentieth of an inch thick, which I cleaned very well, and then passed it several times through the hot air over the flame of a candle. In this state one end of the glass was laid gently upon the electrified plate of Bennet's electrometer, and then suddenly raised by a turn of the wrist. It was scarcely possible to discern that the leaves were at all affected; but when the electrometer was in the plus state a very slight collapshion was produced by raising the glass, and the contrary effect was produced when the electrometer was negative. Some days afterwards the experiment was repeated, after the gold-leaf had been changed for other pieces, which were very pointed and delicate in their movements. The result was, that the glass was always shewn by the electrometer to be in a weak positive state; and, when the electricity of the electrometer was made plus, the collapshion was equal to the divergence when it was minus.

In making these experiments I had previously supposed that the influence of the metallic state of the electrometer would produce somewhat of the nature of a charge upon the glass; and consequently that the intensity of the leaves would have been diminished during the existence of that charge; and also, that in such a case the action of the metal through the glass would be subject to the same diminution as in the series of jars. But as the glass did not appear to act in this manner, it seems proper to conclude that clean glass does not affect the electric state of bodies by its vicinity, and that the divergence of the balls or the gold-leaf in the electrometers of Cavallo and Bennet is not diminished by the tube which surrounds them.

From a variety of experiments it was clearly ascertained that the metallic coatings, though by their vicinity they may diminish the intensity of the electric state in the leaves, do nevertheless increase the angle of divergence by their attraction.

When the gold-leaf electrometer is made with a very small tube, its sensibility is somewhat increased by the nearness of the coatings; but the chance of rendering it unserviceable from casual friction, which excites the glass, and causes the gold leaf to stick to it, together with the less perfect view of the divergence through a tube of small curvature, afford reasons why a diameter of less than an inch should be rejected. Other reasons of convenience indicate that the diameter of the glass should not much exceed this quantity.

I was once induced to think that the considerable magnitude of the cap of Bennet's electrometer might render it less capable of being acted upon by small quantities of electricity. Experiment did not however give much countenance to this supposition. By trials with heads of different size, the smallest were found to be rather more sensible to extremely minute electricities, and less so to such as were greater. The influence of very weak electricity may produce the opposite state in the whole of a small head, but only in part of a larger; the remaining part of this last assuming the opposite state, and robbing the leaves of part of their intensity. But in higher electricities the whole of the large head may be urged to give electricity to the leaves, in a quantity which the smaller head could not give without acquiring a higher degree of intensity, and consequently more strongly resisting the desired process. It appears therefore that the maximum of effect with a given electricity, acting without communication, will not be obtained but by an head of a definite figure and magnitude.

From some experiments of Hoadley and Wilson with a number of prime conductors separately insulated, it was established that an electrified body brought near one end of such a set of conductors, forming by contact a right line, will produce the contrary state in the nearer conductors, and the same state in those which are more remote. Or, upon the hypothesis of a single fluid, the electricity of the body presented will repel that of the compound conductor; so that by separating them before the influence is removed, and afterwards examining them, the respective states of the several portions of the whole line may be ascertained.

It was a conclusion obviously enough deducible from this experiment, that if two electrometers of Bennet were connected by a metallic bar, there ought to be a difference when an electrified body is presented at either extremity, or at the middle of the bar. I made this experiment with a brass bar eighteen inches long. Both electrometers were affected at the same instant. Their divergence was equal, and the effect was the same, whether the glass or sealing-wax was presented at either end or at the middle of the bar. This result seems to indicate a difference in the mode of action between the very weak electricity I used and the much stronger of Hoadley and Wilson.

The action of pointed bodies and of flame likewise exhibits a remarkable difference between the strong and the weak electricities. The prime conductor of an electrical machine is very suddenly deprived of the greatest part of its electricity by the operation of a metallic point; but does not seem to be much affected by the vicinity of burning  
candles.

candles. On the contrary, the electrometer of Bennet will scarcely communicate the least portion of its electricity to a point not absolutely in contact with its cap; but is readily deprived of its electric state by a candle.

Coulomb, by his method of torsion, has determined that the action of weak electricities diminishes as the square of the distance. I do not know that any experiments have been made to determine the ratio of the repulsion or attraction of bodies by electricity, with regard to the distance, when the intensities are considerable.

To examine this object I caused a brass conductor to be made, four inches in diameter, with a spherical part at each end, of five inches diameter; and, on the whole, twenty inches long. It was fixed with its axis in the vertical position. In the upper spherical part there was a stage supporting a pulley, the axis of which ran upon two pair of very delicate friction wheels. The lower spherical part was perforated in the axis, to receive the stem of a very light sphere of paper gilt, and of the diameter of five inches. Care was taken that this stem, which was nearly the whole length of the conductor from the friction wheels to the lower orifice, should not touch that orifice; and for greater security there was a small frame carrying three friction rollers, against one of which it was sure to bear in case of accidental irregularity. The upper part of the stem was fastened to a fine silken thread which passed over the pulley, and was tied to a counterpoise adapted by its figure to receive small weights, in order that the equilibrium or preponderance might be adjusted at pleasure. On the axis of the pulley was an index and hand, which moved upon a face without rouching, beneath a convex glass, and shewed the descent of the gilded ball in inches and parts. I expected to have obtained a considerable scale on my stem of twelve inches. But in the trials I made when it was finished, in which the ascending power of the ball could be varied at pleasure by addition to the counterpoise, I found either that it did not move at all, or that, if it did, it ran with considerable velocity through its whole length. This unexpected event, together with some other imperfections in the instrument itself, which required amendment and alteration, prevented me from pursuing and diversifying a course of experiment that promised so little. The facts appear however to shew, that in great intensities and short distances, the diminution of effect, if it follow the law of the squares of the distances, is, like the attraction of the earth upon projectiles, too small to be perceived. But from the indications afforded by Henley's quadrant electrometer, and the floating electrometer of Nollet, there appear to be reasons why the pursuit should not be thought unworthy of further experiment.

#### IV.

*Analysis of the Red Lead of Siberia; with Experiments on the New Metal it contains. By Citizen VAUQUELIN, Inspector of Mines, and Conservator of Chemical Products at the Mineralogical School.*

(Concluded from page 393, Vol. II.)

*Combinations of the Acid of Red Lead with the Alkalis.*

THE acid of red lead forms, in its combination with the alkalis, salts which are soluble, crystallisable, and coloured. The simplest process for making these salts consists in

in boiling upon one part of red lead reduced into fine powder, two parts of carbonate of alkali with forty parts of water. By this means a double affinity is made to operate, by virtue of which carbonate of lead is formed, and falls to the bottom, while a combination of the acid of red lead with the alkali made use of remains suspended in the water, by virtue of its solubility.

The proportion of alkaline carbonate proper to be used for the decomposition of the native red lead must vary according to the proportions which subsist between the bases, the acids, and the water of crystallization. The proportions stated in the last paragraph are suited to the carbonate of pot-ash; but the proper quantities in the other cases are easily ascertained, by using a small quantity of the carbonate at first, and afterwards gradually increasing it, till the greatest part of the red lead is decomposed. On the whole, it is better to use a smaller than too large a portion of the carbonate, in order that its base may be more completely saturated with the acid of the red lead.

There is no reason to fear a similar inconvenience with the carbonate of ammoniac. It is proper, on the contrary, to add more than is sufficient to saturate the acid of the red lead; because a portion of this salt will always be driven off during the ebullition which is indispensably necessary to produce the intended decomposition. And even in case there should remain an excess after the complete decomposition of the red lead, there would be no danger, because this excess of alkali is driven off by the evaporation of the salt newly formed, and there is always a certainty of obtaining the combination perfectly pure.

The colour of the combinations of the acid of red lead with the alkalis is an orange yellow \*. Their solutions afford crystals nearly of the same tinge, the differences being only slight variations of shade. These salts are decomposed by barytes, lime and strontian. The mineral acids likewise decompose them by an inverse operation.

These salts afford oxygen gas by the action of heat, and their residue has the appearance of a green mass. The ammoniacal compound must however be excepted, as its base is partly decomposed by the oxygen of the acid, and leaves in the retort a pure green oxide, the undecomposed portion of ammoniac being also driven off by the heat.

These salts decompose by double affinity the calcareous, barytic, magnesian, aluminous salts, &c. Most of the metallic salts are likewise decomposed by these substances, and afford new combinations scarcely or not at all soluble in water; and for the most part red, yellow, orange, or lemon colour.

I have not hitherto been able to determine the figure of these salts with accuracy, nor their degree of solubility in water, for the reasons I have already explained; neither have I made all the combinations of this acid with the metallic oxides, nor determined its mode of action on the metals. Such experiments would no doubt have afforded interesting phenomena for the completion of the history of this metallic acid.

But I indulge the hope that these experiments are only retarded, and that some fortunate opportunity will hereafter enable us to perform them: and, at all events, the experiments already made will be sufficient to ascertain the presence of the acid of red lead wherever it may be met with, and distinguish it from every other natural substance.

\* That of the ammoniacal salt, which has the form of plated crystals, is yellow, and exhibits the metallic brilliancy of gold. V.



## SECTION VII.

*Reduction of the Acid of Red Lead to the Metallic State.*

THOUGH the properties of the acid of red lead, described in the foregoing paragraphs, are in strictness sufficient to convince those who are in the habit of treating metallic substances, that this substance belongs to the same class, it is nevertheless desirable, in order, as it were, to add the stamp of evidence to the proofs of demonstration, that this acid should be reduced to the metallic state.

To obtain this object I took 72 parts of the acid in question, extracted from the red lead by the muriatic acid, as described in our fourth section, which I introduced into a crucible of charcoal placed in another of hard porcelain likewise filled with charcoal powder.

This apparatus, placed in a forge furnace, was exposed for an hour to the action of a very strong fire urged by the blast of a strong pair of bellows with three pipes.

When the crucible was cooled and broken, I found, to my great satisfaction, in the small vessel of charcoal, a metallic mass of a white grey colour, shining, very brittle, and upon the surface of which were many feathered crystals of the same colour perfectly metallic. This metallic mass weighed forty-three parts.

It appears, from the result of this operation, that the oxygen does not adhere with very great force to this metallic base, and that the acid contains about 0,40 of its weight of this acidifying principle.

## SECTION VIII.

*Properties of the Metal of Red Lead.*

THIS metal, as I have already remarked, is of a white colour inclining to grey: it is very brittle, and crystallises in an elevated temperature into a kind of feathered filaments, which rise above the metallic mass.

The metallic button being broken, presented internally compact points forming a close grain, and in other parts needles crossing each other in all directions, leaving void spaces between them, which prevented me from determining its specific gravity.

A fragment of this metal exposed to the heat of the blow-pipe acquired a tarnish on its surface, and by continuing the operation it was covered with a light green crust; but it afforded no sign of fusion.

When heated by the same apparatus with borax it did not melt, but was in a small degree diminished, and communicated an emerald-green colour to the salt.

When reduced to a fine powder, and treated with the concentrated boiling nitric acid, it was oxidised with much difficulty, and communicated to the acid a green tinge slightly inclining to blue. The nitric acid attacks this metal with so much difficulty, that it was not till after treating it repeatedly with considerable quantities of that solvent that I could succeed in dissolving six grains.

The different solutions being added together were evaporated to dryness. Towards the end of the operation the residue assumed the form and the ductility of a vegetable extract of a red brown colour. Caustic potash poured on this residue dissolved a great part, and assumed a lemon-yellow colour; but part remained of a very beautiful green, which was not acted upon by that re-agent: it was a portion of the metal which had not received

from the nitric acid the whole quantity of oxygen necessary for its acidification. It was still in the state of oxide.

I treated it again with the concentrated nitric acid; and by a series of operations of this nature I at length succeeded in completely acidifying it.

The combination of this artificial acid with potash exhibited absolutely the same appearances with the different re-agents as the salt which is formed with the natural acid. See Section II.

## SECTION IX.

### *Denomination of the Metal contained in the native Red Lead.*

WHEN an unknown substance is to be examined, the only method to ascertain whether it has been before described, is to examine its properties, and compare them with those of other bodies; an operation which supposes a knowledge of all that has been before described in natural history. And when, after an accurate comparison of the properties of the body under examination with those of other bodies, it is found that none of these last exhibit the whole of those properties, a fair conclusion may be formed that the body is unknown, and consequently that it is new.

After this point is determined, it becomes necessary, in order to make it known to others, that its distinctive characters should be clearly ascertained, and a name given to it, for the purpose of brief designation, and of inscription in the catalogue of human knowledge.

This name may be derived from various sources;—the place whence it was obtained, the author who discovered it, or the specific properties it possesses, &c.: but it is easy to see that the two first sources are vicious. In fact, the name of the place seems to announce that the substance is to be found exclusively there; and it is very far from being known, at the early period of discovery, whether it may not be found elsewhere. The name of the author teaches nothing, except that such a man was the discoverer; a circumstance of little interest to science. But the name of a substance deduced from its leading properties is truly useful, because it brings to mind, and in some measure places the object before the eyes by a faithful sketch of its attributes.

From these considerations I have thought fit to adopt the name *chrome*, which was proposed to me by Cit. Haüy, to designate the new metal found in the native red lead. In truth, this name does not perfectly agree with the complete metal, because it has no very distinct colour; and because, even if it had one, this would not be a sufficient reason, since every metal has a more or less peculiar colour.

But it agrees wonderfully well with its combinations with oxygen, which afford a green oxide, or a red acid, according to the proportions of that principle, and because each of its primary combinations communicates its colour to all the secondary combinations into which it enters; properties which belong to it almost exclusively.

This name appears to me to be so much the better founded, as this substance has not yet been discovered except in the state of acid or oxide, and because it may perhaps never be found in the native metallic state.

Nevertheless, as I am not disposed to consider the adoption of one name rather than another as of any importance, provided the name do express some remarkable or distinctive property of that which it denotes, I shall with pleasure, if a better name be found, consent to substitute it instead of *chrome*.

## SECTION X.

### *Uses of Chrome, of the Oxide of Chrome, and of the Chromic Acid.*

THE brittleness of chrome, its resistance to the action of fire, and the small masses in which it has hitherto been found, afford little expectation that this metal will ever be of great use in the arts. But this assertion may be too strong; for a new substance, of which the properties at first appear to be of small interest to society, is sometimes found, in the subsequent processes of investigation, to be capable of very important applications in the arts and sciences.

The acid and the oxide of this metal are certainly not subject to the same observation. The former, by the beautiful emerald-green which it communicates even to enamels, without being subject to alteration in the purity of its shade, will afford to painters and enamellers an additional object to enrich their productions, and add to the perfection of their art: the second, by the beautiful cinnabar red which it assumes and keeps in its combination with mercury; the orange-red colour it affords with lead; the carmelite red it communicates to silver, may become highly valuable for paintings in oil and water-colours.

Chemistry will be greatly benefited by this excellent re-agent to detect the smallest quantities of mercury, silver, and lead, dissolved in acids by the different colours it produces when its action is assisted by an alkali. And, in the same manner as this acid indicates the presence of the metals here spoken of, those metals in their turn may serve to discover the chromic acid, if it be previously put into the necessary condition to produce the before-mentioned effects.

If the chromic acid should hereafter be abundantly found in any other combination besides that of lead, it might, after extraction by carbonate of potash, be used for the artificial composition of red lead, and furnish, in abundance, this valuable orange-red colour for painters' use, which is sold very dear in Siberia, and applied to this purpose with great success. For this object the native red lead, which is disseminated in small crystals, or plates, in the fissures of quartzose or gritstone gangues, in which it is commonly found, might be pulverized, then boiled with a solution of carbonate of potash, and mixed with nitrate of lead; the acid of which will saturate the potash, and afford its metal to the new acid, which will thus produce a colour no less beautiful than the natural, and perfectly clear of the matrix.

There is reason to presume that chrome, either in the state of oxide or of acid, will be found singly or engaged in some other combinations. For the analysis of the emerald of Peru has already shewn me that its colouring part is afforded by the oxide of this metal; a circumstance which gives the most agreeable proof of the goodness and fixity of this colour, as it is known that the emerald can sustain the most violent degree of heat without losing its colour.

I have likewise found that the yellowish green tufted crystals, which often accompany the red lead ore of Siberia, are formed of chrome and lead, both united, in the state of oxide. Green crystals possessing the same form, the same dimensions, and the same situations on the matrix as those of red lead, but which are a combination of the oxides of chrome and of lead, are likewise found in certain specimens of the native red lead. It is probable that these combinations originally existed in the state of chromate of lead, and that in process

of time a portion of oxygen having been carried off by unknown causes, converted them to the state of oxide, and changed the red colour to green.

From paragraph III. it follows that the Siberian red lead contains

		By analysis. Experiment V.	By synthesis. Experiment VI.
Oxide of lead	-	63,96	65,12
Chromic acid	-	36,40	34,88

## V.

*Experiments to determine the Density of the Earth. By HENRY CAVENDISH, Esq.  
F. R. S. and A. S.\**

MANY years ago the Rev. John Michell, F. R. S. contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian professor at Cambridge, who, not having conveniences for making experiments with it in the manner he could wish, presented it to Mr. Cavendish.

The apparatus is very simple: it consists of a wooden arm six feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position by a slender wire forty inches long, and to each extremity is hung a leaden ball about two inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre than is necessary to twist the suspending wire, it is plain that, if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. Michell intended to use were eight inches diameter. One of these was to be placed on one side of the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and consequently half the difference of these positions would shew how much the arm was drawn aside by the attraction of the weights.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do by putting the arm in motion, and observing the time of its vibrations, from which it may be easily computed†.

\* Abridgment of his paper in the Philosophical Transactions, 1798, p. 469.

† Mr. Coulomb has in a variety of cases used a contrivance of this kind for trying small attractions; but Mr. Michell informed Mr. C. of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.

Mr. Michell had prepared two wooden stands, on which the leaden weights were to be supported and pushed forwards till they came almost in contact with the case; but he seems to have intended to move them by hand.

As the force with which the balls are attracted by these weights is excessively minute, not more than one fifty-millionth part of their weight, it is plain that a very minute disturbing force will be sufficient to destroy the success of the experiment: and from the following experiments it will appear, that the disturbing force most difficult to guard against is that arising from the variations of heat and cold; for, if one side of the case is warmer than the other, the air in contact with it will be rarefied, and in consequence will ascend, while that on the other side will descend, and produce a current which will draw the arm sensibly aside\*.

As Mr. Cavendish was convinced of the necessity of guarding against this source of error, he resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without by means of a telescope, and to suspend the leaden weights in such a manner that he could move them without entering into the room. This difference in the manner of observing rendered it necessary to make some alteration in Mr. Michell's apparatus: and as there were some parts of it which Mr. Cavendish thought not so convenient as could be wished, he chose to make the greatest part of it afresh.

Plate XIX. is a longitudinal vertical section through the instrument, and the building in which it is placed. A B C D D C B A E F F E is the case,  $x$  and  $x$  are two balls which are suspended by the wires  $bx$  from the arm  $gbmb$ , which is itself suspended by the slender wire  $gl$ . This arm consists of a slender deal rod  $bmb$ , strengthened by a silver wire  $bgb$ ; by which means it is made strong enough to support the balls, though very light†.

The case is supported and set horizontal by four screws, resting on posts fixed firmly into the ground: two of them are represented in the figure by S and S; the two others are not represented, to avoid confusion. G G and G G are the end walls of the building. W and W are the leaden weights which are suspended by the copper rods Rr PrR and the wooden bar  $rr$  from the centre pin Pp. This pin passes through a hole in the beam H H, perpendicularly over the centre of the instrument, and turns round in it, being prevented from falling by the plate  $p$ . M M is a pulley fastened to this pin, and Mm a cord wound round the pulley and passing through the end wall, by which the observer may turn it round, and thereby move the weights from one situation to the other. When the weights are in the position represented in the engraving, both conspire, by their action on the small weights  $xx$ , to draw the arm in the same direction; but when, by means of the pulley

\* Mr. Cassini, in observing the variation-compass placed by him in the Observatory (which was constructed so as to make very minute changes of position visible, and in which the needle was suspended by a silk thread), found that standing near the box, in order to observe, drew the needle sensibly aside; which I have no doubt was caused by this current of air. It must be observed that his compass-box was of metal, which transmits heat faster than wood, and also was many inches deep; both which causes served to increase the current of air. To diminish the effect of this current, it is by all means advisable to make the box in which the needle plays not much deeper than is necessary to prevent the needle from striking against the top and bottom. C.

† Mr. Michell's rod was entirely of wood, and was much stronger and stiffer than this, though not much heavier; but as it had warped when it came to Mr. Cavendish, he chose to make another, and preferred this form, partly as being easier to construct and meeting with less resistance from the air, and partly because, from its being of a less complicated form, he could more easily compute how much it was attracted by the weights.

MM, the observer causes them to move through a considerable portion of a semicircle, they will arrive at a second position, in which their joint action will move the arm in the contrary direction. These weights are prevented from striking the instrument by pieces of wood fastened to the wall of the building.

The situation of the arm is determined by slips of ivory placed within the case near each extremity of the arm. These slips bear a set of divisions of the inch into twenty parts, to which a small slip at each end of the arm applies, without contact, a vernier that affords a subdivision into hundredth parts. Estimation by the eye looking through the short telescopes T and T, gives the position of the arm to still greater nicety. The lamps L and L throw light on the divisions by convex glasses, and no other light is admitted into the room.

EK is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *g l* is fastened, and thereby enables the observer to turn round the wire till the arm settles in the middle of the case without danger of touching either side. The wire *g l* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws. The different parts are drawn nearly in the proper proportion to each other.

Suppose the arm to be at rest and its position to be observed, let the weights be then moved; the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion were destroyed, or the point of rest. To do this, Mr. Cavendish observes three successive extreme points of vibration, and takes the mean between the first and third of these points as the extreme point of vibration in one direction, and then assumes the mean between this and the second extreme as the point of rest; for, as the vibrations are continually diminishing, it is evident that the mean between two extreme points will not give the true point of rest.

It may, he adds, be thought more exact to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that, notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason it is best to determine the point of rest from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which he finds in this manner: He observes the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as he can guess, that these divisions shall be on different sides of the middle point, and not very far from it. He then completes the middle point of the vibration, and by proportion finds the time at which the arm comes to this middle point. He then, after a number of vibrations, repeats this operation, and divides the interval of time between the coming of the arm to these two middle points by the number of vibrations, which gives the time of one vibration. The propriety of this method is more fully explained in the Memoir itself.

In Mr. Cavendish's first experiments the wire by which the arm was suspended was  $39\frac{3}{4}$  inches long. It was of copper silvered, one foot of which weighed  $2\frac{4}{10}$ ths grains; and its stiffness was such as to make the arm perform one vibration in about 15 minutes. It was immediately

immediately found that it was not stiff enough, as the attraction of the weights drew the balls so much aside as to make them touch the sides of the case. It was thought proper, however, to make some experiments before a stiffer wire was substituted in its place.

In this trial the rods by which the leaden weights were suspended were of iron, the probability of magnetism in which seemed to be of no consequence, as the arm contained nothing magnetical. Some trials however, for greater security, were made with the rods alone. The effect was such as might justify a very slight suspicion of magnetism; for which reason the rods were changed for others of copper. When the weights were hung on, the attraction on the ball was very sensible, as it carried the latter through fifteen divisions, or  $1\frac{1}{2}$  inch; and the regularity of the results was such, that the extremes did not differ from each other more than one-tenth part. It was found that the effect of the attraction seemed to increase for half an hour or an hour after the motion of the weights; an effect which might be supposed to arise from a want of elasticity, either in the suspended wire or something it was fastened to, which might make it yield more to a given pressure, after a long continuance of that pressure, than it did at first. This suspicion was put to the trial by keeping the wire for two or three hours in a state of torsion amounting to fifteen divisions, and then returning it by means of the handle K to its original situation. It was not found by two repetitions of the experiment that the wire had acquired any set or permanent twist. The arm was then suspended by a stiffer wire, with which the apparatus, instead of performing its vibration in about fifteen minutes, employed only seven. The motion of the arm was in this case not quite six divisions between the two opposite positions of the weights, or the deviation from the natural station of the arm was half that quantity.

A variation of the power of the weights upon the balls, in some respect similar to that observed with the small wire, was also found to take place in the present experiments. It happened that the case in which the arm was enclosed was placed nearly parallel to the magnetic east and west; and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth—and the weights also, after remaining for some time in either of the near positions, would acquire a polarity of the same kind, and attract the balls more strongly on that account; and on the contrary, when removed to the opposite near position, if the magnetism continued permanent in the weights for sufficient time, the magnetic power would operate in the way of repulsion. This was in fact the kind of effect pointed out by the observations. The accurate habits which so eminently distinguish all the experimental processes of Mr. Cavendish did not however permit him to rest contented with the induction to which these facts seemed to point. He altered his apparatus so as to produce at pleasure a rotation in the weights upon their vertical axes, without opening the room or disturbing their actual situation. By this contrivance he had it in his power to dispose the imaginary magnetic poles in such positions as were best calculated to detect their operation. The arm was not affected; and consequently it follows that the irregularity in question did not arise from magnetism. This conclusion was still more firmly established by substituting two actual magnetic bars in the place of the weights; for it was not observed that any alteration was produced by reversing the position of their poles.

The next circumstance that suggested itself was, that the effect might be owing to a difference of temperature between the weights and the case; for it is evident, that if the  
weights

weights were much warmer than the case, they would warm that side which was next to them, and produce an ascending current of air, which would be accompanied by a descending current towards the opposite side, and two horizontal currents near the top and bottom; the latter of which would impel the ball towards the weight. Though it seemed scarcely probable that the weights should happen to be warmer than the case, and that in a sufficient degree to afford any perceptible consequence, our author nevertheless determined to submit the matter to examination. He therefore placed two lamps under the weights while in the mid-way position, and placed a thermometer with its ball close to the outside of the case, near that part which one of the weights would approach when brought into the position to exert its attraction upon the ball. The door of the room was then shut, and some time afterwards the weights were moved to this last-mentioned position. At first the arm was drawn aside only in its usual manner; but in half an hour the effect was so much increased that the arm was drawn fourteen divisions aside instead of about three, as it would otherwise have been; and the thermometer was raised near  $1\frac{1}{2}^{\circ}$ , namely from  $61^{\circ}$  to  $62\frac{1}{2}^{\circ}$ . On opening the door, the weights were found to be no more heated than just to prevent their feeling cool to the hand.

From the great effect of a difference of temperature it became desirable to ascertain the temperature of the weight itself, and of the air near the case, in the subsequent experiments. A small thermometer was therefore inserted in one of the balls, and another placed close to the case. A number of experiments were then made, in which the differences of temperature between the weights and the external air were observed; the weights being in some instances heated by a lamp, and in others cooled by means of ice. It was found that a higher temperature in the weight occasioned a greater deviation or apparent attraction of the ball; and, on the contrary, that a lower temperature caused the ball to be driven by a current in the opposite direction.

After this minute and scientific investigation of the causes which are capable of producing error in the results, Mr. Cavendish proceeded to make his fundamental experiments. These are tabulated at length in the memoir itself, and followed by a detail of the method of computing the density of the earth. The first thing to be done is to determine from the time of a vibration, what force is required to draw the arm aside; and the next, to find the proportion which the attraction of the weight upon the ball bears to that of the earth upon the same ball, supposing the ball to be placed in the middle of the case: and from these results (since the diameter and specific gravity of the weights and balls compared with water, and the diameter of the earth, are also known; together with the facts that the quantities of matter are as the attractive forces at like distances, and vary in the inverse duplicate ratio of the distance) the mean density of the earth itself may also be found. Several corrections requisite to be made in this density are then stated; namely, on account of the resistance of the arm to motion, the attraction of the weights on the arm, and on the remote ball; the attraction of the copper rods, and of the case itself; and for the alteration of the attraction of the weights upon the balls according to the position of the arm; which last is of more signification than all the rest. For all which I must, for the sake of brevity, refer to the Transactions.



The following TABLE contains the Results of the Experiments.

Experiment	Mot. weight.	Mot. arm.	Do. corr.	Time vibr.	Do. corr.	Density.
1.	m to +	14,32	13,42	1 "	—	5,5
	+ to m	14,1	13,17	14,55	—	5,61
2.	m to +	15,87	14,69	—	—	4,88
	+ to m	15,45	14,14	14,42	—	5,07
3.	+ to m	15,22	13,56	14,39	—	5,26
	m to +	14,5	13,28	14,54	—	5,55
4.	m to +	3,1	2,95	—	6,54	5,36
	+ to —	6,18	—	7,1	—	5,29
	— to +	5,92	—	7,3	—	5,58
5.	+ to —	5,9	—	7,5	—	5,65
	— to +	5,98	—	7,5	—	5,57
6.	m to —	3,03	2,9	} by mean.	6,57	5,53
	— to +	5,9	5,71			5,62
7.	m to —	3,15	3,03			5,29
	— to +	6,1	5,9			5,44
8.	m to —	3,13	3,00			5,34
	— to +	5,72	5,54			5,79
9.	+ to —	6,32	—	6,58	—	5,1
10.	+ to —	6,15	—	6,59	—	5,27
11.	+ to —	6,07	—	7,1	—	5,39
12.	— to +	6,09	—	7,3	—	5,42
13.	— to +	6,12	—	7,6	—	5,47
	+ to —	5,97	—	7,7	—	5,63
14.	— to +	6,27	—	7,6	—	5,34
	+ to —	6,13	—	7,6	—	5,46
15.	— to +	6,34	—	7,7	—	5,3
16.	— to +	6,1	—	7,16	—	5,75
17.	— to +	5,78	—	7,2	—	5,68
	+ to —	5,64	—	7,3	—	5,85

In the second column the letter m denotes the middle position of the weights when each is equidistant from the balls, + denotes one of the near positions, and — the opposite near position.

I copy

I copy Mr. Cavendish's concluding remarks in his own words:

"From this table it appears, that though the experiments agree pretty well together, yet the difference between them both in the quantity of motion of the arm and in the time of vibration is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference.

"By a mean of the experiments made with the wire first used, the density of the earth comes out 5,48 times greater than that of water; and by a mean of those made with the latter wire it comes out the same; and the extreme difference of the results of the twenty-three observations made with this wire is only ,75; so that the extreme results do not differ from the mean by more than ,38, or  $\frac{1}{4}$ th of the whole, and therefore the density should seem to be determined hereby to great exactness. It indeed may be objected, that as the result appears to be influenced by the current of air or some other cause, the laws of which we are not well acquainted with, this cause may perhaps act always or commonly in the same direction, and thereby make a considerable error in the result. But yet, as the experiments were tried in various weathers and with considerable variety in the difference of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems very unlikely that this cause should act so uniformly in the same way as to make the error of the mean result nearly equal to the difference between this and the extreme; and therefore it seems very unlikely that the density of the earth should differ from 5,48 by so much as  $\frac{1}{4}$ th of the whole.

"Another objection perhaps may be made to these experiments, namely, that it is uncertain whether in these small distances the force of gravity follows exactly the same law as in greater distances. There is no reason however to think that any irregularity of this kind takes place until the bodies come within the action of what is called the attraction of cohesion, and which seems to extend only to very minute distances. With a view to see whether the result could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to rest as close to the sides of the case as they could; but there is no difference to be depended on between the results under that circumstance and when the balls are placed in any other part of the case.

"According to the experiments made by Dr. Maskelyne on the attraction of the hill Schellien, the density of the earth is  $4\frac{1}{2}$  times that of water; which differs rather more from the preceding determination than I should have expected. But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantity I cannot measure."

## VI.

*An Inquiry concerning the Chemical Properties that have been attributed to Light. By*  
*BENJAMIN, Count of Rumford, F. R. S. M. R. I. A.*

(Concluded from page 405.)

HAVING been so successful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moist way*, I lost no time in making similar experiments with the oxide of silver.

*Experiment No. 11.* A solution of fine silver, in strong nitrous acid, was evaporated by dryness, and the residuum re-dissolved in distilled water.

A portion of this solution (which was perfectly colourless), diluted with twice as much distilled water, was poured into a phial containing a number of small pieces of charcoal; and the phial, being well closed with a new cork stopple, was exposed to the action of the sun's rays.

In less than an hour small specks of revived silver began to make their appearance on the surface of the charcoal; and, at the end of two hours, these specks became very numerous, and had increased so much in size, that they were distinctly visible to the naked eye at the distance of more than three feet. They were very white, and possessed the metallic splendour of silver in so high a degree, that when enlightened by the sun's beams their lustre was nearly equal to that of very small diamonds.

The phial, which was in the form of a pear, and about  $1\frac{1}{2}$  inch in diameter at its bulb, was very thin, and made of very fine colourless glass; the aqueous solution was also perfectly transparent and colourless; and, when the contents of the phial were illuminated by the direct rays of a bright sun, the contrast of the white colour of these little metallic spangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and interesting sight.

As the air had been previously expelled from the charcoal by boiling it in distilled water, it was specifically heavier than the aqueous solution of the metallic oxide, and consequently remained at the bottom of the bottle.

*Experiment No. 12.* A phial as nearly as possible like that used in the last experiment, and containing the same quantity of diluted aqueous solution of nitrate of silver, and also of charcoal, was inclosed in a cylindrical tin box, and exposed one hour to the heat of boiling water in an apparatus used for boiling potatoes in steam for the table.

The result of this experiment was uncommonly striking: the surface of the charcoal was covered with a most beautiful metallic vegetation; small filaments of revived silver, resembling fine flattened silver wire, pushing out from its surface in all directions!

Some of these metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were easily detached from the surface of the charcoal, to which they seemed to adhere but very slightly.

These experiments were repeated several times, and always with precisely the same results.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of small scales, adhering firmly to the surface of the charcoal. May not the difference

of the forms under which gold and silver are revived from their oxides, in this process, be owing to the difference of the specific gravities of those metals?

The following experiments, which were first suggested by an accident, were made with a view to investigate still farther the causes of those effects which have been attributed to the supposed chemical properties of light.

Having accidentally put away two small phials, each containing a quantity of aqueous solution of the oxide of gold and sulphuric ether, in each of which the ether had extracted the gold completely from the solution, as was evident by the yellow colour of the solution having been transferred to the ether, and the solution being left colourless; in one of the phials, which happened to stand in a window in which there was occasionally a strong light (though the direct rays of the sun never fell on it), I found, in about three weeks, that the oxide was almost entirely reduced; the revived gold appearing in all its metallic splendour in the form of a thin pellicle, swimming on the surface of the aqueous liquor in the phial, and the colour of the ether which reposed on it having become quite faint; while no visible change had been produced in the contents of the other phial, which had stood in a dark corner of the room.

As these appearances induced me to suspect, or rather strengthened the suspicions I had before conceived, that the separation of gold from ether, under its metallic form, when a solution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther investigation of that matter.

*Experiment No. 13.* Into a small pear-like phial of very fine transparent glass I put equal quantities of an aqueous solution of the muriatic oxide of gold and sulphuric ether; and the phial, which was about half filled, being closed with a good cork well secured in its place, was exposed to the action of the direct rays of a bright sun.

A pellicle of revived gold, in all its metallic splendour, began almost immediately to be formed on the surface of the aqueous liquid, and soon covered it entirely; and at the end of two hours the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourless*.

On shaking the phial, the metallic pellicle, which covered the surface of the aqueous liquid, was broken into small pieces, which had exactly the appearance of leaf-gold, possessing the true colour and all the metallic brilliancy of that metal.

On suffering the phial to stand quiet, the aqueous liquor and the ether separated, and most of the broken pieces of the thin sheet of gold descended to the bottom of the phial: the remainder of them floated on the surface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly transparent and *colourless*.

By the length of time which was required for the ether and the aqueous liquid to separate, I thought I could perceive that the ether had lost something of its fluidity; but as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I saw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been suffered to cool, there was no appearance of any considerable quantity of air, or other permanently elastic fluid, having been either generated or absorbed during the experiment.

Finding that the oxide of gold might be so completely and so expeditiously reduced by  
means

means of ether, I conceived it might be possible to perform that chemical process, *in the moist way*, by means of essential oils; and this conjecture proved to be well founded.

*Experiment No. 14.* Upon a quantity of a diluted aqueous solution of nitro-muriate of gold, in a small pear-like phial, about  $1\frac{1}{2}$  inch in diameter at its bulb, was poured a small quantity of etherial oil of turpentine, just as much as was sufficient to cover the aqueous solution to the height of  $\frac{2}{3}$  of an inch; and the phial, being well closed with a good cork, well secured, was exposed one hour to the heat of boiling water in a steam-vessel.

The gold was revived, appearing in the form of a splendid pellicle, of a bright gold colour, which floated on the surface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourless as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it reposed, had entirely lost its yellow colour.

On shaking the phial, its contents were intimately mixed; but, on suffering it to stand quiet, the oil of turpentine soon separated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourless.

On shaking the phial, *before it had been exposed to the heat*, and mixing its contents, and then suffering it to stand quiet, the oil of turpentine, on taking its place at the top of the aqueous solution, was not found to have acquired any colour; nor was the bright gold colour of the solution found to be at all impaired. When sulphuric ether was used instead of the oil of turpentine, the effect was in this respect very different.

To find out whether the oil of turpentine used in this experiment, and which had acquired a deep yellow colour, had lost that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous solution of the oxide into the phial, and, shaking the phial, exposed it with its contents to the heat of boiling water.

After it had been exposed to this heat about two hours I examined it, and found, that though a considerable quantity of gold had been revived, yet the aqueous liquid still retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial I now added about half as much distilled water, and, mixing the whole by shaking, I exposed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly colourless.

On repeating this experiment with oil of turpentine, and varying it, by using a solution of the oxide of *silver* (an aqueous solution of nitrate of silver,) instead of that of *gold*, the result was nearly the same: the metal was revived, and the oil of turpentine acquired a faint greenish-yellow colour.

I also revived the oxides of gold and of silver with *oil of olives*, by a similar process, with the heat of boiling water. The oil of olives used in these experiments lost its transparency, and became deeply coloured: that used in the reduction of the oxide of silver taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold being changed to a yellowish-brown, with a purple hue.

In the experiment with the oxide of silver, the inside of the phial, in the region where the oil reposed on the aqueous solution, was beautifully silvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, small detached pellicles of revived metal were visible in the oil, and adhered in several places to the inside of the phial, forming bright spots, in which the colour of the metal and its peculiar splendour were perfectly conspicuous.

*Experiment No. 15.* As carbon is one of the constituent principles of spirit of wine, as well as of essential oils and sulphuric ether, I thought it possible that I might succeed in the reduction of the oxide of gold, by mixing alcohol with an aqueous solution of nitromuriate of gold, and exposing the mixture in a phial well closed to the heat of boiling water; but the experiment did not succeed.

By pouring upon this mixture a small quantity of oil of olives, and exposing it again to the heat of boiling water, the gold was revived.

Is it not probable that the reason why the oxide was not reduced by alcohol, is the mobility of those elements, which ought to act on each other, in order that the effect in question may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to rest on the surface of the aqueous solution without mixing with it.

I wished to have been able to have collected and examined the elastic fluids, which probably were formed in most of the preceding experiments; but my time was so much taken up with other matters that I had not leisure to pursue these investigations farther.

In order to see what effects would be produced by the heat generated at the surface of an opaque body, of a nature different from those hitherto used in the reduction of the metallic oxides, and one that is little disposed to form a chemical union with oxygen, (*magnesia alba*) when, being immersed in an aqueous solution of the oxide of gold, the rays of the sun were made to impinge on it, I contrived the following experiment.

*Experiment No. 16.* I took four small thin phials, A, B, C, and D, of very fine glass, and putting into each of them about five grains of dry *magnesia alba*, I filled the phial A, nearly full, with a saturated aqueous solution of the oxide of gold.

I filled the phial B, in like manner, with some of the same solution, diluted with an equal quantity of distilled water; and the phials C and D were filled with the solution still farther diluted.

These phials, open or without stoppers, were exposed one whole day to the action of the direct rays of a bright sun, their contents being often well mixed together during that time by shaking.

The contents of all these phials changed colour more or less, but they acquired very different hues. The contents of the phial A became of a very deep rich gold colour, approaching to orange, the earthy sediment being throughout of the same tint.

The contents of the phial B, which were at first of a light straw colour, first changed to a light green, and then to a greenish blue. The phial having been suffered to stand quiet several days, in an uninhabited room in a retired part of the house, the solution became nearly colourless, and the sediment was found to be of a dirty olive colour.

The

The colour of the contents of the phials C and D was changed nearly in the same manner; and having been suffered to stand quiet two or three days to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.

## VII.

*Some Account of the Persian Cotton-Tree. By MATTHEW GUTHRIE, M.D. F.R.S. &c. &c.\**

COTTON is a plant of both the old and the new world; at least it is found wild in both: but I have my doubts whether it was a native of America before the Europeans carried it over, and shall assign reasons for my incredulity when I come to treat of the *Persian cotton*, which is the very species that is said to be American.

Five species of the cotton-tree are enumerated by Linnæus; and there is reason to suspect the existence of a sixth, if what we are told of the extreme fineness and silky nature of a particular kind reared in some of the Antilles be literally true. This curious variety is called Siam cotton, because the reed was originally obtained from Siam.

The first species of cotton is the *gossypium arboreum*, or *Indian cotton-tree*, which has been cultivated and manufactured in the East Indies from the remotest period of the authentic history of that country, or between three and four thousand years. It delights in a sandy soil.

The second species is the *gossypium religiosum*, which is likewise a native of India, and a tree, or at least a high shrub; but why Linnæus dignified it with so singular a specific name I shall leave the learned Asiatic Society in Bengal to determine, as they must know if it be used for any religious purposes by the Bramins. This species of cotton is said to be that which is cultivated by the French in Martinico.

The third is the *G. barbadense*, a species of biennial cotton shrub cultivated in our British island of Barbadoes, from which it obtains its specific name. I believe it is likewise the same species which is cultivated in Jamaica.

The fourth is the *G. hirsutum*, an American perennial cotton shrub in the warmer provinces, but annual in the colder, as is sometimes the case with plants in climates where their roots lose their vegetating power by winter frost.

The fifth and last species is the *G. herbaceum*, or *G. annuum*, an annual cotton plant, which rises to the height of three or four feet, and is sown and reaped, like corn, twice a

\* Manchester Memoirs, Vol. V. Part I.

year in hot countries, and once a year in colder climates. It bears a large yellow flower with a purple centre, and fruit about the size of a walnut containing the cotton.

This is the famous *Persian cotton* properly the subject of the paper, although a slight mention of other species was necessary to give a more complete view of the subject. Linnæus calls it a native of America; and there is no doubt but that it is become so, although there is much more reason to suppose America naturalized a Persian plant than that Persia got it from the new world; especially if we are to credit a paper lately presented by a British merchant to the Economical Society at Petersburg, in which it is positively asserted that several of the European nations furnished their American colonies with *Persian cotton* seed procured at Smyrna. Now this fact (if sufficiently authentic, which I do not doubt from my knowledge of the veracity of the author) will easily account for the *G. herbaceum* being found wild in America; when we recollect the wonderful provision of nature for the wide dispersion of seeds, and Linnæus's assertion that the *Erigeron canadense* was dispersed from the botanic garden of Paris by the winds over a great part of Europe, and several other plants\* from the botanic garden of Upsal over a whole province.

My reasons for suggesting these doubts relative to the native country of this species of cotton are, that all vegetables of this genus are supposed to have been indigenous in Persia exclusively, and that even the East Indies derived the cotton plants from thence; a conjecture which seems to have acquired some degree of credit from the late discovery of Sir William Jones, viz. that the Hindoos, or inhabitants of India, were originally a colony of the ancient Iran or Persia, which seems to have been the cradle of the human species, since its ancient language appears to have been the mother of all those now existing (with the exception of the Arabic and Tartarian), of which nevertheless it contained many words.

Now it is very possible that the first colony carried the cotton plant with them to India, and that it was afterwards dispersed from Hindostan to the adjacent countries and islands. The cotton plant is widely dispersed likewise throughout Europe and some parts of Africa, particularly the annual or herbaceous species (the very plant treated of here) reared in the north of Persia, and which is also cultivated in Malta †, Sicily, Chio, Lemnos, and other islands of the Archipelago, although possibly the cotton of these islands may be varieties of the species from difference of soil, climate, &c.

The best of the European cotton is brought from Cyprus; but Smyrna, Aleppo, Damascus, Jerusalem, &c. furnish likewise a quantity of cotton at least equal to the European.

#### *Cultivation of Cotton in Persia.*

THE annual cotton, or this last species, of which we have treated more amply, is much cultivated in the northern or colder provinces of Persia bordering on the Caspian Sea (as the perennial is in the southern); and it is from thence that the seeds now sent to Portugal have been obtained through the Bucharian Tartar merchants, and are the production of the *Gossypium herbaceum* of Linnæus, the *Gossypium annuum* of Pallas. It is sown in Persia from the end of March to the end of April, and reaped in September. This species requires a rich soil mixed with sand; and therefore where the land is not rich enough they

\* The *Antirrhinum minus*, the *Datura stramonium*, the *Gnaphalium americanum*, &c.

† There is a kind of cotton cultivated in Malta, of a nankcen colour, which exceeds in fineness all other cotton, and is much superior even to that from the Antilles.



manure it with cow or sheep dung ; although we are told that when the plants are once raised above the ground any species of soil will answer. The ground is worked in the spring, and the seeds are planted at the distance of eight or ten inches from one another, whilst care is taken to weed it, to give air to the young plants. Dry summers give the best crop, as rain is more particularly hurtful when it falls in great quantities during the flowering and ripening of the cotton. It is gathered, as said above, in September, care being always taken to collect a sufficient quantity of seed for the next year. Lastly, watering the young plants with a mixture of wood-ashes and water in certain situations is sometimes necessary to guard them from destructive worms.

The Russians have cultivated the same species of Persian cotton in the government of Caucasus, and rear enough of it to serve their own national manufactures, which are not as yet either numerous or considerable ; but on the Terck, at the foot of the Caucasus, where it is reared, they do not sow till the middle of May, lest a late spring frost, which is sometimes felt in those parts, should destroy the hopes of the planter. With that one exception, the Russians strictly observe the Persian mode of cultivation.

There is a species of silky cotton much cultivated at present in Germany, which possibly may merit the attention of Portugal for their plantations in America. It is the *Asclepias syriaca* of Linnæus, and affords so fine a species of cotton (if I may so name it) that fabrics have been erected in Saxony, where stuffs are made of it which rival in lustre, &c. the true animal silk. But this new vegetable silk has circumstances attending it that seem to recommend its cultivation in some of the American colonies and islands : First, because it is originally the native of a hot climate, as Linnæus's specific name indicates ; and of course it is likely to be in its greatest beauty and excellence in climates which approach nearest to that of its native country. Secondly, because its stalks afford a coarse sort of cloth well calculated to clothe negroes, whilst from the pith of them paper is made.

## VIII.

*Facts and Observations concerning the Measure and Expence of first Movers, namely, Wind, Water, Steam, and Animal Strength, and on other Objects of general Utility. (W. N.)*

THE consideration of the value and importance of natural first movers is of consequence not only to practical engineers, but to every individual in cultivated society. There are numberless situations, even in the spirited manufacturing kingdom we inhabit, where large revenues are expended to perform mechanical and hydraulic operations by the force of human labour, or by horses, which might be for the most part saved by substituting a steam-engine or windmill, or making use of a stream of water now running to waste. It is well known, that, since the extension of the cotton and other works, estates of small rent in the neighbourhood of Manchester and elsewhere have been, and continue to be, let at more than twenty times their original rent, merely from the fortunate circumstance of their possessing a small stream of water falling with a sufficient declivity to give motion to a mill. If the proprietors of lands and manufacturers in general were better acquainted with the simple methods of estimating the forces of those currents of water which run neglected through

through their grounds and premises, and which an intelligent observer need not walk many miles in any country to point out, their property and revenue might immediately receive a considerable accession; and the community would be still more effectually benefited. The incessant demands for the employment of such forces in grinding corn, colours, drugs, tobacco; in cutting bark and other tanners' and dyers' materials; in sawing wood; in laminating, drawing, or fashioning metallic bodies; in spinning, weaving, fulling, &c. the products of the organized kingdoms by arts already practised, exclusive of the many improvements which may be expected in their application, are too numerous to afford the least reason for any proprietor to fear a want of employment, or to consider the erection of a mill in a proper situation as a speculation of the least danger or probable disadvantage. Similar observations are to a certain extent applicable in favour of the use of horses instead of men, and steam instead of horses, in every case where the power is required to be great or long continued, and the skill either little, or capable of being supplied by machinery.

I am tempted to digress for a moment from my subject by the natural recurrence of a political reflection, so obvious that it scarcely ever fails to be made when the extension of machinery and the application of inanimate powers are considered. It is stated by certain humane but mistaken objectors, that the scheme of mechanical and chemical improvement is pointed against the human species, and tends to drive them out of the system of beneficial employment. Two creatures offer themselves to me for employment and support—a man and a horse. I must invariably prefer the latter, and leave the former to starve. Two other beings—a horse and a steam-engine, are candidates for my favour. My preference to the latter tends to exterminate the species of the former. In both cases it is stated, that the number of intelligent creatures capable of the enjoyment of happiness must be diminished for want of support; and that, on the whole, the sum of the proposed improvements is not only a less proportion of good to society, but a positive accession of much misery to the unemployed poor.

On this wide and extended argument, which can in fact be maintained against improvements in no other way than by insisting that the savage state, with all its wants, its ignorance, its ferocity, and its privations, is preferable to the social intercourse of effort and division of labour we are habituated to prefer, it may be sufficient to observe, that the topic includes matter not only for reasoning and induction, but also for experiment. By reference to the matter of fact, though it must be allowed that new improvements, which change the habits of the poor, must at first expose them to temporary inconvenience and distress, against which, in fairness, it is the duty of society to defend them; yet the invariable result of such improvements is to better the condition of mankind. The nations which have shewn the most ingenuity and industry in this way are not only the richest, the most populous, the most intelligent, and the best defended; but the provinces of those nations are seen to flourish in proportion to their respective degrees of activity in this respect. And from these exertions it is, as Smith\* emphatically remarks, that “the accommodation of an European prince does not always so much exceed that of an industrious and frugal peasant, as the accommodation of the latter exceeds that of many an African king, the absolute master of the lives and liberties of ten thousand naked savages.”

\* *Wealth of Nations*, i. ch. i.

But to return to our subject. In this practical and popular communication I shall avoid entering into any discussion of the theory of windmills, which has employed the attention of so many eminent men for a century past. The advantages of these engines for such work as will admit of being performed and suspended by intervals are sufficiently known, but perhaps not sufficiently attended to. There can be no doubt but that the Dutch, who use windmills for sawing, pumping, and various other works, as well as grinding, must have found them very profitable, since their country is overspread with them. It may be proper however to take notice, that many writers have copied one from another the determination of Maclaurin, that the best angle for windmill-fails to make with the line of direction of the wind is  $54^{\circ} 44'$ , which is only true, as that excellent mathematician observes, at the very commencement of the motion, and requires to be enlarged as the velocity of the fails increases: for the law of which, and other essential objects, his account of Sir Isaac Newton's Philosophical Discoveries, and his Treatise on Fluxions, may be consulted. Smeaton, who had much experience in the business of a civil engineer, and whose data may always be depended on, though his theories are not constantly accurate, made a set of experiments on the construction and effects of windmill-fails, which are described in the Philosophical Transactions for the year 1759\*. This engineer states, that the mean rate of work for mills with Dutch fails is when they make about thirteen turns in a minute, which is when the velocity of the wind is  $8\frac{2}{3}$  miles in an hour, or  $12\frac{2}{3}$  feet in a second: and this wind in common phrase would be called a fresh gale. Taking the maximum of Defaguliers, hereafter to be mentioned, as his standard for computation, he deduces the size of a windmill-fail of the figure just mentioned, and also according to a figure constructed from his own experiments, which shall be equal in mean power to one man; and thence he arrives at the inference, that one of his own fails, thirty feet in length, will, when working at a mean rate, be equal to the power of 18,3 men. He had an opportunity of verifying this in the large way in a mill used for crushing rape-seed. The mean power of a windmill is therefore very considerable; but what may be the annual or average quantity of work such an apparatus is capable of performing under all the vicissitudes of the wind, I possess no means of ascertaining.

In the same treatise Smeaton makes several very just remarks on those windmills which are acted upon by the direct impulse of the wind against fails fixed to a vertical shaft. His objections have, I believe, in every instance been justified by the inferior efficacy of these mills when compared with the charges of erecting them. He also maintains that water-mills with oblique fails, upon the principle of the common windmill, cannot prove beneficial to the undertaker. It is indeed probable that most of the circumstances of running-water are likely to render the common over and undershot-wheels cheaper and more effectual, and that the oblique float-board will in no instance come near the effect of a close overshot wheel. But it is also certain that such wheels are used in China, in the south of France, and elsewhere, with much more effect than Smeaton appears disposed to think them capable of: and at all events, the subject of these wheels deserves to be considered.

\* This account has since been republished, together with other valuable papers of the same author, under the title of An Experimental Inquiry concerning the Natural Powers of Wind and Water to turn Mills, &c. By John Smeaton, F.R.S. 8vo, printed for Taylor, London, 1794.

The enquiry which most immediately interests land-holders, and others who have the advantage of a current of water, is to ascertain whether it will afford sufficient power to justify the erection of a mill, and what that power may be. If the stream be ample without much fall, it must necessarily be applied to move an undershot wheel by its impulse, and the power will be determinable from the velocity of the water, and the quantity which passes through the section of its bed. One of the easiest methods of ascertaining these data is that given by Desaguliers \*, as follows. Observe a place where the banks of the river are steep and parallel, so as to make a kind of trough for the water to run through, and by taking the depth across make a true section of the river. Stretch a string at right angles over it, and at a small distance another parallel to the first. Then take an apple, an orange, or other small ball, just so much lighter than water as to swim in it, and throw it into the water above the strings. Observe when it comes under the first string by means of an half-second pendulum, a stop-watch, or any other proper instrument, and likewise when it arrives at the second string. By this means the velocity of the upper surface, which in practice may generally be taken for that of the whole, will be obtained. The section of the river at the second string must be ascertained by taking the depth as before. If this surface or section be the same as the former, it may be taken for the mean section; if not, add both together, and take half the sum for the mean section. The area of the mean section in square feet being then multiplied by the distance between the strings in feet, will give the contents of the water in solid feet, which passed from one string to the other during the time of observation. And this, by the rule of three, may be adapted to any other portion of time. Suppose, for example, the time had been 12'', and the hourly expenditure of water were required, the proportion would be: As 12'' are to 3600'', so is the number of cubic feet observed to the hourly expenditure in cubic feet. If the mere velocity be required in proportion to any fixed interval of time, the same proportion will give it, provided, instead of the solid contents in the third term, there be taken the distance between string and string.

The intelligent observer may in general abridge this operation, by taking notice of the arrival of the floating body opposite two stations on the shore, especially when it is not convenient to stretch a string across. The arch of a bridge is a good station for an experiment of this kind, because it affords a very regular section and two fixed points of observation: and in some instances the sea practice of heaving the log may have its advantages. Where a time-piece is not at hand it may be equally convenient, provided two observers attend, to note the time with a half or quarter-second pendulum. The half-seconds pendulum is made by suspending a small round button, or other spherical weight, by a thread looped over a pin of such a length that the distance from the bend of the loop to the centre of the weight shall be 9,8 inches. The quarter-seconds pendulum is one fourth of this length. If, by observations at several stations above and below any particular point of the river, the velocity is not found to vary, the section of the river in all that space may be concluded to be uniform; and it will not be necessary to determine more than one section by actual measurement.

In the case of an overflowing pond, or small stream, which will admit of a dam across it,

\* Course of Experimental Philosophy, ii. 419.

the quantity of water afforded may be ascertained by suffering it to run through a notch in a board, or a vertical hole of an inch square, according to the following table, which Defaguliers says he calculated from repeated experiments\*.

*A TABLE of the Expence of Water through an Inch-square Hole, and through a Cut an Inch wide and of different Depths.*

Inches below the surface.	Expence of water through a hole of a square inch at different depths, according to the number of inches in the first column.				Expence of water through a notch of different depths, according to the number of inches in the first column			
	Tuns in an Hour.							
1	-	-	-	1,04	-	-	-	1,04
2	-	-	-	1,46	-	-	-	2,50
3	-	-	-	1,79	-	-	-	4,29
4	-	-	-	2,08	-	-	-	6,37
5	-	-	-	2,31	-	-	-	8,68
6	-	-	-	2,53	-	-	-	11,21
7	-	-	-	2,74	-	-	-	13,95
8	-	-	-	2,92	-	-	-	16,87
9	-	-	-	3,12	-	-	-	19,19
10	-	-	-	3,28	-	-	-	22,47
11	-	-	-	3,44	-	-	-	25,81
12	-	-	-	3,58	-	-	-	29,39
13	-	-	-	3,74	-	-	-	33,13
14	-	-	-	3,88	-	-	-	36,91
15	-	-	-	3,95	-	-	-	40,86
16	-	-	-	4,16	-	-	-	45,02
17	-	-	-	4,28	-	-	-	49,30
18	-	-	-	4,40	-	-	-	53,70
19	-	-	-	4,52	-	-	-	58,22
20	-	-	-	4,62	-	-	-	62,84
21	-	-	-	4,76	-	-	-	67,60
22	-	-	-	4,87	-	-	-	72,47
23	-	-	-	4,94	-	-	-	77,41
24	-	-	-	5,06	-	-	-	82,47
25	-	-	-	5,2, or	-	-	-	87,67

5 tuns and 50,4 gallons.

A cubic foot of water weighs very nearly  $62\frac{1}{2}$  pounds averdupois, and an hoghead of water weighs about 550 pounds.

\* Course of Experimental Philosophy, ii. 129.

In the consideration of power or force to be derived from water in motion, the water may be taken as a determinate mass falling through a given height in a given time. In order that this descending weight may cause another weight to ascend, or may overcome some resistance in the way of work with that degree of speed which shall be the most profitable, it is necessary that the resistance or work to be done should be neither too great nor too little. If it be too great, the slowness of operation will diminish the quantity of work; and if it be too small, the speed will not sufficiently compensate for this smallness. When the power is therefore known, it remains to deduce what may be the effect. But in the first place, as the height from which the water flowing in a river may have descended, in order to acquire its velocity, is, from a variety of circumstances, difficult to be ascertained, and also very different from that height which would immediately and without impediment produce the same velocity, it becomes necessary to compute this last height, which hydrostatical writers usually call the height of the virtual head. These writers teach, that the velocity of a fluid spouting through an orifice in a thin plate is the same as would be acquired by a body falling in clear space from the height of the surface of the fluid above the orifice. Hence, from the common doctrine of falling bodies, if the uniform velocity of a stream be expressed in feet per second, the virtual height of the fall will be found by multiplying the given velocity into itself, and dividing the product by 64,2882; the quotient will be the required height expressed in feet \*.

The effect of undershot and overshot-wheels has been treated by various authors, who have given results extremely different from each other. Smeaton, in the *Treatise* often quoted in the course of this communication, observes, that Belidor in his *Architecture Hydraulique*, i. 286, endeavours to demonstrate that water applied undershot will do six times more execution than the same applied overshot; while Defaguliers, whom he (Smeaton) misquotes by overlooking the difference of fall, is said to have given the advantage as ten to one in favour of the overshot †. The particular experiments of Smeaton himself, as well as his experience, point out the following results.

The effect in undershot-mills in the large way is at best one third of the power; that is to say, the wheel, being driven with two-fifths of the velocity of the stream, will raise a quantity of water equal to one-third of the column, which strikes the float-boards, to an height equal to that of the virtual head or fall: and the effect of an overshot-wheel will be, at a medium, twice that of the undershot. Mills having a breast-wheel, or other kind of wheel on which the water acts, partly by its weight and partly its impulse, will produce more or less effect, accordingly as the circumstances approach more nearly to those of the over or undershot-wheels.

For the advantage of such as are least conversant in subjects of this nature, for whom chiefly the present memoir is intended, I shall illustrate the subject by an example.

Suppose a stream to pass through an estate without any evident fall, with a velocity of nine feet per second, and affording sufficient room to place an undershot-wheel with a pro-

\* The proof of this is simple, but may also be seen in Defaguliers, ii. 520.—Those who use logarithms may with less trouble subtract the constant log. 1.8681312 from twice the log. of the velocity, and the remainder will be the log. of the virtual height.

† The proportion of work of the two actual mills, compared by Defaguliers, is as 3,25 to 1 in favour of the overshot.

per number of float-boards, each six feet long and two deep: It is required to determine, 1st, What quantity of water might be raised by that power, during the day of ten hours, to the height of thirty feet? or, 2dly, What number of bushels of wheat, or malt, might be ground in the same time? or, 3dly, What number of men or horses might this wheel supply the place of, in performing any other kind of work or manufacture?

In the first place, to find the virtual fall, multiply 9 feet by itself, which gives 81, and divide this by 64,2882: the quotient will be 1,26 feet.—Again, the surface of the float being 6 feet by 2 feet, is 12 square feet; which, multiplied by the velocity 9, gives 108 = the number of solid feet of water which has in effect fallen from the virtual head in a second. But this power will partly be consumed in producing eddies and lateral motions, and partly remain uncommunicated in the tail water as it flows off. For which reasons, as has already been remarked, the effect will only be one-third of the power. That is to say, one-third part of 108, or 36 cubical feet of water will be raised in a second to the height of 1,26 feet. The question however is directed to the height of thirty feet, and the quantities of water, or other weights, raised by equal powers, are inversely as their heights: that is, 30 feet : 1,26 feet :: 36 cubical feet : 1,512 cubical feet raised 30 feet in a second. But the hour consists of 3600 seconds, and the day of ten hours, = 36000, which multiplied by 1,512 produces 54432 cubic feet, or 6437 hogheads, (reckoning the gallon at 231 solid inches) which are raised 30 feet in the day\*.

With regard to the operation of grinding, it is allowed that about 3420 cubic feet of water with a fall of 10 feet will, by an overshot wheel, grind one bushel of wheat into flour, and the same force will cut five quarters of malt. But our under-shot wheel will only do half the work. From the foregoing calculations our stream is equivalent to 13,6 solid feet per second with a fall of ten feet, or 816,5 solid feet per minute. Therefore as 3420 cubic feet : one bushel of wheat :: 816,5 cubic feet : 0,239 of a bushel which would be ground per minute by an overshot wheel; or 0,1195 by our under-shot wheel. This last number multiplied by 60 produces 7,17 bushels per hour, or nearly 72 bushels, or 9 quarters of wheat per day of ten hours. And by the proportion of malt to wheat just mentioned, it will follow that 360 quarters of the former grain might be cut in the same time.

Animal strength is of so fluctuating a nature, that it is difficult to subject it to any estimate. Physical causes must affect both the quantity and duration of the efforts

\* This is an outside estimate, and gives no allowance for frictions and errors in the construction of the hydraulic work. The reaction in Smeaton's Experiments was afforded merely by a weight and pulley, which is greatly in favour of the mill in deducing the effect. I find in the "Reports of the late Mr. John Smeaton, F. R. S." printed in quarto, London 1797, the power of the water called "Hubbert's mill stream" is deduced from the quantity and fall, (p. 245.) by a rule which he does not mention, but is evidently this, for overshot or close breast wheels: *Multiply the number of cubic feet of water discharged per minute by the fall in feet: this number may be called the power. Divide the power by two, and the quotient may be called the effect. Assume any height at pleasure in feet: divide the effect by the height, and the quotient will be the cubic feet of water, which a good hydraulic apparatus will raise to that height per minute.*

The under-shot wheel will raise half that quantity: or in general less than one-sixth of the power:

I have retained the proportion of the Treatise on mills in the text, because the ratio of the effect to the power can be easily altered at pleasure in computing.

possible.

possible to be made either by man or beast, and the former is more particularly influenced by his moral habits. From these last it is that the influence of reward, the expectation of favour or patronage, and various other similar motives, have operated in the temporary exhibition of hydraulic machines, to produce results contrary to every sound deduction from permanent work, and most perniciously delusive to the parties concerned in supporting or encouraging such engines. Defaguliers\*, who has taken much pains to ascertain the maximum of power in this respect, has determined that a man can raise of water, or any other weight, about 550 lbs. or one hoghead ten feet high in a minute; and he states that a horse will raise five times that quantity; or, which is the same thing, that quantity through five times the height. His deduction respecting the man, though he says it will hold good for six hours, appears from his own facts to be too high, and certainly such as could not be maintained one day after another. Smeaton† considers this work as the effort of haste or distress. He reports‡ that six good English labourers will be required to raise 21141 solid feet of sea water to the height of four feet in four hours. This quantity is of the same weight as 21669 cubic feet of fresh water, with which, and the above rate, it will be found by an easy calculation that the men will raise a very little more than six cubic feet each to the height of ten feet in a minute. But the hoghead containing  $8\frac{1}{2}$  cubic feet, Smeaton's allowance of work proves less than that of Defaguliers in the proportion of 6 to  $8\frac{1}{2}$ . And as his good English labourers, who can work at this rate, are by him estimated to be equal to a double set of common men picked up at random; it seems very proper to state that, with the probabilities of voluntary interruption, and other incidents, a man's work for many days together ought not to be estimated at more than half a hoghead raised ten feet high in a minute. In the same report last quoted, Smeaton states, that two ordinary horses will do the work in three hours and twenty minutes, which amounts to a little more than two hogheads and a half § raised ten feet high in a minute. One horse will therefore do the work of five men.

To apply these deductions to our example, it must be recollected that the quantity raised in ten hours to the height of thirty feet was inferred to be 6437 hogheads, which are equivalent to 32 hogheads raised ten feet high per minute. Consequently at the rate of one man for each half hoghead, the stream would perform the work of 64 men, or nearly thirteen horses.

With regard to streams which fall by a considerable declivity, the water may be conveyed by the well known means of a dam or trough to the buckets of an overshot wheel, placed in that part of the stream which is found the most convenient in point of expence and local situation. Suppose, for example, the fall amounted upon the whole to fifteen feet, upon a length of 200 yards, it might be more convenient to lead the whole stream in a wooden trough supported upon posts, with a slight declivity, to the wheel near the lower end of the current; or in other circumstances, according to the face of the land, the work might prove cheaper or more durable if the wheel were placed near the upper end of the stream, and the channel sunk so as to convey away the tail water with no more fall than should be necessary for that purpose. In this case it is obvious that the fall must be ascer-

\* Course of Lectures, ii. 498, 505, 536.

† Reports, i. 216.

‡ Ibid. i. 323.

§ He elsewhere (ibid. p. 229.) rates an horse at 250 hogheads, ten feet high, in an hour; but I prefer the deduction in the text.



tained by the operation of levelling, and not by computation as in our former supposition. This operation is easy, and may be performed upon such short distances with sufficient accuracy, with a carpenter's square and a staff.

For this purpose it will be necessary to drive a pin into the handle, or thickest part of the square, near the corner, upon the flat side. A line must be drawn from the pin, parallel to the edge of this handle, and a looped thread supporting a small weight must be hung upon the pin. In this situation the instrument is ready for taking levels. For if it be held in such a position that the plumb line may cover the line drawn from the pin, the blade will lie horizontal, and by looking along its upper edge the eye will be directed to some object on the same level with itself. Let the observer provide himself with a staff five or six feet long; which, if divided into feet and inches, will be the more useful. This is to be pitched at or near the lower end of the stream, and against it the observer is to press the handle of his square, which may be conveniently done by grasping both it and the staff together in the same hand. Then, holding the apparatus so that the plumb line may lie fair, he is to direct the blade of the square to some stone or other remarkable object higher up the stream; for which purpose it will be necessary to slide the handle of the square up or down the staff till the positions are accurately obtained; that is to say, till the blade points fairly to the object at the same time that the plumb line denotes that the blade is level. When this is done, a memorandum must be taken of the feet and inches from the bottom of the staff to the upper edge of the blade, and the observer must proceed to the stone or object to which his sight was directed, and, planting his staff there, repeat the same operation with regard to some other object still higher up the stream, and again note down the feet and inches observed upon the staff. This process continued will, by a few reiterations, bring him to the upper extreme of the water which we suppose to be within his grounds or power. The sum of all the measures taken upon the staff is the difference of level or whole fall of the water. The quantity of water in cubic feet may be ascertained by either of the means before mentioned; or, if the stream be very small, it may actually be dammed, and measured off with two tubs or measures, of as large a size as can conveniently be managed.

I should be inclined to apologize to such of my readers as are intimately acquainted with these subjects, for the minuteness of detail into which I have entered, if I were not assured that they will be the first to perceive, that many individuals who possess beneficial streams are totally uninformed of their value, or of the means of determining the same.

(To be continued.)

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## IX.

*Enquiries concerning the Invention and Practice of the Art of Hat making.*

TO MR. NICHOLSON.

SIR,

Newcastle, 15th Dec. 1798.

YOUR Publication being open to enquiries relative to the advancement of manufactures, as well as the sciences, will you favour me with a place for the following queries,

ON

on a business but little known? And probably, amongst your subscribers, they may be answered through the same medium.—How, and at what time, was hat-making invented, and by whom?—Were there ever any engines made use of in that business?—Are there any at present? And if so, in what part of the country are they made use of? And how far do they go in the process?—Are there any machines for the cutting of hare, rabbit, or beaver skins? And if any, where are they worked?—Your inserting the above in your next number will oblige, Sir, yours,

N. L.

There is no mention made of the above, either in the Transactions of the Royal Society of London, or in Beckman's History of Inventions.

THE above did not come to hand early enough for me to make any enquiries respecting the invention, history, and practice of the art of hat-making. Researches of the kind pointed out by this correspondent are peculiarly adapted to the views and intention of a Journal of the Arts; and I hope he will not be disappointed in his expectations from others of my readers, whose pursuits may have enabled them to throw light upon the subject. At all events, I shall certainly have some information to communicate in the next number, which, for want of time, I cannot at present arrange and digest.

W. N.

## X.

*New Observations on the Method of producing very loud Fulminations with various Bodies, by Means of Phosphorus\*. By Cit. BRUGNATELLI.*

I WAS aware that the oxygenated muriate of potash produces effects superior to nitre, when mixed with charcoal and sulphur and converted into gunpowder; that it detonates by percussion or trituration with a great number of combustible substances; but I did not expect to produce effects much more considerable by using the simple nitrates, and even the metallic oxides mixed with phosphorus and struck with a hammer.

*Experiment 1.* I took a gros of the crystallized nitrate of silver, which I placed on an heavy anvil, and laid a very thin slice of phosphorus in the midst of the crystals. The temperature of the atmosphere during these experiments was not higher than 6° above the zero of Reaumur's thermometer. The materials being thus disposed, I struck the mixture rather smartly with a hammer. The consequence was one of the most terrible detonations I ever witnessed, which shook the anvil and its wooden support. Streaks of silver were observed upon the anvil five or six lines in length. One of the edges of the hammer was bended and turned upwards. I was perfectly stunned, and my clothes were torn in various places.

\* Translated by Van Mons from the Italian manuscript of the author into French, and inserted in the *Annales de Chimie*, xxvii. 72.; from which work the present translation is made.

I have

I have frequently repeated the same experiment, even in my lectures; and though I operated with no more than a few grains of the salt, the noise of the explosion was never less than that of a musquet.

The lapis infernalis has nearly the same effect as the nitrate of silver.

*Experiment 2.* I placed on an anvil a pinch of the oxygenated muriate of potash with a small portion of phosphorus, and struck the mixture with a hammer. The detonation was excessively strong.

*Experiment 3.* The dry nitrate of bismuth detonated very strongly. The same effect was produced with all the metallic nitrates which were to be found in the laboratory, and particularly with the nitrate of tin.

*Experiment 4.* I repeated the same experiment with the fused nitrate of mercury, of which I put six grains with a small quantity of phosphorus on an anvil, and struck them with a hammer. The phosphorus simply took fire, without producing any noise: but when the hammer was slightly heated, the same mixture detonated with a shocking noise. After the fulmination the mercury was found reduced, having, as it were, silvered the anvil in very brilliant radiations.

*Experiment 5.* I afterwards tried the alkaline nitrates, particularly that of potash. When a small slice of phosphorus was laid on a pinch of nitre and struck with a cold hammer, no fulmination was produced even by repeated blows; but having slightly heated the hammer, in order that the affinities might act more decisively, the very first blow produced a very loud fulmination, incomparably stronger than that of gunpowder.

*Experiment 6.* I obtained no fulmination or detonation with the sulphates of alumine, of copper, or of iron struck with phosphorus in the before-mentioned manner, though I heated the hammer more than usual.

*Experiment 7.* Neither did I obtain any fulmination from the simple muriate of silver, commonly called luna cornea, by the same treatment with phosphorus.

*Experiment 8.* I likewise tried the oxygenated muriate of silver, obtained by decomposing the nitrate of silver by the oxygenated muriatic acid. A pinch of this salt (with phosphorus), struck with a warmed hammer, produced a much weaker fulmination than was obtained with the crystallized or fused nitrate of silver, or the other salts before mentioned. The oxygenated muriate of mercury, treated in the same manner, afforded a very weak detonation.

*Experiments 9 and 10.* I endeavoured to produce fulminations with the metallic oxydes treated with phosphorus. Those of manganese, of zinc, of copper, of iron, of antimony, and of lead, produced no effect, even when struck with the hammer considerably heated; but I obtained fulminations with the yellow oxyde of mercury (turbith mineral) and the grey oxyde of the same metal.

Turbith mineral does not fulminate in contact with phosphorus when it is pulverised. It must be in a lump to produce this effect. The same thing happens with regard to the grey oxyde of mercury by the nitric acid, except that the fulmination is stronger.

*Experiments 11 and 12.* I was curious to ascertain whether I should obtain similar fulminations with the salts before mentioned, by substituting another acidifiable combustible substance instead of phosphorus; as, for example, sulphur and charcoal.

I accordingly took nine grains of lapis infernalis and three grains of pulverised sulphur,

which I struck with the cold hammer. The sulphur took fire without affording any noise; but, when the hammer was used hot, a detonation was heard, and rays of silver appeared on the anvil.

Having repeated the same experiment with lapis infernalis and charcoal, I could produce no more than a very dull detonation, though the hammer was well heated.

*Experiment 13.* I took several of the salts which had fulminated by the stroke, and threw them, together with the phosphorus, into the liquid oxygenated muriatic acid; but no detonation was produced. In an atmosphere of the gas of the same acid, the fulminating mixtures burned with a slight crepitation, similar to that which is excited by phosphorus alone.

*Experiment 14.* I wrapped most of these detonated mixtures in small pieces of paper, and threw them one after the other into a red-hot crucible. They burned with a very lively flame, but did not fulminate nor detonate.

#### CONCLUSION.

1st. THE nitrate of silver, whether crystallized or fused, fulminates when struck with a hammer, together with phosphorus, even at a low temperature. *Exp. 1.*—All the salts of silver do not fulminate equally, nor in the same manner. *Exp. 7.*

2d. Most of the metallic nitrates fulminate with phosphorus. *Exp. 3 and 4.*

3d. Common nitre, of which the fulminating property was before known, in its mixture with various combustibles, elevated to a certain temperature, or put into contact with an inflamed body, as in gunpowder or fulminating powder, is now found to detonate with phosphorus alone. *Exp. 5.*

4th. Those salts into which the nitric acid has not entered as a component part do not fulminate.

5th. The oxygenated muriates of potash, of silver, and of mercury, fulminate with phosphorus, but the latter with much less effect than many other salts. *Exp. 8 and 2.*

6th. The salts are not the only bodies which fulminate with phosphorus: several metallic oxydes have the same property. *Exp. 9 and 10.*

7th. Phosphorus likewise is not the only acidifiable and solid combustible matter capable of producing fulminations. Charcoal produces the same effect at a more elevated temperature. *Exp. 11 and 12.*

8th. Those substances which fulminate with phosphorus produce no effect when thrown into the liquid oxygenated muriatic acid. *Exp. 13.*—Neither do they fulminate when exposed together with phosphorus to an elevated temperature. *Exp. 14.*—The blow of the hammer is necessary to throw the component parts of these bodies into a state of oscillation, in order to determine the affinities with effect.

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#### ADDITION BY VAN MONS.

THE facts observed by the learned editor of the Italian Annals of Chemistry appeared too curious and important for me to lose any time in repeating his experiments. My success was as follows:

The

The crystallised nitrate of silver detonated very strongly, emitting only a weak flame. The silver was reduced to the state of a blackish oxide.

Lapis infernalis also detonated, but much more weakly. The metal was completely reduced.

The nitrate of tin fulminated with considerable strength.

The fused nitrate of mercury produced a strong effect.

The different oxides tried by Brugnatelli, that of manganese excepted, produced no noise by friction; and the detonations under the stroke were at least doubtful. The oxides were all in part reduced, and the mixtures burned in a lively manner, throwing out ignited sparks to a distance; but the phosphorus alone, after three or four strokes with the hammer, exhibited the same appearance.

The oxygenated muriate of mercury afforded perceptible marks of detonation.

I produced these several fulminations without heating the hammer for any one of them. Friction, or a few slight previous blows, heat the materials sufficiently to cause the effect to take place with the last smart blow.

I tried a great number of times to make the experiment with sulphur and lapis infernalis. In some instances I rubbed the two substances together with all my force in iron or marble mortars, and in others I struck them with a heated hammer; but I did not obtain the most feeble detonation. The matter itself did not even take fire, excepting when I struck it smartly in an iron mortar strongly heated with a pestle equally hot. I was not more fortunate with charcoal. Similar experiments, which I have since tried with various other substances, afforded me the following results:

The nitrate of gold by evaporation produced a louder detonation than is afforded by the oxygenated muriates of alkali. The marble mortar in which I made the experiment was covered with small particles of excessively thin plates of reduced gold. The metal seemed to have undergone a very liquid fusion.

The muriate of mercury detonated with a force at least equal to that of the nitrate of gold. The metal was almost entirely dissipated.

The nitrate of lead produced a weaker detonation than the foregoing. The metal was found reduced to the state of black oxide.

The oxygenated muriates of antimony, zinc, and tin, produced a much weaker effect. I succeeded, however, once in producing a very strong and instantaneous detonation with the first of these salts.

The oxides of gold, silver, and mercury, by fire, were found to occupy the first rank among the fulminating substances. The oxygenated muriate of potash does not afford effects equally constant with those of the oxides.

The oxide of lead, in its different degrees of oxydation, afforded in no instance any appearance but that of inflammation.

I formerly exhibited in my public lectures the detonation which may be produced by percussion in a mixture of the nitrate of potash and phosphorus. I had even produced explosions sufficiently violent to induce the audience to suppose that I operated with the oxygenated muriate. I submitted the nitrate of ammoniac to the same treatment, and produced a detonation so terrible that it shook the doors of my laboratory.

The nitrates of barytes, of strontian, and of magnesia, by desiccation, detonated with nearly the same force as the nitrate of potash.

I had still remaining five grains of the oxygenated muriate of ammoniac, of which I took the half, with about four grains of phosphorus. I slightly crushed these two substances, with the intention of mixing them together, when a detonation so terrible was heard that the whole house was alarmed. The phosphorus was for the most part thrown upon my hat, which it burned for a long time before I perceived it. The violence of the blow caused the hammer to fly out of my hands. I very distinctly felt it raised by the expansion of the gases. The other half of the oxygenated muriate of ammoniac was placed alone upon the bottom of an inverted iron mortar, and struck rather briskly with the cold hammer. The first blow caused it to detonate, but no light was disengaged. I regretted that no more of this salt remained to make the experiment with sulphur and charcoal.

I also struck the nitrate of ammoniac alone. The fourth blow caused it to fulminate. I say it fulminated, because the detonation was accompanied with a disengagement of light.

I obtained an effect equally considerable from the white nitrate of mercury and ammoniac. I think that all the nitro-ammoniacal triple salts will detonate more or less strongly with phosphorus.

I was desirous of trying the solid oxygenated muriatic acid; for which purpose I caused this acid to crystallize by artificial cold. A few crystals of this acid were put together, with a small piece of phosphorus, upon an iron mortar placed in a cooling mixture. When these were struck with a hammer, a dull detonation was heard at the sixth or seventh blow. The fused acid was projected to a distance.

I repeated a great number of these experiments with pyrophori and the phosphori called artificial carefully prepared; and in almost every case I obtained a detonation. I shall describe my experiments on these singular inflammable preparations in a separate article.

Whenever I struck the before-mentioned mixtures with a heated hammer, I always obtained a weaker detonation, at the same time that the inflammation was stronger; and when the hammer was too hot, no detonation or noise was produced. This observation I had already made with the oxygenated muriate of potash; and it appears to me to explain the phenomena of detonation. The blow with the cold apparatus, by strongly compressing the matter, and perhaps exciting some heat, produces a partial combustion of the phosphorus, and consequently engages only part of the oxygen of which the other portion assumes the elastic state, and produces the noise of the detonation. At a very elevated temperature the effects are not altogether the same. All the oxygen is at once employed to burn the combustible, whence the inflammation is the strongest, and no detonation takes place. In fact it is observable, that the more sudden and sonorous the noise, the less is the development of fire; and the contrary. To this principle also is referable the observation made by Brugnatelli, which I have found to be true, that the oxygenating body must in some instances possess the form of a lump to obtain the effect; because it applies too extensively to the combustible matter when powdered.

These experiments succeed much better by friction than by a blow, and the effects take place more readily upon a rough than a smooth body. I usually place the two substances on the bottom of an inverted marble mortar, which is left rough from the saw.

Upon

Upon these I press the face of an iron hammer, holding the handle with the left hand, and suddenly slide the hammer forward so as to produce a kind of shock. In this manner the detonation or noise is likewise more distinct, and its force more easily determined. Simple compression produces the detonation in many cases.

I think it proper to warn those who are not familiar with experiments of this kind, that when the operation is performed with more than a grain and a half, or at most two grains of phosphorus, there will be danger of burning their hands or clothes by the excess of this combustible, which flies off in a burning-state.

I cannot finish this article without restoring to its true proprietor the portion of honour which is due to the discoverer of the detonating property of the oxygenated muriate by percussion or strong friction, which Citizen Fourcroy has attributed to me in the *Memoir* \* he has communicated to the National Institute, no doubt in consequence of my having mentioned this phenomenon in my edition of the *Philosophy of Chemistry*, under the article of *Oxygenated Muriates*. Professor Wurzer, of Bonn, was the first who observed this fact, by triturating with some force, in a mortar, a mixture, scarcely weighing a grain and a half, of three parts of the oxygenated muriate of soda and one part of sulphur. He obtained a detonation which rendered him deaf for several days. See *Crell's Chemical Annals* for the year 1792, vol. ii. page 402.

### TUNNEL BENEATH THE THAMES.

Reports, with Plans, Sections, &c. of the proposed Dry Tunnel or Passage from Gravesend in Kent to Tilbury in Essex, demonstrating its Practicability, and great Importance to the two Counties and to the Nation at large; also, on a Canal from near Gravesend to Stroud, with some Miscellaneous and Practical Observations. By R. Dodd, Engineer. Quarto. 28 pages, with 3 plates: viz. 1. Plan and Sections of the proposed Tunnel. 2. A View of Gravesend and Tilbury, with the Section of the River, shewing the Strata and Depth of Water: and, 3. A Map of the Country within twenty Miles of Gravesend. London, printed for J. Taylor, 1798.

SOME account of this undertaking was given in this Journal a few months ago. (II. 239.) The present Reports will afford satisfactory information of its detail and progress, and the public will hear with pleasure that it is likely to be carried into effect.

In the month of May 1798, Mr. Dodd circulated an Introductory Report or Address to the Nobility, Gentry, &c. of Essex and Kent, in which he states that the extended scale of commerce on the river Thames forbids the construction of a bridge at Gravesend; but that the practice of making passages, tunnels, or drifts under rivers has been adopted to a great extent in various parts of the kingdom; namely, at the coal works under the rivers Tyne and Wear, and at Whitehaven under the very ocean; that a tunnel is actually intended to be made under the mouth of the river Tyne, to answer the purposes of a bridge for carriages and passengers;—that the measure now proposed will save a circuit of near

\* *Philosophical Journal*, I. 162.

fifty miles to carriages passing from one county to the other; and that the situation at or near Gravesend is peculiarly eligible from its natural facilities, as the greatest part of the ground to be passed through is chalk. These observations are followed by the proposal and estimate which have already been given at the page of our Journal last quoted. Mr. Dodd observes that 12 l. per running yard must be considered as a handsome allowance, since it is a fact, that some of the most considerable tunnels in this kingdom on a scale of nineteen feet by seventeen, which is larger than the tunnel at present recommended, have been executed at that price.

On the 18th of July 1798, a meeting was held in the Town Hall of Gravesend, the Earl of Darnley in the chair; when it was resolved, That there being no reason to doubt the practicability of so desirable a measure, the co-operation of government, the assent of the neighbouring land owners, or the willingness of the public to subscribe to its accomplishment, the opinion of the meeting was, that transferable shares of 100 l. each would afford the best means of effecting it; and that a subscription should be opened at the office of Messrs. Evans and Son, at Gravesend, for its support and encouragement.

In the report and estimate which the engineer presented to this meeting, he states, that from the actual survey he finds no reason to add to the former estimate, but rather to abate somewhat more than 3000 l. on account of the excellent chalk which he finds will be afforded at the most eligible place for the excavation. The deviation from an horizontal line of the tunnel, which passes at about thirty feet below the bed of the river, will be four inches in the yard. Several military and commercial advantages of the undertaking are likewise clearly stated, and an estimate given of the cost of a twelve feet tunnel, admitting carriages to pass only one way at a time, in case the sixteen feet tunnel should be thought too expensive. In the subsequent observations the author enlarges still more upon the same interesting topics, and makes a comparison between the communication by tunnels and the great modern improvement of iron bridges, which last erection he shews to be much less durable, and more liable to speedy destruction by an enemy, than the tunnel. He remarks, that the destructive elastic fluids, which are known to be extricated in mines, cannot affect the present work when once finished, because of the clear communication with the external air at both ends; and he gives some practical remarks on the discovery of inflammable air or fire-damp, which, on account of the interesting nature of the subject, I shall here transcribe.

“ On entering any excavation under ground, where this fire-damp is suspected to lie; the candle ought to be held in the left hand (the smaller the candle the more to be depended upon), and the flame thereof to be shaded by the right-hand fingers being placed horizontal, and on a level with the top of the flame of the candle. If the air be good, and perfectly free from any inflammable air or hydrogen, a small brown top or pinnacle is seen on the top of the flame, the same as is seen in the chamber of a house; but on approaching the real fire-damp another top or spire is seen of a blue colour, above the brown just described as arising therefrom; and on advancing a step or two forward the damp is commonly more dense, and consequently the appearances over the candle assume a more serious aspect by the brown spire or small top beginning to disappear; another step perhaps reduces it altogether, on which the danger commences; and the spire or top sits down altogether blue on the flame. At this last stage it is not safe to proceed further without

great



great circumspection joined with steadiness: a little further the blue top is seen to be more perfect and dense, which if general through the place, all lights should be extinguished, and the workmen be made to quit the place. Perhaps on advancing a very little further a light blue bead appears to circumscribe the other dark blue, as if approaching to flame, and having a more than ordinary pointed spire terminating like a white thread; and the danger becomes very great if to remain any time in this position. The next stage shews the sprents to have taken place, which sprents are similar to those produced by squeezing the oil from the rind of a lemon into the flame of a candle: at this period sometimes has been observed a small bushy dark-coloured cloud hanging over the top of the flame, still more and more attracting the fire; when in an instant perhaps after this last observation the whole appearance expands into flames, and one general conflagration takes place. The mischief which ensues is in proportion to the accumulated quantity of this pernicious vapour; which if great flies to the nearest aperture leading to the atmosphere, being about eleven times lighter than common air, with a dreadful explosion and extraordinary eruption. But in case the said fire-damp or inflammable air be mixed with styth, or black damp, these appearances are somewhat varied; and by being mixed with a very dark brown is in general more safe.

“Black damp or styth” (doubtless carbonic acid, or hydro-carbonate) “which arises in mines is directly the reverse of inflammable air or fire-damp; the former extinguishes the flame of a candle as quickly as if put into water or any other fluid; the other causes the candle to burn too fast. This styth or black damp prevails most where there is little or no inflammable air, and when the due circulation of atmospheric air is neglected.”

It was of essential consequence, by way of shewing the practicability of the present undertaking, that Mr. Dodd should impress the minds of his employers with the fact that such tunnels had been made with success on a much larger scale. For this purpose he relates various historical matters, besides those already mentioned, from which we may notice, that the earliest tunnel for inland navigation was executed by M. Riquet, to convey the canal of Languedoc through a mountain near Beziers. The first executed in this country was by the celebrated Brindley on the duke of Bridgewater's canal near Manchester. The next is the famous tunnel of Hare Castle Hill, in Staffordshire, by the same engineer, which is 2880 yards long, and passes through a variety of strata, quicksands, &c. more than seventy yards below the surface of the earth, and serves as the receptacle for part of the Grand Trunk Canal. The tunnel of Saperton is two miles and three quarters long, and was carried through two miles of solid rock. Many other drifts or tunnels have been made in this kingdom, through rocks and obstacles of various kinds, in a course of time and at expence much less than would be apprehended by persons unacquainted with works of this nature.

Mr. Dodd's report and estimate on the projected dry tunnel under the river Tyne contains illustrations and argument of nearly the same import. His pamphlet concludes with a proposal for an inland canal from the Thames near Gravesend, to the Medway near Stroud; which, by a short course of about six miles, would save the circuitous and less certain passage of forty-seven miles by the way of the Nore. The peculiar advantages of this cut to the country at large, and to the government establishments on these rivers, as well as the nature of the ground itself, are strongly and perspicuously stated.

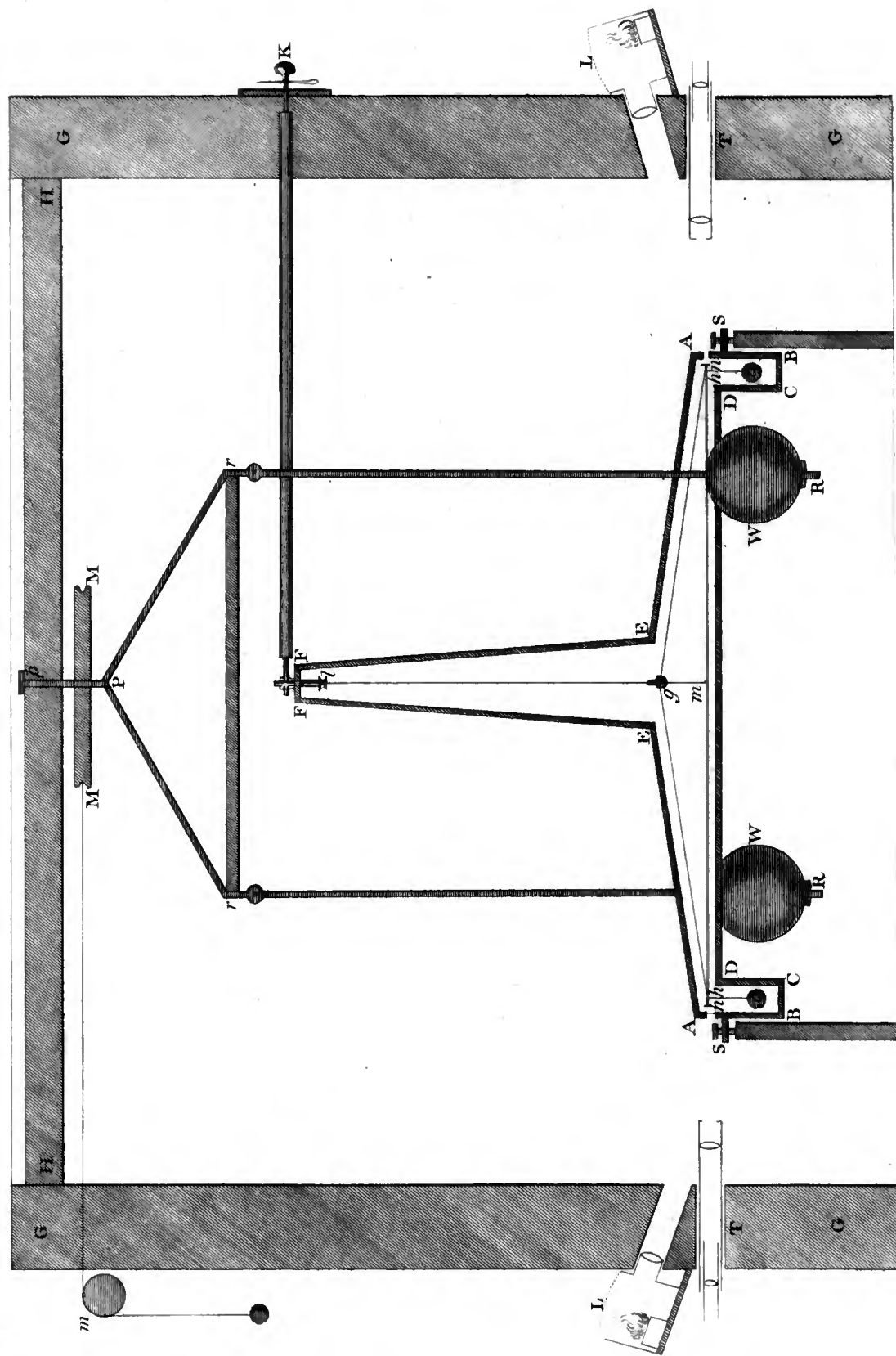
The last public meeting on the former business was held at the London Tavern on the 14th of last month (December); when it appeared by the Report that the subscriptions amounted to upwards of ten thousand pounds. The managing committee of subscribers consists of the Earl of Darnley, Lord Petre, the Hon. J. T. Townshend, M. P. the Hon. Robert Petre, Sir William Geary, Bart. M. P., Mr. Ald. Lushington, M. P., John J. Angerstein, and Claude Scott, Esqrs. Lieut. Col. Twiss, Royal Engineers, Capt. Schank, R. N. T. Woodruffe Smith, Esq., John Mavor, Esq., Geo. Hawks, Esq., and Benjamin Harrison, Esq.—Claude Scott, Esq. is Treasurer.

The undertaking will be begun and carried on by those who shall first subscribe to the proposed shares, which are three hundred at a hundred pounds each; but the actual work will not be commenced till the whole sum of thirty thousand pounds shall have been subscribed.

A fellow-labourer in the cause of science, Mr. A. Tilloch, objects totally to the account I gave of Mr. Cartwright's apparatus for rendering the pistons of steam-engines tight by metallic fittings, at page 365 of the present volume. As the whole of his remarks appear to me to be obviously erroneous, and the matter is before the public, I must be excused from entering into controversy. He will himself perhaps, on second thoughts, recollect that a circle, even though in diameter equal to that of the earth, will not be converted into a triangle by cutting it in three pieces; and that it is absurd to suppose the third law of nature, that *action and re-action are equal and contrary*, can either be dispensed with or explained away. He will then probably look (not to the vacuum, but) to the lower plate of Mr. Cartwright's piston for the re-action; which, by means of the piston rod and the work required to be done, is made to act beneath the moveable pieces of the apparatus, while the steam presses their upper surface with no inconsiderable force; admitting its elasticity to be equivalent to a single atmosphere only: a force sufficient, in my apprehension, to prevent ground surfaces from sliding freely, if at all, upon each other. But, as we are all liable to mistakes in new practical matters, I could have wished that Mr. Tilloch had reasoned less diffusely, and referred at once to the facts. If it be true that there was a steam-engine at work six months ago, when Mr. T. invited the public to inspect it, at Mr. Rowley's, in Cleveland-street, Marylebone\*; or if he can bring evidence that such an engine has been at work for any considerable part of the time since he published his description, and that the facility of operation, the power, and the durability of the apparatus are such as he conceives them to be;—I cannot but think that he has, unfortunately, overlooked his best argument. To this argument, if offered, I must grant my assent; and I beg leave to assure him that I shall most readily attend, inspect, and report concerning this engine, if he or the inventor will give me the opportunity, without in the least regarding whether this part of my duty to the public shall confirm or overthrow those opinions which the present state of the facts has compelled me to give.

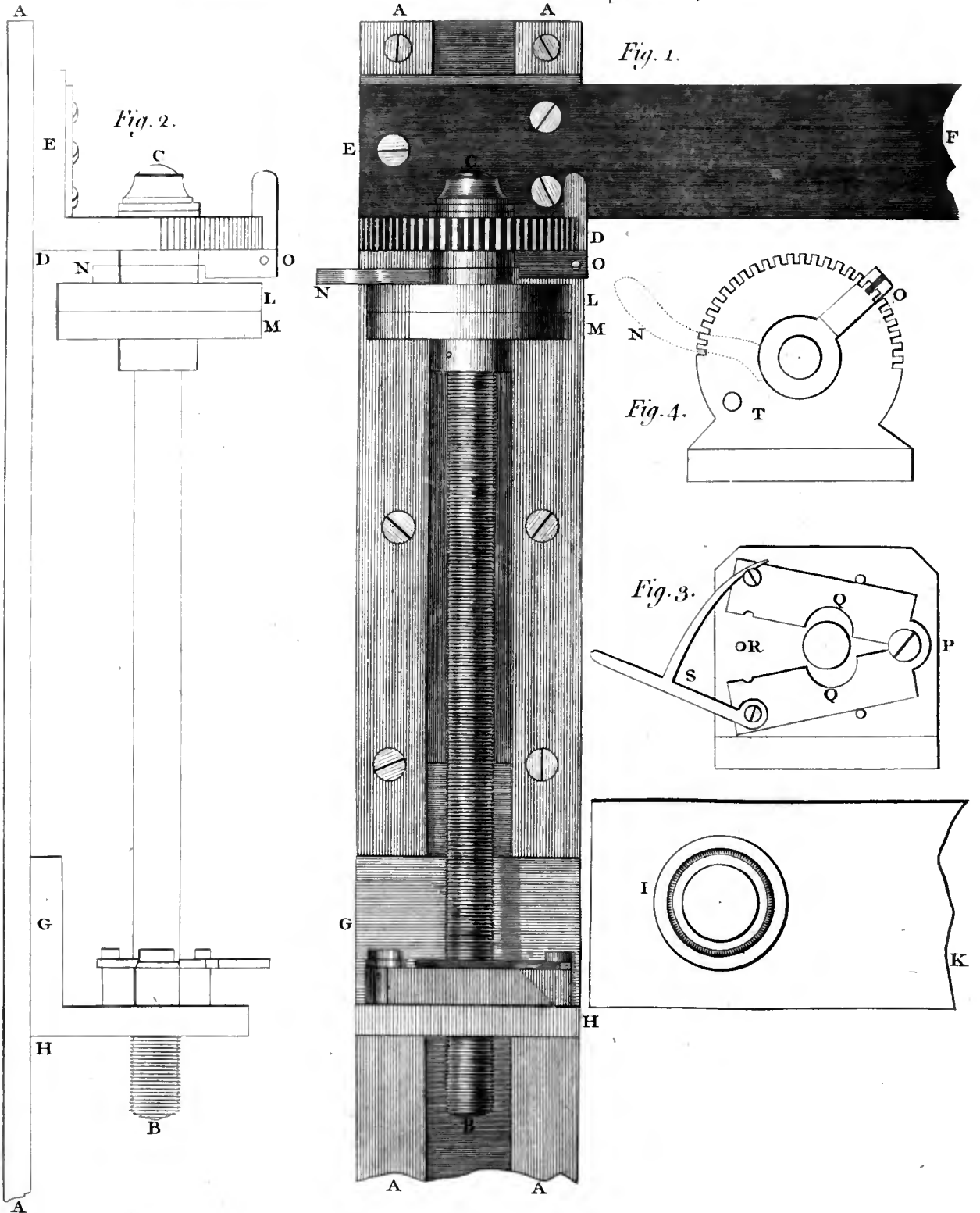
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\* I went there at the time, and saw the parts of a small model of engine, which was not at work; and, upon late enquiry, I understand there is none there at present. I could gain no information whether there was any engine of this construction at work, or in progress, at any other place.





# Instrument for ruling the Shades of engraved plates.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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FEBRUARY 1799.

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ARTICLE I.

*On the Corundum Stone from Asia. By the Right Hon. CHARLES GREVILLE, F. R. S.\*.*

Analyti crystallorum, tam ejusdem quam diversæ figuræ, multum lucis scientiæ expectat.

BERGMAN, Opusc. de Terra Gemmarum.

HAVING contributed to bring into notice the mineral substance from the East-Indies, which is generally called adamantine spar, I beg leave to lay before the Royal Society the following account of its history and introduction.

About the year 1767, or 1768, Mr. William Berry, a very respectable man, and an eminent engraver of stone, at Edinburgh, received from Dr. Anderson, of Madras, a box of crystals, with information of their being the material used by the natives of India to polish crystal, and all gems but diamonds. Mr. Berry found that they cut agate, cornelian, &c. but in his minute engraving of figures upon seals, &c. the superior hardness of the diamond appeared preferable; and its dispatch compensated for the price: the crystals were therefore laid aside as curiosities. Dr. Black ascertained their being different from other stones observed in Europe; and their hardness attached to them the name of adamantine spar. My friend, Colonel Cathcart, sent me its native name, *Corundum*, from India, with some specimens given to him by Dr. Anderson in 1784, which I distributed for analysis.

When the native name was obtained, it appeared from Dr. Woodward's catalogue of foreign fossils, published about the year 1719, that the same substance had been sent to him by his correspondent Mr. Bulkley.

In his first catalogue of foreign fossils, p. 6. §. 17. "*Nella corivindum* is found in fields where the rice grows: it is commonly thrown up by field rats, and used, as we do emery, to polish iron."

\* Philosophical Transactions, 1798, p. 403.

Page 11.  $\lambda$ . 13. "*Tella convindum*, Fort St. George, Mr. Bulkley. 'Tis a talky spar, grey, with a cast of green: it is used to polish rubies and diamonds."

In Dr. Woodward's Additional Catalogue of Foreign Fossils, published in 1725, p. 6.  $\zeta$ . 10. "*Nella corivendum* is found by digging at the foot, or bottom, of hills, about five hundred miles to the southward of this place. They use it as emery, to clean arms, &c. it serves also to grind rubies, by making it like hard cement, by the help of stick lac mixt with it. East-India. Mr. Bulkley."—These, with a few others in Woodward's Catalogues, are the only instances by which any author, prior to 1768, appears to have noticed this substance.

This information being unsatisfactory, and every appearance of the stone indicating it to be part of a stratum, I wrote repeatedly to friends in India, to ascertain, if possible, the situation of the rock, and if near the sea, to send a considerable quantity as ballast, with a view of applying it to cut and polish granites, porphyry, and other stones, which the high price of cutting and polishing excluded from useful or ornamental work. But my inquiries at Madras were fruitless: by some I was assured it came from Guzarat. From Bombay I obtained no satisfactory information. At last, in the year 1793, I obtained a satisfactory account. Sir Charles Oakley was disposed to oblige me: he was then Governor of Madras; and his success is due to the activity and judgment of Mr. Garrow.

Mr. Garrow knew how difficult it was to avoid the causes of my failure, from every Hindoo being occupied by the duties of his cast; scarcely thinking on any thing else, and wherever his interest is concerned, being suspicious and reserved. Mr. Garrow, in the first place, ascertained the cast connected with corundum, to be the venders of glass *bangles*; that they used it in their business, and sold it to all other casts. This cast of natives at all times had free access to every part of Tippoo's country; nor until the districts about Permetty were ceded to the English, could it be procured in any other way. Mr. Garrow depended on his personal inspection; the particulars are contained in the following letter communicated to me by Sir Charles Oakley.

#### SIR CHARLES OAKLEY, BART.

"Sir,

Tritchinopoly, 10th Nov. 1792.

"I derived so little satisfaction from the various accounts given me of the corundum, from the indifference of the natives to every subject in which they are not immediately interested, that I resolved to ascertain the particulars I wished to know, on the spot where the stone is found. The glass-men agreed in one material circumstance, that the place was not far from Permetty: in other particulars they disagreed, apparently with intention to mislead.

"It is near a fortnight since I dispatched a servant I could depend on to Permetty, with one of these people, who on his arrival there, probably through fear of his cast, said he knew no farther. My servant persevered, and informed me he had found the place I wished to see.

"I arrived at Permetty, by the route of Namcul, the 6th; and learning that the distance to the spot was about  $3\frac{1}{2}$  hours, or 14 miles, I left Permetty in time to arrive there about sunrise the next morning. At this time no person but my servant was present, and from a continued excavation at different depths, from 6 to 16 feet, in appearance like a water-course, running



running in length about a mile and a half east and west over the brow of a very rising ground, I saw at once the place from which the stone was procured. The prodigious extent that at different times appears to have been dug up, with the few people employed, shews that it has been a business of ages.

"The ground on which the vein of excavation runs, and of course the mineral, commands one of the finest and most extensive prospects it is possible to conceive. The surface of the ground is covered with innumerable fine alabaster stones, and a variety of small shrubs, but not a tree sufficient to shelter my palanquin.

"There is not the appearance of an habitation within three quarters of a mile. The nearest village is called Condrastra Pollam. In this village are about 30 small thatched houses: among these are 5 families, who, in descent by prescriptive right, are the miners, and dig in the pits. The nearest place of any consequence in Rennel's Map, is Caranel, on the south side the Cavery. The distance of the pits from the river is above four miles; but the ground between prevents its being seen in a direct line. A fine view of the river is seen near Erode; which fort, as well as Sankerdroog, are plainly visible with the naked eye, as is also the Coimbitoor country, south and west of the river, to an immense extent.

"I procured at Permetty a cadjan, from the Bramin manager to the head man of the Pollam; which on my arrival at the pits I sent to him; and soon after three of the miners came from the Pollam, with their implements, and families following with provisions. As they came up they inquired of my servant how they were to address me, having never seen an European before.

"I followed them into a pit in the line of the excavation, above 14 feet from the ground level. The instrument they used is a very heavy iron crow, ending in a broad point, with a straight wooden handle clamped with iron. The soil they cut through is of different colours, but composed chiefly of a gritty granite; and at the depth of seven feet are layers of a substance not unlike dried pitch, which crumbles into small flakes when taken out. With considerable labour the miners, with the points of their crows, cut out several pieces of the strata, of some pounds weight each; and when a considerable quantity was broken off, it was carried up, and crushed to pieces with great force by the iron crow. Among these broken lumps the corundum stone is found; but in many of the pieces there was none. The mode of getting it, made it difficult to get any with the stratum adhering to it; this, however, after several trials, I obtained very perfect, and shall forward to Madras, with specimens of the strata at different depths. The stone is beyond all comparison heavier than the substance which encrusts it.

"It appears extraordinary how this stone, so concealed, should, under such difficulties, have been sought for, and applied to any purpose; and that the knowledge of the few people who dig for it, and who do so from father to son, is confined entirely to the finding the stone. For they told me they knew none of its uses, and that the labour was so hard, and their gain so small, that they would, through choice, rather work in the fields; that the sale of it, from the spot, is confined solely to the glass-sellers, who vend it over the whole country, and who had, while I was there, above forty Parriar horses, bullocks, &c. ready in the

Pollam to carry it to Tinnevely, and the southern countries; through which track, if the stone is known in Europe, I apprehend it has found its way by means of the Dutch.

"The people on the spot declare it is to be got in no other situation, or place, whatever; and the stone-cutters tell me, they can do nothing without it. It pays no duty either where dug up or retailed.

"The colour of the stone is either very light brown, or purplish, in the proportion of twenty to one of the latter; but in use no preference is given, and they are used equally. To an indifferent person the most striking circumstance is its great weight.

"As the spot I have been speaking of now composes a part of the Company's territories, the most minute information on the subject may be acquired.

"I felt particular satisfaction at having been the first European who was ever at the place; and I shall be much gratified, if the account given meets with your approbation.

"I shall dispatch a load of the stone in a day or two, which I got at the Pollam, with the charge of it. The distance from this place, by Namcul, is 84 miles.

*"The charges of 50lb. weight of Corundum.*

"Nine Tritchinopoly measures of the corundum stone weigh 50lb.

Average and Cost at the Pits *.						P.	F.	C.
" 1½ Madras farams per measure	-	-	-	-	-	0	13	40
Cooley, from thence to Tritchinopoly	-	-	-	-	-	0	28	40
Ditto from Tritchinopoly	-	-	-	-	-	1	13	40
						<hr/>		
						Pagodas	2	10 40

"The stone is delivered by measures, and paid for at the Pollam in the gold fanam.

"I am, &c.

"Nov. 15, 1792.

"EDWARD GARROW."

This letter contains very interesting topographical observations on the mine. The specimens sent were of one sort of a greyish colour, with a shade of green. The entire crystals which I selected among the broken ones were, of course, few in proportion; but with the addition of some distinct crystals, which Col. Cathcart, and Capt. Colin Macauley had sent me, have been sufficient to ascertain the structure and form of the crystals, of which an analytical description will close this paper. I shall, therefore, now say nothing concerning their form, but proceed to give an account of the varieties of corundum stone, which I have obtained from India and China.

In the year 1786, Col. Cathcart sent me a small fragment of a stratified mass from Bengal, with this label: "Corundum, much inferior in price to that of the coast." It is of a purplish hue; its fracture like compact sand-stones; and a confused crystallization appears in all

\* The above is the prime cost. I have been informed by correspondents who purchased some in retail, that it was sold for about six shillings a pound at Madras.

parts

parts of the stone, by fibres of a white colour, from which the light is reflected as in feldspar, &c.

I have since obtained a larger lump of the stone of the same texture, but rather paler in its purplish hue. Sir John Macgregor Murray informed me, that it is called by the natives of Bengal, *corone*, and used for polishing stones, and for all the purposes of emery.

Its specific gravity is 3,876.

Capt. Colin Macauley procured a lump of corundum from a *skuldar* (a polisher, this term is most appropriate to polishers of steel), in whose family it had been above twenty years, employed for grinding and polishing stones or gems. The use to which it had been so long devoted, had occasioned grooves in its surfaces which facilitated greatly the examination of its structure. It is about  $5\frac{1}{2}$  inches long,  $3\frac{1}{2}$  inches broad, and above two inches thick. On one of its broad surfaces are two oval grooves; one of them is four inches long, one broad, and  $\frac{3}{4}$  of an inch deep. On the opposite side is a shorter oval groove, above  $2\frac{1}{2}$  inches long,  $1\frac{1}{2}$  inch broad, and one inch deep. In these grooves, the ends of the laminæ of the class reflect the light like the crystals. It serves as a specimen of the simple apparatus of an Indian lapidary. Stones polished in these grooves, would be of the common India polish, and form *en cabochon*, which is often called tallow-drop from the French lapidaries' term *goutte de suif*, convex, oval, or circular. A very small quantity of the corundum powder would be required, as the action of the powdered corundum and gems on the lump of corundum would, as appears from the depth of the grooves, wear away from it a supply of powder for the operation of polishing. It appears to be part of a larger mass, is of a purplish colour, and of the same laminated texture as the crystals of corundum; it has this peculiarity, there appear cracks branching irregularly across the laminæ of the lump, which are filled with homogeneous matter, distinguished, however, by the superior purity, which might be expected to arise from the degree of filtration required for its deposition in the fissures. Some of these cracks, which terminate on the surface, appear to have the same crystallized arrangement which characterizes the laminæ of corundum. The cracks not being, in any degree, influenced in their direction by the laminæ of the crystallized mass, it is probable they had not been consolidated when they cracked; and from this specimen, we may expect to find corundum cementing masses of stone by the same process of stalaclitical cementation, by which quartz and calcedony connect great nodules and masses of siliceous stones.

In this specimen, I consider the veins as pure corundum, that is, having the same specific gravity, hardness, and texture, as corundum crystals; and I found the whole lump possessed all the qualities of corundum, except its specific gravity, which amounted only to 2,785; and, in this property, it corresponded nearly with the matrix of the corundum crystals, or the vein in which corundum is before stated to be found; the specific gravity of which is 2,768. The texture of the matrix appears sometimes like adularia, and confusedly crystallized; often compact like cipoline or primitive marble; sometimes sparry, sometimes granulated; and on the outside of the vein, and near fissures, decomposed and becoming opaque. In all its states it scratches glass, but not rock-crystal, possibly from want of adherence of its particles:

particles: and in this, it differs from the substance of the above lump, which cuts glass and rock-crystal with great facility.

This lump, and the matrix of corundum, appeared to possess the same properties as corundum, when examined by the blow-pipe with the different fluxes.

The matrix of corundum having sometimes an appearance like adularia and feldspar, I ascertained, by Mr. Hatchett's scales, the specific gravity of adularia to be 2,558, and of feldspar 2,555. The corundum, and the lighter corundum of the lump; cut adularia and feldspar; the latter effervesced, and combined with soda, which the former did not.

It is, therefore, evident that the matrix of corundum, or substance of the vein, is a distinct substance from adularia and feldspar, and nearly connected with corundum.

The matrix, or vein, contains also a black substance like thorl, which, on closer examination, appears to be *hornblende*. This substance, Mr. Garrow had remarked to have the appearance of charcoal; and, on that account, he had attributed the formation of these strata to the agency of fire. Other gentlemen, from the appearance of the matrix of corundum, have stated it to be a calcareous vein.

Mr. Garrow observed that there ran through the strata, in which the corundum was found, veins of a substance like dried pitch, apparently on their edge, which separated like a pack of cards. It is a brown micaceous substance, which, in drying, foliates, and shews a certain degree of regular arrangement of the component parts; in this case, the fragments of the folia subdivide with some degree of regularity into rhombs, whose angles are  $60^\circ$  and  $120^\circ$ : it is more smooth and less flexible than pure mica.

These are all the sorts of corundum which I procured from India.

I now proceed to the result of my inquiries in China.

I requested Capt. Cumming, in 1786, at that time commanding the company's ship, *Britannia*, to take a specimen of corundum to China, to ascertain its nature, and to obtain specimens, if possible, adhering to their matrix, and regularly crystallized. On his arrival at Canton, he collected the information I wished, with the good sense and zealous desire which he always exerts for his friends. He ascertained that the stone I inquired for was in common use with the stone-cutters; and he brought me the stone in its rude and in its pounded state, taking care to select the most regularly crystallized pieces, and others adhering to the rock. A stone-cutter was sawing rock-crystal with a hand-saw, which he also brought to me; it is a piece of bamboo slit, about three feet long, and  $1\frac{1}{4}$  inch broad, thickened at the handle by a piece of wood, rivetted with two iron pins; having a lump of lead tied with a thong of split rattan, steadying an iron pin, on which the end of a twisted iron-wire is fastened, which being stretched to the handle, is passed through a hole in the bamboo with the superabundant wire; a wooden peg being pressed into the hole, keeps the bow bent, and the wire stretched, and serves to coil the superfluous wire, till, by sawing the crystal, the stretched wire is worn, and requires to be renewed from the coil. The twisted wire answers the purpose of a saw, and retains the powder of corundum and water, which are used in this operation. Dr. Lind had before brought specimens similar to the above from China.

From Sir Joseph Banks, I obtained Dr. Lind's specimens, and some in powder, which Mr. Duncan, supercargo, in China, had sent him with the Chinese name, pou-fa. The matrix

trix being mixed with a red and white sparry substance and mica, is generally called red granite; but it appears to me of the same nature as the matrix of corundum from India. The white is more fibrous, and like cyanite; the red part of it is compact and opaque; other parts appear to foliate, and pure mica is in considerable patches, and generally adheres to the crystals. This corundum is of a darker brown, and more irregular on the surface, than the corundum of the coast, and often mixed with black iron ore\* attractable by the magnet.

It is described as the third modification of the corundum crystal in the analytical description which follows. The *chatoyant*, or play of light, on these dark crystals, is very remarkable: some are of a bright copper colour; others exhibit the accident of reflection of light, which, in a polished state, gives varieties to the cat's-eye, star-stone, sun-stone, &c; which as yet are classed from such accident without strict attention to their nature, which is various, and, in general, has not been ascertained.

These are the circumstances connected with the strata, worth mentioning. The examination of corundum, on which our present knowledge rests, is nearly that which an India mineralogist might derive of the history of feldspar from a lump of Aberdeen granite out of one or two different quarries. He might ascertain a few modifications of the crystal of feldspar, its fracture, and matrix; but he would have no knowledge of the purest or more beautiful sorts, which other quarries produce in Scotland, at Baverno, at St. Gothard, and Auvergne. I therefore think it essential to mention that corundum, under circumstances favourable to its crystallization, becomes glassy in its fracture, and of various colours. I have not only observed in crystals of corundum specks of a fine ruby colour, but I have fragments of crystals in texture and every respect like the colourless corundum, of a fine red colour. It is certain that we obtain from India, corundum which may pass for rubies. I have sent to India some of the corundum with small ruby specks, which were not sufficiently distinct or large, either for measurement or analysis, in hopes of being enabled to ascertain correctly the form of *Salam* rubies found in corundum; in the mean time, I have the corundum of a fine red colour. Looking over some polished rubies from India, I selected one which appeared laminated like corundum, and had also the *chatoyant*, or play of light, on its laminae, which formed an angle in the stone. The lapidary called it an oriental ruby. I altered the form of the cutting so fortunately that the reflected rays formed a perfect star; a phenomenon I had observed in the sapphire, and expected in corundum, but not in the octoedral ruby. The specific gravity of this stone being 4,166, confirmed my opinion that it is one of the *Salam* rubies, so much esteemed by the natives on the coast or peninsula of India, which are found in the corundum vein. The specific gravity of a colourless sapphire very little less opaque than corundum, forming also a perfect star, was 4,000; that of a deep blue sapphire, and of a star-stone, 4,035; all which I connect with the corundum; the specific gravity of a

\* A small group, consisting of three or four octoedral crystals, presents the least common variety of this kind of iron ore; the edges of the octoedra being replaced by planes, which almost cover the triangular planes. *Romé de l'Isle. Cristallog.* vol. IV. plate 4, fig. 69.

distinct

distinct crystal of which was 3,950; of a fragment of ruby-coloured corundum, 3,959; and of a fragment of corundum with vitreous lustre, 3,954.

It may be objected to me, that Bergman has stated the variety of specific gravity in gems to be so great, as to leave no certain rule of judging thereby of the species. He observed, that the topaz generally prevails in weight, being from 3,460 to 4,560; the ruby from 3,180 to 4,240; then the sapphire from 3,650 to 3,940\*. But, in the preceding page, he had said, "Analysi crystallorum tam ejusdem quam diversæ figuræ multum lucis scientia expectat. Illæ quarum antea compositionem explorare licuit naturali forma per artem privatæ erant." It is not, therefore, an hypothesis unworthy of examination which I advance, that gems, derived from the rectangled octoedra, whose specific gravity is above 3,300 to 3,800, will be found to be diamonds, or octoedral rubies; and these will be easily distinguished from each other by their lustre and hardness. Diamonds, whether red, yellow, blue, or white, being hardest, though their specific gravity will be less; viz. from 3,356 to 3,471, as I found among different diamonds in my collection: whereas the octoedral ruby was from 3,571 to 3,625, and inferior in hardness not only to the diamond, but to the corundum; the specific gravity of which, in its different appearance of form and colour, I found to vary from 3,876 to 4,166; and I suppose it to be subject to a variation from 3,300 to 4,300: after which the jargon will come with a specific gravity of 4,600; easily distinguished also by its crystallization from the above-mentioned gems. The above specific gravities Mr. Hatchett very obligingly assisted me in taking with his accurate scales, in the temperature of 60°. It will not be understood that I depend entirely on the specific gravity; on the contrary, I connect this quality with crystallization: hardness is the next criterion; and analysis must separate the component parts, and demonstrate the analogy, or identity, of substances, or of compounds. The improvements of Mr. Klaproth's process are evident by the comparison of his first analysis, and in his last analysis of corundum.

In the first it consisted of

Corundum earth	-	-	-	-	68	0
Siliceous earth	-	-	-	-	31	50
Iron and nickel	-	-	-	-	0	50
						<hr/>
						100
						<hr/>

By the last analysis of Mr. Klaproth, the corundum of the peninsula of India consisted of

Argillaceous earth	-	-	-	-	89	50
Siliceous earth	-	-	-	-	5	50
Oxide of iron	-	-	-	-	1	25
Lofs	-	-	-	-	3	75
						<hr/>
						100
						<hr/>

\* De Terræ Gemmarum. Berg. Opusc. vol. II. p. 204.

The corundum of China,

Argillaceous earth	-	-	-	-	84	0
Siliceous earth	-	-	-	-	6	50
Oxide of iron	-	-	-	-	7	50
Loss	-	-	-	-	2	0

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100

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That the analysis of sapphire of Mr. Klaproth may be compared, it is here added.

Argillaceous earth	-	-	-	-	98	50
Calx of iron	-	-	-	-	1	0
Calcareous earth	-	-	-	-	0	50

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100

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Iron-ore crystallized is often mixed with the Chinese corundum, as I have before stated, and may be considered as accidentally interposed, not combined. In the corundum of the coast, the greenish colour may indicate the combination of iron, as the blue colour does in the sapphire; and the proportion of iron in both is nearly alike.

There, then, is the  $\frac{5}{100}$  and  $\frac{6}{100}$  of filix in corundum, evidently an integral part of the coarse corundum crystal, and not of the sapphire; but it will require an analysis of the vitreous or pellucid corundum to decide that filix is a constituent part of corundum: there will then remain to account for the calcareous earth; and, having established its being a constituent part of the sapphire, the small proportion of  $\frac{0}{100}$  cannot be expected to produce a very notable difference.

It is not necessary to do more than thus to hint at what further analysis and examination of former experiments are required, to ascertain the analogy or identity of the sapphire and oriental ruby with corundum.

I have before stated, that I have corundum (which has the same texture and fracture as the common colourless corundum) of a ruby red, and also of sapphire blue, and of sapphire blue and white colours.

I have sapphires yellow and blue, white and blue, brown and greenish, and of a purplish hue; these I should consider as corundum, with fracture of vitreous lustre.

(To be continued.)

## II.

*An Account of a singular Halo of the Moon. In a Letter from WILLIAM HALL, Esq. of Whitehall, F.R.S. Edin. to Sir JAMES HALL, Bart. F.R.S. Edin.\**

DEAR SIR JAMES,

Whitehall, near Berwick, April 2, 1796.

I SEND, under cover, the representation of a very singular halo of the moon (pl. XXII. fig. 25.) seen here on the night of the 18th of February last, about ten o'clock; and this I

\* Edinburgh Transactions, vol. IV. page 173.

have hitherto delayed, in order, if possible, to gain farther information in the neighbourhood concerning it.

During the short continuance of the small halo, which did not exceed ten minutes after I got notice of it, I could not lay my hands on any other instrument, to take the angles, but a Siffon's theodolite, which unluckily having been constructed so as not to admit of a vertical angle so great as the moon's altitude then was, I laid it aside, not recollecting that it might have measured several of the smaller angles. But I observed sundry marks, from which I took the angles as exactly as I could next day.

The moon was about south-west, and her altitude nearly  $54^{\circ}$ , which, of consequence, was also the highest altitude of the limb of the greater halo where it was highest, and where it passed through the moon; the altitude of its opposite limb was  $14^{\circ}$ ; so that its diameter subtended an angle of no less than a hundred and twelve degrees.

The diameter of the small halo, which appeared to be a perfect circle, with the moon in its centre, I found, after repeated trials, was under  $12^{\circ}$ , and more than  $8^{\circ}$ ; but as the different diameters of the large halo were not measured, it cannot positively be affirmed, that it was an exact circle; on the contrary, its limb did not seem to intersect the small circle quite so much at right angles as the circular arch delineated in the plan. It may, therefore, have been somewhat elliptical.

The small circle was remarkably bright, particularly at West Refton, about five miles to the northward, the only other place where the halo was observed, and where it was thought to send forth flame. The small halo also continued there much longer than here, where some thin fleecy clouds soon put an end to it; but the large halo continued with us near an hour.

The weather about this time was, for the season, remarkably mild, particularly on the day of the halo. The sky was pretty clear all that day, and also in the evening; but, at the time of the halo, there was a small degree of haziness, particularly towards the north, which did not, however, prevent the moon from shining with brightness; and the stars were even visible within the circle of the small halo: there was little or no wind.

The circles, or belts, of both halos are represented in the plan nearly of their apparent breadth, or, perhaps, a little broader: the light of both was whitish, and considerably bright, without colour; that of the large circle was the paler of the two, particularly where it passed through the small circle: to the northward it was somewhat obscure.

By means of the angles taken as above, after having ascertained, on a vertical circle of the heavens, the situations of the moon, of the small halo, and of the north-eastern limb of the large halo, whose south-western limb passed through the moon, the whole was projected on the horizontal plane, as in the figure already referred to. The moon, a little more than half, is placed in the centre of the smaller halo; and both halos are represented in their true situations, relatively to the horizon, and in the circular shape which they appeared to have; though they ought, perhaps, to have been somewhat foreshortened, and thrown into an elliptic form.

This halo, as you will see by the above description, appears to be of the kind called by the learned a *corona*; and as it somewhat resembles the famous one of the sun observed at

Rome



Rome in the year 1629, and described by Scheiner\*, it deserves the more attention, especially as the great halo on the present occasion, having its south-western limb elevated to the height of  $54^{\circ}$ , and its north-eastern depressed to within  $14^{\circ}$  of the horizon, was in an oblique position, not easily reconciled with the theory of Huygens, which seems to require that such circles should be equally elevated above the horizon all round. It also shews that Scheiner's original plan of the halo at Rome, which represented it as oblique, may have been right, and that Huygens' correction, which makes it parallel to the horizon, was probably an erroneous conjecture.

I am,

dear Sir James,

your humble servant,

WILLIAM HALL.

### III.

*Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.*

(Continued from page 426, vol. II.)

**EXPERIMENT XXIII.** The two tubes A B C, D E F, fig. 14. Plate XXII. are 15 inches long; their diameter is 14,5 lines. The conical portions A, D, have the form of the contraction of the vein of fluid, and are applied to the orifice P, fig. 1. Plate VIII. of the present volume, which is 18 lines in diameter, with 32,5 inches depth, or charge of superincumbent fluid. The elbows, or flexures, B C, E F, are made in the plane of the horizon. These two pipes are made of copper foldered with silver, and the workmanship carefully executed. The curvature B C was drawn out, or bended, into the form of a quarter of a circle, by filling the tube with melted lead, in order that it might preserve its diameter during the act of bending. The elbow D E F is constructed in a right angle. The expenditure through these two tubes was compared with that afforded through a right-lined cylindrical tube of similar dimensions, and in like circumstances. The four cubical feet of water flowed out of the cylindrical tube in 45"; out of the curved tube A B C in 50"; and out of the angular tube D E F in 70".

4. It is of importance that the tube B C, fig. 13. Plate VIII. should be of an equal diameter throughout. It is not enough that care be taken that there shall be no contraction, it is also necessary that it should not be enlarged at any part. For such enlargements have nearly the same bad effect in the expenditure as contractions. The pipe A O, fig. 12. affords a much less quantity of fluid with the dilatations D E, H I, than if it were of a diameter equal to that at B throughout its whole length. The following experiment agrees with the theory.

\* Smith's Optics, vol. I. § 534.

*Experiment XXIV.* The circular orifice A, fig. 12. has the form of the contraction of the vein, and the remaining part of the tube is interrupted by various enlargements of its diameter. This tube is applied to the aperture P, fig. 1. The dimensions of its parts measured in lines are as follows. Diameter at A = 11,2. Diameter at B, C, F, G, &c. = 9. Length of B C = F G, &c. = 20. Length of C D = E F = G H, &c. = 13. Diameter of the enlarged parts = 24. The length of each of the enlarged parts was variable. The first time of trial it was 38 lines, the second 76, and the result of the experiment was the same in both cases.

Number of enlarged parts.	Time during which four cubical feet issued out.
0	109"
1	147"
3	192"
5	240"

I afterwards applied to the same orifice a tube, having the same form, and the same diameter, as A B C, but cylindrical throughout, without any enlargements, and its length was 36 inches, the same as that of the tube with five enlarged parts; in this case the expenditure of four cubical feet was made in 148".

When the fluid passes from C to the middle of the enlarged part D E, part of the motion is diverted from the direction C F towards the lateral parts of the enlargement. This part of the motion is consumed in eddies, or against the sides. Consequently there remains so much the less motion in the following branch F G. This is also the cause which destroys, or weakens, the pulse in the arteries beyond an aneurism.

From this consideration we are justified in concluding, that if the internal roughness of a pipe diminishes the expenditure, the friction of the water against these asperities does not form any considerable part of the cause. A right-lined tube may have its internal surface highly polished. Throughout its whole length, it may every where possess a diameter greater than the orifice to which it is applied; but, nevertheless, the expenditure will be greatly retarded if the pipe should have enlarged parts, or swellings. This is a very interesting circumstance, to which, perhaps, sufficient attention has not been paid in the construction of hydraulic machines. It is not enough that elbows and contractions are avoided; for it may happen, by an intermediate enlargement, that the whole advantage may be lost, which may have been procured by the ingenious dispositions of the other parts of the machine.

#### PROPOSITION VIII.

*In the machine for blowing by means of a fall of water, the air is afforded to the furnace by the accelerating force of gravity and the lateral communication of motion, combined together.*

The Academy of Toulouse, in the year 1791, invited philosophers to determine the cause and the nature of the stream of air which is produced by the fall of water in certain forges. I propose, in this place, to develop the complete action of this kind of blowing apparatus, and to ascertain the best form of construction. Kircher is the first I know of who has explained

plained the production of wind by a fall of water\*. Barthes, the father, has given a theory which appears to me to be defective in many respects†. Dietrich was of opinion, that this wind is produced by the decomposition of water‡. Fabri had a similar notion in the last century§. Most philosophers are well acquainted with this kind of engine||.

I shall begin with an idea, the foundation of which did not escape the penetration of Leonardo Da Vinci. Suppose a number of equal balls to move in contact with each other along the horizontal line A B, fig. 16. Plate XXII. Imagine them to pass with an uniform motion, at the rate of four balls in a second. Let us take B F, equal to 16 feet English. During each second four balls will fall from B to F, and their respective distances in falling will be nearly B C = 1, C D = 3, D E = 5, E F = 7. We have here a very evident representation of the separation, and successive elongation, which the accelerating force of gravity produces between bodies which fall after each other.

The rain water flows out of gutters by a continued current; but during its fall it separates into portions in the vertical direction, and strikes the pavement with distinct blows. The water likewise divides, and is scattered in the horizontal direction. The stream which issues out of the gutter may be one inch in diameter, and strike the pavement over the space of one foot. The air which exists between the vertical and horizontal separations of the water which falls, is impelled and carried downwards. Other air succeeds laterally; and in this manner a current of air, or wind, is produced round the place struck by the water. I went to the foot of the cascades which fall from the Glacière of La Roche Mélon, on the naked rock at La Novalèse, towards Mount Cenis, and found the force of the wind to be such as could scarcely be withstood. If the cascade falls into a basin the air is carried to the bottom, whence it rises with violence, and disperses the water all round in the form of a mist.

The water which is precipitated in the hollow internal parts of mountains carries the air with it, which afterwards issuing forth from apertures at the foot of the mountain, produces those natural blasts, those *ventaroli*¶, which are most frequently observed in the volcanic mountains, because these mountains are most commonly hollow within.

Let B C D E, fig. 16. represent a pipe, through which the water of a canal A B falls into the lower receiver, M N. The sides of the tube have openings all round, through which the air freely enters to supply what the water carries down in its fall. This mixture of water and air proceeds to strike a mass of stone Q; whence rebounding through the whole width of the receiver M N, the water separates from the air, and falls to the bottom at X Z, whence it is discharged into the lower channel or drain, by one or more openings, T, V. The

\* Mundus Subterr. lib. XIV. cap. 5, edit. 1662.

† Mémoires des Savans étrangers, vol. III. p. 378.

‡ Gites de Minéral des Pyrénées, p. 48, 49.

§ Physic. tract I. lib. II. prop. 243.

|| Art des Forges, part II. Mariotte des Eaux, part I. disc. III. Transact. No. 473, &c.

¶ These *ventaroli* are sometimes produced by the difference of temperature between the air of the cavern and the external air. V. From the effects they seem to be oftener produced by this last cause, than by a fall of water. On this subject in general, namely, the cold winds which issue out of the earth, see Philos. Journal, I. 229.—N.

air being less heavy than the water, occupies the upper part of the receiver, whence being urged through the upper pipe O, it is conveyed to the forge.

*Experiment 25.* I formed one of these artificial blowing engines of a small size. The pipe B D was two inches in diameter, and four feet in height. When the water accurately filled the section B C, and all the lateral openings of the pipe B D E C were closed, the pipe O no longer offered any wind.

It is, therefore, evident that in the open pipes the whole of the wind comes from the atmosphere, and no portion is afforded by the decomposition of water. Water cannot be decomposed, and transformed into gas, by the simple agitation and mechanical percussion of its parts. The opinion of Fabri and Dietrich have no foundation in nature, and are contrary to experiment.

It remains, therefore, to determine the circumstances proper to drive into the receiver, M N, the greatest quantity of air, and to measure that quantity. The circumstances which favour the most abundant production of wind, are the following.

It is known that in the parabola, if  $d x$  be assumed as constant,  $d y$  will decrease in the ratio of  $\frac{1}{\sqrt{x}}$ . The separation of the balls in fig. 15. is more rapid in the upper spaces of the fall than in the lower. In order, therefore, to obtain the greatest effect from the acceleration of gravity, it is necessary that the water should begin to fall at B C, fig. 16. with the least possible velocity; and that the height of the water F B should be no more than is necessary to fill the section B C. I suppose the vertical velocity of this section to be produced by an height or head equal to B C.

2. We do not yet know, by direct experiment, the distance to which the lateral communication of motion between water and air can extend itself; but we may admit with confidence, that it can take place in a section double that of the original section, with which the water enters the pipe. Let us suppose the section of the pipe B D E C to be double the section of the water at B C; and in order that the stream of fluid may extend and divide itself through the whole double section of the pipe, some bars, or a grate, are placed in B C, to distribute and scatter the water through the whole internal part of the pipe.

3. Since the air is required to move in the pipe O with a certain velocity, it must be compressed in the receiver. This compression will be proportioned to the sum of the accelerations, which shall have been destroyed in the inferior part K D of the pipe. Taking K D = 1,5 feet, we shall have a pressure sufficient to give the requisite velocity in the pipe O. The sides of the portion K D, as well as those of the receiver M N, must be exactly closed in every part.

4. The lateral openings in the remaining part of the pipe B K, may be so disposed and multiplied, particularly at the upper part, that the air may have free access within the tube. I will suppose them to be such that 0,1 foot height of water might be sufficient to give the necessary velocity to the air at its introduction through the apertures.

All these conditions being attended to, and supposing the pipe B D to be cylindrical, it is required to determine the quantity of air which passes in a given time through the circular section K L. Let us take in feet K B = 1,5; B C = B F =  $a$ ; B D =  $b$ . By the common theory of falling bodies, the velocity in K L will be  $7,76\sqrt{(a+b-1,4)}$ ; the circular section K L =  $0,785a^2$ . Admitting that the air in K L to have acquired the same velocity as the

the water, the quantity of the mixture of the water and air which passes in a second through K L is  $=6,1 a^2 \sqrt{(a+b-1,4)}$ . We must deduct from the quantity  $(a+b-1,4)$  that height which answers to the velocity the water must lose by that portion of velocity which it communicates to the air laterally introduced; but this quantity is so small that it may be neglected in the calculation. The water which passes in the same time of one second through B C is  $=0,4 a^2 \sqrt{(a+0,1)}$ . Consequently, the quantity of air which passes in one second through K L, will be  $=6,1 a^2 \sqrt{(a+b-1,4)} - 0,4 a^2 \sqrt{(a+0,1)}$ , taking the air itself, even in its ordinary state of compression, under the weight of the atmosphere. It will be proper, in practical applications, to deduct one-fourth from this quantity; 1, on account of the shocks which the scattered water sustains against the inferior part of the tube, which deprive it of part of its motion; and, 2, because it must happen that the air in L K will not, in all its parts, have acquired the same velocity as the water.

If the pipe O do not discharge the whole quantity of air afforded by the fall, the water will descend at X Z; the point K will rise in the pipe, the afflux of air will diminish, and part of the wind will issue out of the lower lateral apertures of the pipe B K.

I shall not here examine the greater or less degree of perfection of the different forms of water-blowing machines, which are used at various iron forges, such as those of the Catalans, and elsewhere. These points may be easily determined from the principles here laid down.

#### PROPOSITION IX.

*It is possible, by means of a fall of water, to drain a piece of ground, without the help of machines; even though the ground should lie on a lower level, than the established current, below the fall.*

The means of doing this is pointed out in the first experiment of this treatise. We have seen that the water contained in the vessel D E F B, fig. 3. Plate VIII. issues through the channel M B V, which is higher than the surface of the water itself, because the fluid which passes through A C carries with it the water contained in the vessel.

In the artificial fall, which is procured in channels to give motion to mills, when the water rushes down by a rectangular trunk of wood, D B C F, fig. 17. placed nearly horizontal in the middle of the lower channel, the surface of the water at K is one or two feet beneath the inferior current (or back-water) F L\*. The water at F tends to return and descend along F K; but the current, by its lateral action, constantly carries it away, and does not permit it to slide down to K. If an opening G be made in the lateral sides of the trunk, the waters from lands lower than the current of the inferior stream F L may be drained off. In a commission with several of my colleagues, I once proposed, that this principle should be applied to a case of practice. The project was adopted, and the drainage succeeded very well.

The rectangular conduit D D F C must be prolonged to a certain extent along the lower channel, otherwise the water might flow back from F to K, and oppose the drainage through G. The mill-wrights are aware of the utility of this prolongation. Experiment has taught

\* This depression of the level has already been noticed in K. Guicciardi, della natura de fiumi, cap. 7, fig. 46. Boffut, art. 721. The wheel alluded to in the text must, I presume, be of that kind which we call a breast-wheel.—N.

them, that it prevents the water from returning back so readily in the time of floods, which might check the motion of the water-wheel. For this purpose, they make the upper part D F at the height of the waters, which the mill-stream can resist or support. The town of Final, in the territory of Modena, having charged me with the direction of changing the course of part of the waters of the Panaro, which the circumstances of the town required to be done; I availed myself of this prolongation of the tail-pipe D F, combined with other artifices, to maintain the action of mills in the new channel; and I succeeded not only beyond the expectation of the inhabitants, but even beyond my own hopes.

#### PROPOSITION X.

*The eddies of the water in rivers are produced by motion, communicated from the more rapid parts of the stream to the lateral parts, which are more at rest.*

Few authors have examined the cause and the effects of the eddies of water in rivers; and those who have undertaken this investigation, do not appear to have been very happy in their researches.

The water which moves in the channel M N H, fig. 19. meets the obstacle B A, which impedes its course, and causes it to rise and discharge itself in the direction A C with an increased velocity. Suppose the water in B D C A to be dormant, the current A C communicates its motion to the lateral particles E (Prop. I.), and conveys them forward; the surface of the dormant water becomes depressed at E, and the most remote particles towards D are urged according to the laws of the equilibrium of fluids to fill the depression. The current A C continues to carry them off, and the space B D C A continues to be exhausted. The water of the current A C, by virtue of the same laws, is acted upon by a constant force which urges it towards the cavity E, while its natural course or projection carries it towards A C. Under the agency of these two forces, the water A C acquires a curve-lined motion in C D, and descends as it were through an inclined plane, becoming retrograde in D E, whence it would proceed to strike the obstacle B A, and the current A C, after which, it would undergo several oscillations previous to acquiring a state of equilibrium and repose. But the current A C continues its lateral action: a second time it draws away the water through C D into E, and forces it to renew its motion through the curve C D E; in which manner the eddy continues without ceasing.

If the river should pass through a contraction of its bed at N, it will produce eddies on both sides, at P and at Q, similar to those we have contemplated at D C.

Suppose the stream of water, after having struck the bank G H, to be reflected into a new direction H S, the lateral communication of motion will excite eddies in the angle of reflection R.

When two currents of unequal velocity meet obliquely in the middle of the river, the most rapid current will produce eddies in that which is the least rapid.

Suppose a stream of water to flow over a bed of unequal depth. If the longitudinal section of the inequalities of the bottom exhibit a gentle slope, as at A B C, fig. 20. the superior water will impress its motion by lateral communication upon the inferior water, which is near the bottom, beneath the line A C, and a current will take place through the whole depth of the

the section M B. The current which is formed near the bottom at B, is turned out of its course by the slope B C, and proceeds to rise above the surface at Q; sometimes in the form of a curling wave or vertical whirlpool. If the extremities of the hollow place form an abrupt angle, as D E, F G, eddies will be produced even at the bottom, in the vertical direction at D, and sometimes also at G. These phenomena may be observed in an artificial channel with glass sides.

*Every eddy destroys a part of the moving force of the current of the river.* For the water which descends by a retrograde motion, in the inclined plane C D E, fig. 19. cannot be restored in the direction of the current of the river but by a new impulse. It is as it were a ball, which is forced to rise on an inclined plane, whence it continually falls back again to receive new impulses. It is the labour of Sisyphus.

Hence I deduce, as a primary consequence, *that in a river, of which the course is permanent, and the sections of its bed unequal, the water continues more elevated than it would have done, if the whole river had been equally contracted to the dimensions of its smallest section.* The cause of this phenomenon is the same as that which retards the expenditure through the tube with enlarged parts. (Prop. VII. No. 4.) The water which descends from the elevation above the contracted part N, into the basin P Q, fig. 19. loses nearly the whole of the velocity it acquired by descending from it; because the narrow part has a curved slope towards the lower part of the river, which directs the velocity of the stream in an horizontal direction. Guilielmini has well remarked, that a fall does not influence the velocity of the lower stream, because the eddies of the water in the basin P Q destroy the velocity produced by the fall. This velocity increases the depth, and enlarges the width of the channel at P Q. Eddies are formed on each side, at the bottom, and at the surface; both in the horizontal and vertical directions. It would be to no purpose to attempt to prevent this hollowing out and enlargement of the channel by such a fall, by adopting the means of close walls; for the basin would then obtain its enlargement, where these constructions might end.

If the channel have a number of successive contractions and dilations, M N, without cascade or dam, there will still be formed, at each dilatation, eddies which will diminish the velocity more than if the channel had an uniform section equal to that in M or N. It will, therefore, follow, that the surface of the water, after each dilatation, must rise, in order to recover the velocity it lost by the eddies. If we call the height to which the water must rise, above the elevation necessary to have overcome the retardations of a bed of uniform section,  $= a$ , and that the number of equal and successive alternate dilations and contractions be  $= m$ , the height of the rise in the stream thus alternately dilated beyond that of the same river uniformly contracted will be  $= am$ . I here suppose the bottom of the river to be uniform. If this bottom be of such a nature to be attacked by the current, the contracted parts will be hollowed out, and the matter will be deposited in the enlarged parts.

The second consequence which I draw from the principle here established, respecting the loss of force, caused by the eddies, is of considerable importance in the theory of rivers, and appears to have been neglected by those who have treated on this subject. The friction of the water along the wet banks, and over the bottom of rivers, is very far from being the only cause of the retardation of their course, which, consequently, requires a continued descent to

maintain its velocity. One of the principal, and most frequent, causes of retardation in a river, is also produced by the eddies, which are incessantly formed in the dilatations of the bed, the cavities of the bottom, the inequalities of the banks, the flexures or windings of its course, the currents which cross each other, and the streams which strike each other with different velocities. A considerable part of the force of the current is thus employed to restore an equilibrium of motion, which that current itself does continually derange.

(To be continued.)

No. IV.

*Concerning a new Variety of argillaceous Iron-ore. By SAMUEL L. MITCHILL, M.D. of New York.*

SIR,

THE annexed description of a mineral substance, and the remarks upon it, are taken, by permission, from Dr. Mitchill's manuscripts. As the subject appears to me worthy to be made known to the mineralogists of Europe, I have forwarded you a copy for publication, in your useful and instructive Journal; I have the satisfaction to inform you at the same time, that Mr. Bruce, a young gentleman who goes passenger to London in the same ship which conveys this note, is in possession of a specimen of this remarkable fossil.

I am, Sir,

Your very obedient servant,

New-York, November 17, 1798.

ADOLPH. C. LENT, M.D.

To Mr. W. Nicholson.

*A new Variety of Iron-ore of the argillaceous Kind, and figured somewhat like Basaltes.*

The specimen of columnar iron-ore in my collection is from Germany, and corresponds very well with Mr. Kirwan's second variety of his first family of argillaceous iron-ores. (2. Elements of Mineralogy, Dublin, 1796.) I knew of no other iron-ore of that character, until a few months ago. As I was walking on my farm upon Long-island, surveying the various mineral productions that lay thick under my feet, I observed a small spot, that appeared to be paved, with stones of a regular figure; on taking up some of them and examining them, they were evidently of the same constitution with the common argillaceous iron-stone, which lay scattered about in rude lumps. They were, however, of a singular shape. They were about four inches long; and stood erect, side by side of each other, in a stiff loam. Each of them had five sides and five angles; and though not exact pentagons, were readily distinguishable in most of the specimens from one end to the other. This shape was most perfect at the two ends, which were considerably larger than the middle part, and about one inch and a half across. So that when a specimen of this ore was grasped in the hand, it had some resemblance of a double-headed pebble. In some, however, four sides only were to be traced.

The



The number of these figured bodies was about thirty-six, though not all of them equally perfect. And they evidently had been formed from a solid circular mass of argillaceous iron-ore, of about two feet and five inches in breadth, and four inches in thickness, at the central parts, and becoming thinner towards the edges. In the natural fracture of the stone, the fragments took upon themselves these regular forms, very much resembling basaltæ; and, like basaltæ, though *figured* they evidently are not *crystallized*, for they are as perfectly terigenous and opaque as any argillaceous ore of iron whatsoever.

I brought away some of the pentagonal pieces, and could easily get more of them, for in taking out the few that I brought away, I disturbed the rest as little as possible.

Do not these specimens go very far towards deciding the dispute, if any doubts still remain, about the *igneous* or *aqueous* origin of basaltæ? they support, by the most powerful evidence, the *Neptunian* origin of that kind of figured bodies, and are directly opposed to the *Plutonic* system. The specimens before me prove, that argillaceous iron-ore, which nobody has supposed to be a volcanic production, can take on a basaltic figure. With BERGMAN, WEIDEMAN, and KIRWAN, I am inclined to believe the weight of testimony is opposed to the formation of basaltic columns by *FUSION*, and that the true manner, in which many, if not all, of them have been produced, is in the *MOIST WAY*. This new proof, added to Mr. Kirwan's very able paper on *traps* and *basaltæ*, I think settles the controversy in his favour.

## V.

*Concerning the Invention of the Electrical Doubler. By Mr. JOHN READ.*

TO MR. NICHOLSON.

SIR,

**I**N your journal, Nov. 1798, page 368, you charge me with want of candour, in my description of what I call the spectacle doubler of electricity. Whether this charge be just, or not, must be left to the decision of a judicious public: who, it is hoped, will have candour enough to compare your account of the doubler with mine. I was of opinion, that after giving you the priority of invention, which I have expressly done in page 29 of my work, entitled Summary View of the spontaneous Electricity of the Earth and Atmosphere, no further acknowledgement could be required. The passage is, "and to give the plates a considerably more extended insulation than that made by Mr. Nicholson, without augmenting the size of the instrument, &c."

That your charge is frivolous, will more fully appear from the following circumstance, namely, that Dr. Priestley did actually deliver my original manuscript into your hands, with full power to correct, erase, or add whatever you chose, and after you had retained it more than three months in your possession, I received the manuscript from you, and found that you had made no alteration at all in it: of course it went to the press in the dress it is now in.

It is not my intention, at present, to criticise on all you have said in the passage alluded to. As to the adopting some of your own words into my description, of nearly the same instrument, could not well be avoided; nor can it be thought an illiberal proceeding. But if you disliked

it—if you thought it improper, you ought to have mentioned it at a proper time, viz. when you returned the manuscript, or before it was sent to the press; and your request would have been literally complied with. With regard to the instrument itself, it is at present useless; and it is allowed by all, that it has totally failed for want of a perfect insulation. If this defect should at some future time be overcome, the doubler of electricity, will *then* be the most *useful* and the most *noble* instrument in the whole group of electric apparatus.

I remain, sir,

Your very humble servant,

JOHN READ.

*Quadrant, in Knightsbridge,*

*January 17th, 1799.*

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Though the above letter relates to personal incidents, which may not, perhaps, be considered with any great degree of interest, yet I conceive my readers will admit the propriety of publishing it, in order that Mr. Read, whose candour, as an author, has been called in question, may justify himself to that public, which he has essentially served during a long life, employed as an instrument-maker and operative philosopher. I am well content, that the decision respecting the doubler, should be made by the tribunal to which he refers, and to which it of right belongs. If a careful review of what I have written, together with his present communication, had led me to alter my sentiments, it would be my duty to say so in this place. But I think I have faithfully stated the facts in the passage he alludes to, and have only to remark, that the indirect mention of my name, in page 29 of his work, was little, if at all, calculated to destroy the conclusion which his readers would obviously be induced to make, from the unacknowledged copy in the former part of the chapter; in proof of which, the inference of the foreign philosophers, who were misled in his favour as the inventor, is nearly decisive. His narrative respecting my having possessed his manuscript, previous to its publication, is not quite correct in the manner, because it does not communicate the whole of what happened. Either Dr. Priestley, or else Mr. Read with the Doctor's recommendation, did deliver to me certain manuscripts about six or seven years ago, which I believe to have been part, or the whole, of what was since published, under the title of *A Summary View, &c.* but they were not put into my hands with the notion that any relation might subsist between me and their contents, which could require the full powers Mr. Read mentions;—but simply to correct and prepare them for the press, on condition of being paid for my labour. As I have always declined this kind of employ, excepting when motives of personal acquaintance or friendship have led me to it, I returned this copy untouched, and unperused, to the author, and supposed of course, that he had employed some other person to revise it. I hope and believe, therefore, that notwithstanding this incident, upon which Mr. Read seems to place so much reliance, it cannot be thought to afford any ground for an insinuation, that I have been induced to speak as I have done, of his share in the invention of the doubler, from any motives, but such as ought to guide an independent narrator of philosophical facts.

With

With regard to the instrument itself, which is so truly honourable to the sagacity of Lichtenberg, Klinkock, Volta, and Bennett, but in which my share is certainly very trifling, I do not know that its imperfections are greater than was long since ascertained by the English electricians; of which an account is given in a paper, at page 394 of the first volume of this Journal. If Mr. Read is in possession of any new facts respecting its operation, the public will, no doubt, receive them with as much attention and respect, as they have paid to the results of his former pursuits.

## VI.

*Inquiries respecting the Construction of a Water-wheel, and the Manufacture of Bricks.**By a Correspondent.*

Mr. Nicholson will oblige a friend to his useful Journal, by giving, therein, information on the under points.

December, 1798.

ON a stream where the fall is  $6\frac{1}{2}$  feet, what diameter ought the water-wheel to be?

Whether a bucket-wheel, or float-board-wheel?

If the former, ought the water to be brought in at the height of the fall; or a portion given for head? and what portion?

Is there any practical direction in print, to direct the process of brick-making?

If there is not, Mr. N. would do a material service to numbers, who are in some parts of the country, remote from the workmen who are skilled in this art, by collecting and publishing such practical directions of the nature of the materials and the process, as will enable the uninformed to supply themselves with this useful article.

The data, respecting the above-mentioned stream, are not sufficiently precise to determine whether a bucket-wheel, or float-board-wheel, would be preferable: the quantity of water afforded per minute ought to have been mentioned. The bucket-wheel appears, upon the whole, to be best adapted to small streams of water with a considerable fall; but, in contrary circumstances, the close breast-wheel appears to be preferable, that is to say, a wheel with float-boards, moving in a channel so well fitted, as to permit the least possible quantity of water to escape, without acting upon the wheel.

The diameter of the wheel may, in theory, admit of considerable variation. Admitting it to be a breast-wheel, its radius must be somewhat more than the height of the fall. The velocity of the surface of such wheels, as are driven by the gravitating power of water, lies between two feet and six feet per second. Three feet may be considered as a good practical velocity, as determined by experience. It is of no advantage, but, on the contrary, a loss, to consume any part of the head, in throwing the water against the floats with a considerable impulse; it is only necessary that the stream should be delivered upon the wheel with some-

what

what more velocity than that of the wheel itself. If the stream can be brought on with the velocity of three feet at the top of the fall, it will, undoubtedly, be best; but if the water should flow from a pond, having no perceptible current, the portion or space required to produce this velocity will be about one foot and a half.

These remarks are equally applicable to bucket or overshot wheels.

The art of brick-making is concisely described in the French Encyclopedie. Bergmann has attended to this subject in a direct chemical way. A translation of his treatise into English, may be seen in the third volume of his *Physical and Chemical Essays*; or an abridgement of the same in my *Chemical Dictionary*, article Brick. As the art itself is of extensive utility, and of considerable simplicity in the practice, I should, in this place, have described the process, with remarks, if I were not desirous of again inspecting the methods which are practised in the neighbourhood of this metropolis. When I shall have done this, the account he desires will appear.

## VII.

### *On the Combustion of Phosphorus.*

TO MR. NICHOLSON.

SIR,

UPON putting a small piece of phosphorus in a shovel over the fire, in some experiments I lately made for the entertainment of a company of friends, that substance smoked, began to melt, and took fire as usual. When the combustion had ceased, and we were looking at the ruddy coloured residue, or acid, one of the company wetted the residue by spitting upon it; at which instant, the combustion was renewed, with a crackling noise, at every part where the moisture had reached. I suppose the residue to have been oxygenated phosphorus, or phosphoric acid; and that the addition consisted chiefly of hydrogen and oxygen. What then was the new combination, which was attended with the farther extrication of heat? Will you have the goodness to explain this fact, or submit it to your correspondents.

I am, Sir,

Your obliged reader,

January 4, 1799.

R. S.

I have complied with the request of this correspondent, by publishing his letter; upon which my other friends will make their remarks, if the subject should appear to require it. If phosphoric acid, at an elevated temperature, in contact with iron, can be made to emit heat and light by the addition of water, as R. S. apprehends, the true explanation of the play of affinity must be sought from varied experiments, in which all the products, as well gaseous as fixed, should undergo examination. But in the instance before us, I am much inclined to think the renewed combustion is produced by a portion of phosphorus, which escaped the atmospheric action in the first burning. That is to say, when a piece of phosphorus is burned,

burned, in the circumstances of the present experiment, the upper or greatest part of the mass becomes acidified, and covers a portion beneath, so as to prevent that lower part from combining with the atmospheric oxygen; in consequence of which, the combustion ceases before the whole is burned. But when this dense acid becomes diluted with water, it boils, and most probably rises, at least so far as to leave the surface of the phosphorus below uncovered, and to permit the combustion to be renewed. Instances of volatility being given to various substances by water, are sufficiently numerous in chemistry; of which the acid of borax is one of the most striking. To what degree, the same effect may take place in the phosphoric, I am not aware. Scheele (on Air and Fire, §. 73), indeed, affirms, that water gives it fixity; but he says this in so loose a way, that I am not disposed to rely on the assertion, as universally true, in contradiction to some other facts, which shew that the diluted acid does partly rise by heat.

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## VIII.

*Pyrometrical Essays to determine the Point to which Charcoal is a Non-conductor of Heat.*

*By Citizen GURTON\*.*

SINCE the experiments and researches of philosophers have been directed to the matter of heat, the state in which it is found, and the various manners in which it affects bodies, it has been well ascertained that charcoal is one of the worst conductors of heat. From this observation it has been proposed to form a double wall to furnaces, and to fill the intermediate space with charcoal; and, very recently, an happy application of this property has been made in the construction of vessels designed for the preserving the temperature of warm infusions.

I am not, however, acquainted with any researches which may have been made to fix, even comparatively, the limits of this kind of insulation. The effect of the non-conducting power of charcoal has been so slightly considered, even by the most accomplished chemists, that they have not thought it necessary to take any account of this effect in the result of their operations. Hence it is, that the celebrated Klaproth, in a series of experiments upon the alteration which stones undergo when exposed to extreme heat, seems to think that the heat might have been nearly equal in such as were contained in crucibles of porcelain, and those which were entirely surrounded with charcoal. The reduction of the oxides of tungsten, titanium, and uranium, has also been attempted in crucibles of charcoal, though it is well known that the highest degree of heat is here required for the fusion, and that, in other respects, every endeavour must be made to expose them to the most intense ignition.

These reflections have led me to conclude, that it might be of some importance to obtain a more accurate measure of this insulating effect of charcoal. The following are the experiments which I have made upon this subject.

Out of the same parcel I took two pyrometric pieces perfectly alike, and placed one, which I shall call A, in a crucible filled with pure siliceous sand dried over the fire. The crucible

\* Read to the French National Institute, 6th Germinal, in the 6th year of the Republic (March 26, 1798), and inserted in the *Annales de Chimie*, XXVI. 225.

was 8 decimeters high, 6 in diameter at the mouth: the cylinder of clay was placed in the middle, and the crucible had its cover luted on.

The other piece, B, was placed in a similar crucible, with this difference, that the crucible was filled with powder of charcoal, which had been previously ignited in a close crucible.

The two crucibles were then placed beside each other on the grate of a large melting furnace, in which the fire was kept up for about three quarters of an hour.

When the crucibles were cooled, the cylinder A was taken out of the sand, and presented to the pyrometric scale of Wedgewood; it had undergone a contraction of 89 degrees. The cylinder B was then taken out of the charcoal, and stopped in the gage at 60,25 degrees. It had acquired a grey tinge, but without any appearance of glazing.

Hence it follows, that the transmission of heat through the sand, is to the transmission through the charcoal nearly in the proportion of 3 to 2. In proportion as this difference is in itself striking, the more it becomes necessary to attend to such precautions as are required to prevent deception arising from foreign circumstances. The crucibles had most assuredly undergone the same degree of fire. The state in which they were found exhibited the traces of its action: the conditions were therefore as equal as possible; but it might, possibly, be suspected that some defect, or want of uniformity in the pyrometric piece, or some imperfection in its composition, or fabrication, might have altered its disposition to contract equally, and proportionally to the heat it might undergo. There was a very simple method of removing these doubts; namely, to ascertain whether the same piece put into the sand, and exposed to a much stronger heat, would resume the common course of contraction, and agree with the former. This, in fact, was performed; the same two pieces, A and B, were inclosed in one and the same crucible filled with sand, so that they were not more distant from each other than about 7 or 8 millimetres, and the crucible was exposed for half an hour to the most violent heat of a forge, urged by three twyers.

The crucible, when cold, was found to have lost some of its thickness by vitrification, so that there was a crack in one of its sides. The sand, however, was not deranged within.

The piece A marked 163,5 upon the pyrometric scale, it weighed no more than 1,491 grammes; its specific gravity was 2,232.

The piece B exhibited 160 degrees on the pyrometer, it weighed 1,53 grammes, and its specific gravity was 2,346. It had almost lost the grey tinge which it had acquired in the charcoal, and was no longer distinguishable from the other but by a black vitreous point, produced by the accession of some foreign matter.

I confess I did not expect to be so completely successful in this verification: the small difference of 3½ degrees is nothing, when we consider that the piece first inclosed in the charcoal, and which had stopped at 60, was still capable of contracting through an additional 100 degrees. It is, besides, known to be physically impossible, that two bodies placed in the same crucible, and in contact with the same substance, should be strictly in the same situation as to the reception of heat, particularly when the blast is directed from three different nozzles, which are necessarily unequal. The advanced state of one of the sides of the crucible, with respect to the fusion, is a proof that this was in fact the case.

We may therefore conclude from these essays, that the body included in the charcoal in  
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the same fire, does not receive more than about two-thirds of the heat of a body surrounded with quartzose sand; that the reduction of metals which do not flow at a lower degree of heat than 130 degrees, cannot be effected in charcoal; that the pyrometric pieces do not afford an accurate judgment of the heat communicated, unless they be in contact with substances of the same kind as those which surround the body, on which the chemical process is performed; and lastly, that with these attentions we may make an advantageous use of this instrument, of which the applications will not be sufficiently known, until we shall be able to procure, at pleasure, cylinders fabricated after the manner of Wedgewood, of a clay susceptible of losing 0,18 of its volume by contraction; or, which is the same thing (supposing no elastic matter to fly off), of passing from the specific gravity of 2,05 to 2,45, between the degree of ignition required for baking the biscuit of earthen ware, and the highest heat of furnaces, without undergoing a commencement of fusion.

I have made a great number of experiments, with the intention of discovering a clay which might naturally exhibit these properties, or which might acquire them by easy and cheap preparation. I hope I shall be shortly able to communicate satisfactory results to the class of the institute.

## IX.

*Description of an Air-pump, of a new Construction. By the Rev. JAMES LITTLE, of Lacken, in the County of Mayo, in Ireland\*.*

THE pump here described is a portable one, contrived so as to be confined in a very small space; but the author observes, that it may be made of a different form, and with two barrels, though not conveniently.

Fig. 1. plate XXI. is a perspective view of the whole machine, as it lies before the operator. Fig. 2. is a back view of the same.

The barrel A A (fig. 1.) is almost fifteen inches long, and internally in diameter just two inches. The piston (fig. 3.) is solid, without any perforation; it consists of circular plates of leather, pressed together between the round plate *a* (in the socket of which, *b*, the piston rod was inserted, and fastened by a cross-screw, before the leathers were put on), and the two upper plates *c* and *d*; of which the outer one, *d*, being screwed upon a screw formed on the outside of the socket, presses down the under plate, and thus the leathers, which latter are not represented in the figure. The upper plate, when screwed on, lies even with the upper part of the socket, that when the piston is raised to the top, it may touch the plate covering the upper part of the barrel, leaving no air above the piston. These two plates of the piston, especially the lower one, are made so large as just to move in the barrel without touching it, and care was taken that when the piston is put down, it should be every where in contact

\* Irish Transactions. VI. 319. The paper from which the above is abstracted, could not be given on account of its extent. It occupies 77 pages. Some of the early air-pumps were made with an horizontal barrel. There is a plate of one in Senguerdii Philosophia Naturalis, the second edition of which, in 4to, was published at Leyden, 1685.

with the plate at the bottom of the barrel, this plate being turned in the lathe upon the piston rod, which fitted its socket exactly, so that not the least space might be left for lodgment of air under the piston. The leathers are of the best buck-skin, dressed in the usual manner, firm in its texture, but not harsh; and, being well dried, were soaked in a mixture of three parts suet, melted with one part oil, before they were put together: they were then, when cold, turned in a lathe on the piston with a very sharp tool\*.

The cylindrical iron rod of the piston is well finished, and moves through a collar of leathers. As it is altogether necessary that this rod should move most exactly in the middle, or axis, of the barrel, care was taken that the position of the box, and of the socket annexed to it, should be secured, by having a prominent part of the plate of the box of leathers inserted into the pump barrel, and a like projecting part of the lid of the box inserted into the box itself; also that the piston rod should most exactly fill the socket in the lid. Over the collar of leathers, within the box, lies a tinned brass plate, which is pressed down upon them by the ends of three screws 3 3, screwed through the lid of the box.

The piston is moved up and down by the toothed flat bar, or rack, F, whose end is furnished with an arm G (to be taken off occasionally), which arm is secured against a shoulder formed on the end of the piston rod by a nut H screwed on the rod; the rack is moved by a small steel wheel L, of twelve teeth, turned by the handle X on its axis L. This wheel is sustained by the cheeks ||K K, fig. 1. and 2. furnished with projecting sockets, through which its axis passes, and these cheeks are screwed to the iron bar M, which is a part of the frame supporting the whole machine; by this bar it is fastened by clamps to whatever table it is placed on. From the middle of the bar, and at right angles with it, extends horizontally an arm N (rivetted, and brazed upon the bar), the under surface of the arm being on a level with the upper one of the bar; this arm supports the gage-glass Y, and terminates in a cross-piece (making together the form of the letter T), to which piece and to the arm the receiver-plate is screwed by screws underneath, inserted into its thick margin.

The bar M supports the upright pieces, or pillars, o, o, terminating in the semicircular arms p, p, on which the barrel is fastened by four screws at p, p, screwed a little way into its projecting rings. These pillars are made of iron, and very strong, being half an inch in thickness; they are also most firmly fixed, and should be brazed in the bar which supports them; this being necessary, as all the force of the handle of the wheel, by which the rack is worked, bears against these pillars; and if they were to be shaken, the cement, by which the receiver-pipe is connected with the pump, would be broken, which cannot otherwise happen. These pillars support also the iron case, or sheath, P P, extended underneath the pump-

\* If tanned leather were used for the piston, &c. it might be soaked in oil alone; but, besides that, it is apt to grow too hard by compression, and also to corrode the brass of the barrel more, from the acid imparted by the bark, with which it is tanned; it also emits a greater quantity of elastic fluid in vacuo than leather which is not tanned. On these accounts, I have used buck-skin leather for the piston; but the texture of this being very loose, oil only would not fill its pores, so as to make the piston air-tight, even when the leathers were closely pressed; and it was necessary to fill its grain with the above stiffer composition. Yet this leather will also generate air, and is harder to be made air fast; it also moves so stiffly in the barrel, that, perhaps, good shoe leather is preferable to it.—L.



barrel, within which caselides the toothed bar, or rack, F, moved by the wheel, and this rack (with its case) is fixed exactly parallel to the axis of the cylinder, that it may draw out the piston rod precisely in that direction in which itself moves: the bar is  $\frac{1}{8}$  of an inch in thickness, but is an inch in breadth from the bottom of its teeth to the back of it. It is made thus strong, that the end of it may not be strained upward from the proper direction, when it is urged forward by the wheel, and yet checked by the piston rod, after the piston is raised to the top of the barrel. To confine the rack in the true line of its motion, it is made perfectly straight, and of the same dimensions in its whole length, and its case the same, so as to fit each other most exactly, that it may be kept in its due position, when the greatest part of it is drawn out of the case; for which purpose also it is made (as likewise the case) so much longer at either end than the part necessary to be toothed, as to permit a great part of it to remain in the case, when the piston rod is drawn out to its utmost extent: accordingly in this pump it acts, in this respect, as well as could be wished\*. A notch is cut out of the case at I, to allow the teeth of the wheel to take into those of the rack; and to keep the case firmly in its place, little notches are cut in the upper edge of it, into which the contiguous parts of the pillars are let, and it is secured so by wedges, 2 2, underneath. It will be known that the case of the rack has its due position, when the arm G being taken off, both the rack and the piston rod pulled out to their limit, are found to be parallel. The pillars are made as short as possible; to favour which the frame M has a furrow cut in the middle of it, under the wheel I, to let the teeth of the wheel descend almost to the table on which the machine rests; and the pump barrel is placed as near as may be to the rack.

On the upper part of the box, containing the collar of leathers, is a projecting part of the metal, in the shape of a cube, forming a little pedestal Q: on this is placed the valve f; from beneath which descends a straight duct into the barrel, without penetrating the box: the form of the parts of this valve (all made of brass, and of the size of the originals belonging to the pump) is represented in fig. 4. in a vertical section. A A is a square plate (to be taken off if it should require to be ground and polished anew in contact with the valve), which is fastened on the pedestal with cement; it has a hole in the middle, being the opening of the duct, in which is inserted the little elevated pipe c, to be occasionally taken out; whose use is to prevent the oil applied to the valve from being blown down into the duct by the air rushing into the barrel: the elevated part of the plate a, a, is circular, and has its upper surface made plane and polished, on which rests the valve D; which is so far hollowed within, that only its lower edge, being about  $\frac{1}{8}$ th of an inch thick (which is also well polished), may be in contact with the plate under it, and also that its cavity may rise above the little pipe: the valve

\* By this contrivance of fixing the barrel of the pump horizontal, and its rack underneath the barrel, it is made so portable, that I have packed it (the gage-glass and receiver being taken off) in a box two feet long, eighteen inches wide, and seven in depth; and it should be remembered that the most operative parts of it here described, are the frame and machinery necessary to render a pump, with *so long a cylinder*, portable; a great part of which machinery, if it were not portable, would not be spared, but merely exchanged for the huge frame of those so constructed.—L.

is almost cylindrical, tapering but a little from the base upward, and being laid on the plate, with a small drop of oil interposed, the hollow cylindrical cap E, very little wider internally than the valve, is put over it, being fastened down on the projecting circular part of the plate *a a*. To let the air pass through this cap, three little holes, *e, e*, are made in its sides, the centres of which are just as high as the lower edge of the valve D, when it is raised to touch the top of the cap; the holes being higher than the plate under the valve, that the oil may not run out by them. When this valve is found to admit air into the barrel, it is occasioned either by the oil's being blown away, or some particle of dust, &c. getting between it and the plate (which would produce the same effect in any valve), and is remedied by taking it off, wiping it clean, and applying another drop of oil: the greater the quantity of air is, which passes by the valve, the more is the oil dissipated; and, consequently, this chiefly happens in the beginning of the exhaustion, when much air is drawn from the receiver, and thrown out of the barrel at each stroke; so that whenever great rarefaction is requisite, I often found it convenient, towards the end of the operation, to apply a fresh drop of oil to the valve, keeping the piston in the mean time elevated to the top of the barrel, whilst the valve is taken off, until it be replaced; which is done in an instant: thus it will perform perfectly well if quite clean and free from dust; but as the smallest particle of dirt, or mucus (which is apt to be produced from the oil's corroding the brass, and becoming clammy), will render the valve not air-tight, I am, therefore, persuaded, that no valve will so perfectly or certainly exclude air as a stop-cock; and this I take to be the chief reason why Mr. Smeaton's pump, as improved by Messrs Haas's and Hurter's contrivance, for raising the valve at the bottom of the barrel, does not, in practice, answer as well as any other: for if some air did not return into the receiver by that valve in pumps on his construction, it would be hard to tell why it should not perform as well as a pump of any other construction.

To the bottom of the barrel is fastened by four screws, passed through its prominent rim; the circular plate R, forming in part the shell of the cock S, and which has the high ridge T on the back of it; all these being only one piece of metal, which is represented separately in fig. 5. and was cast in that form: the round plate is about  $\frac{1}{4}$ th of an inch thick; but the cavity of the cock intrudes so far on this, that when the key of the cock is put in, the thickness of the plate in the part directly over the key, where the ducts open into the barrel, is not more than  $\frac{1}{8}$ th of an inch: the length of the shell of the cock is three inches; that of the key  $\frac{1}{8}$ th of an inch less: the diameter of the key, at the thicker extremity, is  $1\frac{1}{10}$ ths of an inch, and at the smaller end  $1\frac{1}{4}$ th of an inch. It is turned by the handle *u*, fastened on its axis as near to the end of the shell as may be; viz. at the distance of  $\frac{1}{4}$ th of an inch. Care was taken that the metal of the cock should be free from pores, by which, if air were admitted in the working, it would be very hard to discover it, as this might take place only in *certain positions* of the key; and the ointment might sometimes prevent and sometimes suffer it, so that it might elude trials; the cock was also so truly and smoothly ground, as to produce an intimate contact of the key and shell throughout (for air will penetrate where liquids would not); it was also made of a true conical shape, its sides being perfectly straight from end to end. The cock  
must

must be lubricated, and made air-tight by ointment \*; and as all oil or greasy ointment has an acid in it (of which, perhaps, it can never be divested without destroying its lubricity) which corrodes the brass, and stiffens the oil or ointment itself, by impregnating it with green rust or verdegris, I found it necessary, after the cock was finished, to have the key, and the inside of the shell coated with tin.

The key of the cock terminates in a little knob *x*, fig. 2. against which the end of the thin spring lever *y* presses to keep the key constantly firm in its shell: this spring is made to urge onward the key, with any degree of force requisite, by the screw at *y*, screwed through the outer part of the little arch, or frame, which frame is itself fastened by screws on the prominent ring of the barrel, as appears at *z*, fig. 2. The lever has a little ridge, or elevated part, which rests in a notch in the frame; by which the lever is fixed in its proper place, the notch being the fulcrum: by withdrawing the screw the ridge may be raised out of the notch, and the lever pulled back from the knob, to let the key be taken out when it requires more ointment; which will be known when it has worked into the shell nearly to its limit; and this should be carefully learned, by marking how far it can be inserted into the shell without any ointment interposed. The form of the key, and its lever, is exhibited in fig. 6. in an horizontal section through the middle of them.

Through the shell of this cock are two round perforations for two ducts, through the cock, each one inch distant from the other, and the same from the end of the cock on either side: they are in a plane, or section, passing through the axis of the key, and also through the axis of the barrel; and both pass through the ridge of metal *T* at the back of the shell. The duct next the smaller end of the key (being a part of the duct communicating with the receiver) is in diameter  $\frac{1}{4}$ th, and the other duct is only  $\frac{1}{8}$ th of an inch: through the key, also, are ducts answering to the perforations in the shell; but the ducts in the key are not both in the same section of it, but one of them is at right angles to the other; so that when one of the ducts through the cock is open, the other will be always shut, and *vice versa*.

There is a continuation of each of those ducts formed in the piece of metal *V*, which is joined (by cement interposed) to the ridge of the shell of the cock by the four screws *g*, fig. 1. by two openings (into the two ducts in the piece of metal) opposite to those in the ridge; and these ducts in the piece *V* have two other orifices at *a*, and *c*, where the two pipes *a b*, fig. 1. and *c d*, fig. 2. communicate with the two canals in the piece of metal, being connected with them only by the help of cement, that the barrel with its cock may be occasionally disjoined from them: thus a separate communication is formed between each of the pipes, and the barrel.

The pipe *a b* is only  $\frac{1}{16}$ th of an inch in diameter, except at its orifices, which are widened;

\* This I have made of one part of common rosin, one part oil, and one part and a half fresh suet: the oil and rosin is first melted, and when a little cooled the suet is added, that the heat requisite to melt the rosin may not burn the fibrous part of the suet, and destroy its firmness: more oil is to be added in cold, than in warm weather. The quality of the ointment for the cock is of the utmost consequence; for if ever the parts of the metal of the cock came into immediate contact, it will not be air-fast; oil will not render it so (nor consequently would a valve); but the ointment must be stiff and adhesive; yet not too stiff, as then it would be hard to turn the key, and it could not be distinguished whether its stiffness was owing to the ointment, or to the friction of the parts of the metal getting into immediate contact, which would be pernicious.—L.

one of them is connected at *b*, with a duct formed in the piece of metal *k*, attached to the box of leathers *C* at the top of the barrel, which duct turning in an angle, without penetrating the box, opens into the barrel: this pipe and duct are joined by a simple application of their orifices, which are made in the contiguous sides of both, and the joint made airtight, by cement melted around with a blow-pipe; and the same might be effected by the pressure of a screw closing the surfaces with oiled leather interposed. By this pipe when the valve is shut, and the key of the cock so placed that the communication of this pipe with the bottom of the barrel is open, if the piston be depressed from the top, all the air which is *under* it will be forced to pass by this pipe into the barrel *above* the piston; and *vice versa* if the piston be elevated from the bottom; so that according to the motion of the piston, the air under, or over it, within the barrel, is made to change its place, and to circulate from the space under or above the piston to the contrary; from which effect of this pipe, I call it the *circulating pipe*.

The other pipe, or duct, is represented at *c d*, fig. 2. which exhibits a back view of the pump: its extremity *c* enters the block of metal *V* to communicate with the duct therein, and so with that through the cock into the barrel: its end *c* has a wing foldered to it, by which it is firmly screwed to the piece *V*; this being necessary lest the cement by which alone the joint there is made airtight, should be cracked by any shock moving the barrel; or it might, perhaps, be secured by the screw with only leather interposed: this duct is a very wide one, that the air may the sooner pass through it; it has a turn at right angles at *d*, and another at *e*, from whence it extends under the pillar *B*, and pump-plate *C D*, through the middle of which it rises at *A*; it has a branch turning at right angles from *A* under the pump-plate, the end of which terminates in a round orifice with an inside screw, that by means of a pipe furnished with a stop-cock; or otherwise another pump-plate and receiver, or several of them, may be connected with it, being laid on the same table which supports the pump: when such are not used, the orifice is closed with a stop-screw as at *D*. From this pipe rises a branch *E*, inserted into the top of the brass cap *F* of the glass vessel *Y*, and opening into the vessel by a duct made in the thick plate of the cap. *G* is a reduced barometer tube, to be filled in the most perfect manner with mercury, and immersed into a pool of mercury in the bottom of the glass vessel. The interval between the tube and the neck of the vessel is closed by cement, which, as the neck must be a good deal wider than the tube, is effected by a deep ring (*I*) fitted to the tube, having a round plate at the bottom of it; which being let down on the tube after it has been immersed in the cistern, is joined with cement to the top of the cap. The tube was easily immersed in the mercury within the vessel, by covering its open end, and sustaining the mercury in it by a little plate, or scale of iron (*P*), fig. 7. having two threads tied to it, through two holes made near its edges; which threads were grasped together with the top of the tube between the fingers, and the tube was inverted, and let down into the mercury in the vessel: the little plate was then pulled up by one of the threads, it being made so small as to pass through the neck of the glass while the tube was within it, and remained immersed in the mercury\*: the

\* I thought this the best way of fixing the reduced barometer, for if there were a separate vessel as a cistern for the mercury, and a receiver placed over it and the tube, the eye when on a level with the surface of the stagnant mercury, could not see the altitude of that in the tube with precision through two glasses.—L:  
vessel

vessel was then closed at the top with melted cement; and as it communicates by the pipe E with the conduit-pipe, it is exhausted with the receiver, and the mercury in the tube will sink as the rarefaction advances: if a perfect vacuum could be made in the receiver, and gage-vessel, it would sink more or less *beneath* the level of the surrounding mercury, as the tube is narrower or wider, even so as to disappear if the tube were very narrow; so that it must not be less than  $\frac{1}{4}$ ths of an inch in diameter; but it would be better if it were  $\frac{1}{8}$ ths. If it were observed, how much the mercury in a tube open at both ends would sink beneath the level of that in a cistern in the open air, it might be pretty nearly estimated how great is the rarefaction in this gage-vessel, and consequently in the receiver\*.

In order that each of the ducts here described should alternately communicate to the barrel, it is necessary, that the cock should be moveable through a quarter of a turn, and confined to that motion. This is effected, by a pin, *n*, fixed perpendicularly in the back of the handle, fig. 1. which, when the handle is fixed in its place, describes a quadrantal arc, under the shell of the cock, where there is just room to allow such a motion.

The action of this pump may be readily apprehended from the construction of its parts. When the piston is at the bottom of the barrel, and the key of the cock turned to its limit which opens the communication, between the barrel and the receiver and at the same time shuts that with the circulating pipe; the piston being then drawn to the top of the barrel discharges the air through the valve, while other air from the receiver rushes into the barrel, and follows the piston. As soon as the piston has arrived at the upper, or more remote end of the barrel, the key of the cock is again turned, and shuts off the connection with the receiver, and opens that with the circulating pipe. The piston being then depressed, drives the air through the pipe, into the upper, or remote end of the barrel. When the piston has arrived at its limit, the stroke is ended, the key of the cock is to be again turned to open the receiver, and a second stroke may be made, with the same consequences as the first; and in this manner the process may be continued at pleasure. The limit of exhaustion will be when the air in the receiver shall have become as rare as the air in the barrel would be when the piston is up; supposing no communication to have taken place between it and the receiver.

When the pump is required to condense, the air is made to move in a reverse direction. The cock of the gage-vessel is stopped, that it may not be burst; the valve is taken off; the communication of the bottom of the barrel, to the circulating pipe, opened by the key of the cock, and the piston raised to the top. The external air passes into the lower part of the barrel; and by turning the cock, so as to open the receiver, and shut the circulating pipe, and then depressing the piston, the air is forced into the receiver, which ought of course to be of sufficient strength, and properly secured. By shutting the receiver, raising the piston, opening the receiver again, and returning the piston to its former situation, another stroke is made, and these alternations may be carried on to any desired or practicable extent.

\* The author prefers the short barometer-gage to the syphon-gage, because in this last he thinks the motion is impeded by the tube, and the mercury becomes foul on that side which communicates with the receiver.—N.

The remainder of Mr. Little's paper, consists of numerous observations and remarks, with some experiments; a considerable number of the former of which are familiar to such as have attended to this branch of natural philosophy: but the whole, though evidently short of what the author must have originally intended, will be read with interest and satisfaction, by those who may consult the memoir itself.

## X.

*Observations on Chemistry and Natural History.* By PROFESSOR VANDELLI\*.

1. *Fossil Prussian Blue from Minas Geraes in the Brazils.*

IN digging a mine at S. Ioao del Rey, to the depth of more than forty palms (about 27 feet English), large brittle bones, and some teeth of a cetaceous animal were found in the clay, which occupied a space of more than fifty palms square (about 35 feet English). These bones, and the clay with which they were intermixed, were white when first extracted, but soon became blue when exposed to the air †.

These bones and clay, in the blue state, being exposed to the action of a moderate fire, became green, emitted a weak flame, and afterwards lost their colour. The reddish earth which remained was, for the most part, attracted by the magnet.

The Prussian blue was soluble in the marine acid, and in this state lost its colour. By addition of a fixed alkali, the precipitate was afforded of a green colour, which afterwards became blue, of more intensity than at first.

The mixture of bones and clay, being fused with a proper flux for iron, afforded three parts of their weight of iron, which was intirely attracted by the magnet.

Respecting the fossil Prussian blue, as the reader may consult the works of Wallerius, Bergman and Kirwan, I shall not enter into any further detail.

2. *Native Copper of Brazil.* This mass of copper was found in a valley two leagues from Cachoeira, and fourteen from Bahia. It weighs nearly 2616 arratels (or nearly pounds English), and its figure is rhomboidal, the upper surface being irregular; on account of some cavities and protuberances. Its greatest height is three feet two inches, its width, at the base, two feet and a half, and its greatest thickness ten inches, Paris measure ‡.

The external colour of the mass is deep reddish, with spots and particles of a greenish blue, produced by the decomposition of the copper. On the lower surface appear some yellow spots of ochre of iron.

At various parts of the surface, particularly the lower surface, several pieces, large and small, are observed, which, at first, seem to be *ferrum micaceum*; but when examined by fire they prove to be indurated oxide of copper. For an ounce of this substance afforded thirteen parts out of sixteen pure copper.

\* From the Transactions of the Royal Academy of Sciences at Lisbon, I. 259.

† For an explanation of this phenomenon, see Proust in this Journal, I. 455.

‡ The foot-royal of Paris is to that of London as 1,0000 to 0,9383.—N.

This copper being assayed, afforded no indications of gold or silver.

Another piece of virgin copper was found at the same place, but much smaller than the foregoing. A mass of copper, of the magnitude here described, has very rarely been met with, as Mr. Monnet remarks\*.

XI.

*On the Manufacture of Hats, and other Objects. By a Correspondent.*

TO MR. NICHOLSON.

SIR,

Newcastle, 9th January, 1799.

THE obliging manner in which you have noticed my enquiries, respecting the hatting business, induce me to send you others on another business, equally productive of mischief, to the morals and the health of a class of industrious, but depraved, fellow men—I mean the journeymen flax-dressers; those and the journeymen hat-makers, are almost proverbially vicious, and I confess I look with sanguine expectations of reform, to the period that will exhibit machinery for each.

Is it possible to construct a machine, for the purpose of dressing flax? There are machines, I understand, in Scotland, invented about 40 years ago (vide English Encyclopedia, article Flax Dressing), for the breaking and scutching of that article: might these not be extended to the further process of the heckles? Perhaps the claim is equally upon humanity as ingenuity, for I have been told (and I have, from my own knowledge in that business, scarce any doubt of its truth), that the journeymen are obliged to give it over, about the age of 40, from an approach of *consumption*; and one, whom I am told was lately opened at his death, *had his lungs covered with a thick crust, composed of the dirt received upon them from his business*.

Accept the following information, as the best I can yet afford you, on the subject of my queries in your last (p. 467); they have been obtained since my writing to you (on the subject. Hats were invented at Paris, A.D. 1404, by what circumstance I am yet ignorant. First made in London, 1510. The above, though an anonymous communication to me, I believe will not be found far from the truth. There is a house in Derbyshire, name Cooper Bibby and Downal, at Lea Wood, near Cromford, in that county, who, I understand, use machines in the making of hats, but how far in the process I am not yet able to discover: perhaps some of your correspondents may have informed you, in consequence of my queries. The common account amongst the journeymen is, that *the property of wool to felt* was first discovered by a shepherd, who had wool in his shoes; his name was Clement, and they keep the 25th of November, as a day *sacred to dissipation and an old shepherd saint*. Saint Catherine, I understand, is the patroness of the journeymen flax-dressers. It is probable, *they* will celebrate the 25th of November.

\* Nouveau Système de Mineralogie, page 314, Mines de Cuivre. The Cabinet of Mines at Freyberg possesses a specimen of this kind, weighing ten pounds, which is the finest and largest specimen of native copper hitherto known.



These suggestions may, I hope, afford a thread to others, more conversant in the theory of Mahuzzim, or saints protectors, than I am, to favour you with information of more consequence on the subjects of the manufactories in question. The editors of the English Encyclopedia say, that, on the subject of dressing flax, they refer their readers to some observations in the Gentleman's Magazine, for June, 1787: I have not that book at hand, but I hope your readers may find in it some ground-work for future machinery.

I am, sir,

Yours, &c.

N. L.

In the liquor for boiling of wool hats, after bowing and basoning, one part of human urine is made use of, and two parts clean soft water: Could not something be substituted in place of the urine, which might be *equally useful in hardening the hood*, and exclude so dirty an ingredient\*?

## XII.

*Extract of a Letter from Citizen Ramond, Associate of the National Institute of France, and Professor of Natural History at Tarbes, to Citizen Haüy, Member of the Institute at Paris, respecting two excursions to Mount Perdu, the most elevated summit of the Pyrenean Mountains\*.*

Bareges, 5 complem. day, in the year V.

**I** FLATTER myself, Citizen, that you will not hear, without interest, such events as have proved most remarkable in the results of my travels of the present year. I hasten to communicate them, with the hope that the portion of your time which I shall engage will be well repaid by the geological fact which is the object of this letter.

Mount Perdu is the most elevated mountain in the chain of the Pyreneans. In my former travels, I proceeded along the bases. Reboul, who has succeeded in determining its height by observations made from various elevated points, had likewise approached it in a different direction. It is certain that the whole of the surrounding group is calcareous, and the aspect, which can scarcely deceive those who are habituated to behold and contemplate mountains, had determined my belief that the entire pic was of the same nature.

Abundance of calcareous matter forms one of the distinct characters of the Pyrenean mountains; but to behold this genus in possession of the very crest of the chain, the place which the granite occupies in every other known chain of mountains, was a phenomenon too singular not to inspire me with the strongest desire to ascertain its existence.

The enterprize was not without its difficulties; and among them, that which was the least

\* The preparations for my change of residence, as mentioned on the wrapper, have induced me to defer my own observations and report, concerning hating, till next month.—N.

† Read to the French National Institute at the sitting of the 21st Vendemiaire, in the 6th year of the Republic (12th October, 1797), and inserted in the *Journal de Mines*, No. 37, of the same year.

foreseen,



foreseen, was the absolute ignorance in which I found all the country people, with regard to the real position of Mount Perdu. It is not visible, but from the elevated situations, and disappears as soon as you descend. It was necessary, therefore, to seek the road, as chance might direct, through the most horrid deserts, which are never frequented, either by the hunter, or by shepherds.

On this occasion, I had the good fortune to enjoy the company of our colleague, La Peyrouse; and I used every effort in my power to secure his company in those regions, in which I foresaw so many interesting observations would present themselves. But his strength did not permit him to accompany me longer than the first day, and part of the second. I left him at the foot of the first glaciere, taking with me his son, and one of his pupils, together with four of my own. I will not fatigue you with the detail of our own sufferings, nor alarm you with an account of our dangers; but shall simply observe, that after having imprinted our steps, during three hours, in the hardened snows, which the inclination of their plane rendered very dangerous, we arrived at a summit opposite Mount Perdu, which raised us to its middle part. I never beheld, even in the Alps, any object of greater magnificence: I do not except the approaches to Mount Blanc. The calcareous mountains possess a simplicity of form and majesty which is peculiar to them: now Mount Perdu is calcareous, absolutely calcareous, and of secondary composition. I descended from my situation towards a vast lake, still frozen, which reposes at its feet. I crossed this lake; I visited every spot where the external snows, and horrid glaciers, have suffered the naked rocks to penetrate. Every where I found grit, breccia, and compact calcareous stone, covered with the prints and remains of marine bodies: marine remains at the summit of the Pyrenees, and on the pic which predominates over all these mountains!—This phenomenon acquires a character still more wonderful, when we consider that the entire chain exhibits none of those attestations of the former residence of the ocean; and that it is in vain we seek them in our secondary calcareous stones, our bituminous stones, or slates, all which are deprived of the prints of shells and vegetables. I passed from the flanks of Mount Perdu into the valley of Pinede, where it pours its waters; I came to the port or passage of Pinede, one of the most elevated of the Pyrenees. The same phenomena every where presented themselves; every where I saw grit, breccia, and common calcareous stones, abounding with marine bodies; and by casting my eyes along the extensive valley of Pinede, I saw, in all parts, the continuation of the banks in which I had observed these remains.

La Peyrouse had seen the opposite side of the mountains which I visited: he coasted along them to meet me at the port of Pinede. He observed part of these wonders, and partook of my harvest. Stormy clouds having covered the pic, at the time of my arrival at my first excursion, it became necessary to revisit it at a more favourable season; I, therefore, resumed the route of Mount Perdu, fifteen days ago, with a couple of friends only, and some hardy mountaineers. All the glaciers were then uncovered, by the dissolution of the snow which had laid upon them; the lake was unfrozen, and the dangers of approach were considerably increased. It was necessary to secure our footsteps in the hardest ice by heavy strokes of the hatchet. We suffered much in this excursion, and I could not reach the summit; but the sky was admirably serene, and I touched, as it were, those rocks which I could not

ascend. At this time, I beheld the whole structure of the mountains, the direction and inclination of the strata, the entire succession of its layers; every fact was manifested to my sight, and I completed my collection of marine bodies, which are contained in these mountains.

To the west, as well as to the east, every part is secondary, and full of shells. I sent one of my pupils towards Vignamale, who brought me a cornu ammonis. It is, as I have ventured to affirm in my printed observations, an enormous secondary mass, superposed on the edifice of the chain, and which covers the southern part to the thickness of ten or twelve kilometres, and is in length, forty.

The most perfect among the remains of marine bodies which I found in these regions is a perfect ammonite, the exact impression of a pectinite, the prints of asterites, many oysters in the solid, caryophyllites, and a multitude of madrepores. I shall not enter into more circumstantial details. The geologic and geographic results; the nature and inclination of the different banks; their connection with the rocks which support them; the state of vegetation; the insects observed in these elevated regions;—will form materials for a memoir, which I intend to draw up, when in a less interrupted state, for the purpose of transmitting it to the National Institute. But I am desirous that the principal facts should be communicated to that body, with all the interest of novelty, and I think them sufficiently interesting to request your mediation in this respect.

If you think it proper likewise to transmit them to the Council of Mines, I request that you will permit me to give you the trouble of performing this office.

### XIII.

*An Abstract of a Memoir upon the Fossil Bones of Animals. By Citizen CUVIER.\**

THE intention of the Author, in this memoir, was to collect as much as it was in his power all the fossil bones appertaining to each species of animal, whether of such as he himself had seen, or those of which he merely had a description in authors, to form or recompose the skeletons of these species, and to compare them with those which now exist on the surface of the globe, in order to determine their relation and differences. The following is a series of the species to which his attention has been directed.

1. The animal which afforded the bones and teeth, called the bones and horns of the mammoth, by the Russians, and inhabitants of Siberia. Similar fossile remains are also found in Europe. It is a species of elephant, resembling the elephant of Asia; but from which it differs in the alveolæ of its teeth, and its tusks being longer, the angle of its lower jaw

\* Communicated to the Société d'Histoire Naturelle at Paris. This abridgment is translated from the bulletin of the Société Philomatique, No. 18, year VI.

being more obtuse, and the laminæ of which its grinders are composed being thinner. The true analogous living animal is not known, though it has been hitherto considered as the ordinary elephant.

2. The animal, of which the remains are found on the banks of the Ohio in North America, which the Americans and English have also named mammoth, though it differs much from the former. Remains of this animal are also found in Europe and in Asia. It must have been nearly the height of the elephant, but more bulky; its tusks are smaller; its grinders are armed with large cutting points, of which the section by wear presents double transversal lozenges. There are three molar teeth on each side, one of four, one having six, and one eight, points.

3. The animal of which the teeth tinged by copper afford the turquois stone, and of which there was a mine at Simore, in Languedoc. The remains of this same species is found in the department of Ain, in Peru, and elsewhere. It must have considerably resembled the former, but the points of its molar teeth are round, and when worn, their section presents, first, a circle, then a semi-oval, and afterwards, a figure of a trefoil, which has caused them to be confounded with the teeth of the rhinoceros: some of these teeth have twelve points, others six, others four.

4. The rhinoceros. The feet and fragments of the jaws of this animal are found in France, and elsewhere, in which the author has hitherto observed nothing which differs from the common rhinoceros; but, as he has not yet seen an entire bone, he cannot positively affirm that they are identical.

5. The species of rhinoceros, with an oblong craneum, which is found in Siberia, Germany, and other countries. The author has seen teeth, and parts of the jaw-bones, found in France, which appeared to him likewise to belong to this animal; the principal character of this species consists in the long closure of the nose: the living analogous animal is unknown.

6. A molar tooth with two transversal eminences, which is in the possession of Citizen Gillet; and of which the National Museum possesses a young tooth that resembles neither the teeth nor the germs of any animal yet known, whether living or fossil: the only tooth which this slightly resembles is the last molar tooth of the rhinoceros. This tooth, therefore, indicates the existence of a sixth fossil species, of which the living analogous animal is unknown.

7. The animal, twelve feet in length, and six in height, of which the skeleton was found under ground at Paraguay, and is preserved in the royal cabinet at Spain, at Madrid. The author proves by a detailed comparison of the bones, with those of all the known quadrupeds, that it is a proper and distinct species, more nearly approaching the sloth than any other genus, and that it may be called the giant sloth. Citizen Cuvier, in this place, communicates the interesting discovery he has made, that the sloth (*bradypus tridactylus*, Lin.) has naturally and constantly nine cervical vertebræ. It is the first known exception, established by Citizen Dauberton, that all quadrupeds have neither more nor less than seven cervical vertebræ.

8. The animal, of which the remains are found in the caverns near Gaylepreuth and Muggendorf, in the margravate of Bayreuth, in Franconia. Various authors have considered

dered it as a white bear; but it differs from this animal, as well as from all the known bears, in the form of its head, which is particularly characterized by the projection of the front, by the absence of the small tooth, which all the known bears have behind each canine tooth, by the osseous channel of the humerus, in which the brachial artery passes; and by several other circumstances in the figure and proportion of the bones. This animal, however, resembles the bear more particularly than any other kind.

9. The carnivorous animal of which the bones are found in the plaister-stone of Montemartre: the form of its jaws, the number of its molar teeth, and the points with which they are armed, indicate that this species is referrible to the genus *canis*; but it does not completely resemble any species of this genus. The most striking distinctive mark is that the seventh molar tooth is the greatest in the animal of Montemartre, whereas the fifth is the largest in dogs, wolves, foxes, &c.

10. The animal of which the lower jaw was found near Verona, has been considered by Joseph Monti as a portion of the cranium of the sea-cow; a notion which all the geologists have adopted, though it be the contrary to the most simple notions of comparative anatomy. This jaw, according to Cuvier, has belonged to an animal resembling, though specifically different from, the mammoth, the animal of the Ohio, and that of Simore. Its most particular character consists in the curve which forms its symphysis.

11. The animal of the stag kind of which the bones and the antlers are found in Ireland, in England, at Maestricht, &c. It is sufficiently different from all the stags, and even the elk, to which it has been referred, by the enormous magnitude of its antlers, the flattening of their superior part, and the branches which spring from their base. Several figures of these are given in the Philosophical Transactions.

12. The genus of the ox or beeve alone affords several fossil species: the craniums of two were found in Siberia, which have been described by Pallas, who referred one of them to the ordinary buffalo; but he has since attributed them to a peculiar species, natives of Thibet, named *arni*. Citizen Cuvier proves, by osteologic comparison, that those craniums have not belonged to the buffalo. The other appeared to Pallas to have belonged to the buffalo of the Cape, or the musk ox of Canada. Citizen Cuvier shews that they cannot have belonged to the former, but not being in possession of the cranium of the *arni*, nor the musk ox, he makes no decision respecting their identity with the fossil craniums.

The author likewise describes two kinds of craniums which have been found in the turf pits of the department of La Somme, which greatly resemble our common ox, and that of L'Aurouchs, but are more than one fourth longer.

From this enquiry, the Citizen Cuvier concludes, 1. That it is not true to affirm that the animals of the south have formerly lived in the north, their species not being perfectly identical. 2. That in every country there have lived animals which do not at present exist, either on the same spot, or elsewhere in any known country. Hence he leaves to geologists, the task of making, in their systems, such changes or additions as they may think best suited to explain the facts which he has thus established.

## XIV.

*Extract of a Memoir of Proust, entitled Enquiries concerning Tin. By CITIZEN DARCEY.\**

**T**HE author of this memoir observes, in the first place, that he considers his work as a supplement to that of Pelletier, on the different degrees of oxygenation which tin can acquire, when dissolved in acids.

When tin is dissolved without heat, in very weak nitric acid, it acquires a degree of oxygenation, different from that which it would obtain if dissolved in a concentrated acid, or if heat had been employed. In the first case, the nitric acid is not decomposed, and affords but little oxygen; but this is not the case with the water, which in fact, affords the quantity necessary for its oxygenation.

This solution, which is of a yellow colour, gradually precipitates, more especially if it be heated, and the separation of the tin is made without re-action, for there is no disengagement of nitrous gas; and if it be saturated with caustic pot-ash, there is a disengagement of ammoniac, as was already observed by Bayen and by Pelletier. The oxyde of tin, as well that portion which remains suspended in the fluid, as that which is precipitated, and re-dissolved in muriatic acid, equally decompose the superoxygenated muriate of mercury, and the mercurial oxydes, &c. being very different, in this respect, from the oxyde of tin, which is precipitated from the solution, in concentrated nitric acid, or the solution which has been made with heat. In this, the nitric acid is decomposed, and the precipitate is insoluble in the muriatic acid, and no longer acts upon corrosive sublimate, or the oxydes of mercury. This difference arises from the circumstances, that in the first case, the oxyde of tin, whether it remain in the liquor, or fall down, is oxyded to the minimum, as Proust affirms, and in the second case, to the maximum. Hence it is, that in the first case, there is no disengagement of elastic fluid, whereas in the second, the disengagement is considerable.

When an acid of the strength of 25 or 30 degrees is used (sp. gr. 1,23), the solution is so violent, that there does not remain an atom of tin in the fluid, but the precipitate is oxyded to 40 parts of oxygen; but when the very weak nitric acid is used, or the solution is made without heat, the metal is oxyded only to 30.

It is ascertained by hepatic water, that all the tin is separated from the acid; for this re-agent, will totally precipitate tin, copper, or lead, if the fluid contain any. Proust has observed, that this re-agent may prove eminently useful. For example, suppose a solution to contain lead, copper, zinc and iron, the hepatic liquor will separate them all in succession; first the copper, then the lead, next the zinc, and last of all the iron, with this remarkable event, that the iron which was oxyded to the maximum in the solution, is thrown down at the minimum, and is precipitated green, by ammoniac and by lime. This happens, because the hepatic water seizes a portion of the oxygen from the oxyde, and becomes decomposed, leaving the iron at the less degree of oxygenation.

It is known, that very fetid hydrogen is extricated, during the solution of tin in the muri-

\* Annales de Chimie, XXVIII. 213.

atic acid, particularly when the tin contains arsenic, which may be very well ascertained, by burning the gas under a glass vessel, when the arsenic is deposited on the sides. Proust also observes, that this gas is very hurtful to the brightness of the purple of powder of cassius. He therefore very properly advises, that the solution should be heated before it is used: an observation of the greatest importance for porcelain and enamel painters.

If a few drops of the muriate of tin, be added to the muriatic acid of commerce, the yellow colour of the latter instantly disappears. The tin deprives the iron of the excess of oxygen, which rendered it red, and ammoniac then precipitates it of a green colour. The same thing happens with the solutions of the sulphates, nitrates, and red muriates of iron.

But, if a stronger dose of muriate of tin be poured into the muriatic acid of commerce, a grey powder falls down, which Proust has ascertained to be mercury.

When a solution of the muriate of tin is distilled, the whole of the liquid comes over, with a portion of the muriate. Towards the end, the matter swells up, and afterwards settles in the state of calm fusion, of a green colour; and if the fire be raised, puffs of fuming vapour are driven off, but nothing comes over in the fluid form. If the muriate of tin contains a small portion of sulphuric acid, that acid is decomposed, and the sulphur partly unites with the tin, forming a sulphuret, in which the tin is oxyded to the minimum. This muriate requires a stronger heat for its distillation, than the smoking muriate in the ordinary process.

But that substance which rises, and is condensed in the neck of the retorts, proves, after washing, and separating the fuming muriate, to be nothing but tin half oxyded, to which acid only need be added, in order to dissolve it again. This distilled muriate effectually decomposes the sublimate of mercury, and all the mercurial preparations, or oxydes; while the fuming muriate, being oxyded to the maximum, decomposes nothing.

The muriate of tin affords, with alkalis, a precipitate, which caustic pot-ash abundantly dissolves. This solution reserved, in a close vessel, at the end of 12 or 15 days, affords a metallic group, in the form of cauliflowers, which consists of tin, nearly pure. In this process, one part of the tin, oxyded to the minimum, robs the other, and becomes saturated; and this muriate of tin, so saturated, is no longer capable of altering corrosive sublimate.

The same thing happens, if a mixture of the carbonate of copper, and the oxyde of tin, separated from the muriate by pot-ash, be kept under water. The oxyde of tin robs all the copper of its oxygen, and the portion of carbonic acid; whence the copper is found reduced, in crystalized plates, among the oxyde of tin.

Lastly, the muriate of tin difoxygenates indigo, and changes it to green. This experiment is analogous to other difoxygenations of the same fecula, known to dyers. Proust mentions this circumstance as an object deserving of great attention, upon which he means to make further researches.

The sulphates, nitrates, muriates, acetates, and carbonates, of copper, and also, the red, blue, or green oxydes, mixed with the muriate of tin, are generally converted into a white oxyde, which is collected at the bottom of the vessel; and if this muriate be passed again upon a new oxyde, it becomes itself green, and is saturated to such a degree as no longer to alter the colour of oxydes. The powder, thus robbed by the muriate of tin, assumes different shades of colours, violet, blue, black, &c. which are more particularly governed by the degree

degree of exposure to light. It is fusible by heat, and assumes the appearance of a muriate of silver, in which state Proust proposes to keep it.

The sulphuric acid has no action on this substance, but the muriatic acid immediately dissolves it, and it crystallizes in tetrahedrons. The nitric acid also dissolves it, during which it passes through various shades of colour; but at last it retains the appearance of a solution of nitrate of copper, in which the presence of muriatic acid may be easily detected. But the disengagement of nitrous gas which takes place during the solution, proves that it is not saturated with oxygen.

Proust, observing that this muriate was without colour, and that it was also soluble in ammoniac without colouring it, thought at first that he might conclude, contrary to the principle established by Lavoisier, that the copper was absolutely clear of oxygen; but the analysis which he made soon undeceived him.

He dissolved 100 grains of this white fused muriate in very pure nitric acid, which he afterwards precipitated by the nitrate of silver, and obtained 142 grains of muriate of silver, which represent  $24\frac{1}{4}$  grains of marine acid. On the other hand, 100 grains of very white muriate of silver treated with nitric acid, and carbonate of pot-ash, afforded him 113 grains of carbonate of copper, which, according to experiment, answer to  $62\frac{2}{3}$ , or 63 of copper, so that from his analysis he found these products:—marine acid  $24\frac{1}{4}$ ; oxyde of tin 1; copper 63; and the  $11\frac{1}{2}$  deficiency remain for oxygen.

Whereas he was satisfied, that in the green muriate, in the sulphate, in the nitrate, and in the acitite of copper, the metal which is at the maximum of oxygenation, contains about 26 of oxygen in the centenary.

Lastly, it is proved that while the copper is oxyded no farther than 17 or 18, its solutions are white, and it cannot then give colour either to its muriate, or to ammoniac.

The following, as Proust informs us, is another proof of the presence of oxygen in this white powder. If a small quantity be put into water with iron filings, the copper soon separates, and the iron becomes oxyded to the minimum, so that when dissolved the alcalies precipitate it of a green colour.

If it be demanded whence the oxygen comes in the white solution of copper in the muriatic acid, Proust replies, that it is from the water. The water is decomposed, as Berthollet had before shewn. The oxygen combines, and oxydes the copper. Proust even relates an experiment in which the hydrogen of the water was rendered perceptible to the smell, by the assistance of a small quantity of sulphur which was formed, and was dissolved and volatilized with the hydrogen. This experiment consists in boiling the blue sulphate of copper in muriatic acid. A solution is obtained, which is precipitated by water, during which the hydrogen becomes perceptible to the smell.

When a green muriate is distilled, it may easily be brought to the point of crystallization; but if the distillation be carried further, it is decomposed, and loses the portion of oxygen, which makes the difference between the oxyde containing 25 parts, and that containing only 18; but the distillation then affords oxygenated muriatic gas. At length this gas ceases, and there remains at the bottom of the retort a grey, well-fused mass, which is the white muriate of copper. This is an easy method of procuring the white muriate. Proust adds,

that the copper in the green sand of Peru, and in the native muriate of Chili, are in this state.

It is of importance to remark, that the chemist might be induced to believe, that when a metallic substance is difoxygenated in any manner whatever, whether by the application of strong heat, or by the affinities of other metals for oxygen; or, in the manner of Proust, by hepatic gas, as happens in the difoxygenation of the tungstic and molybdic acids; the chemist might think, that these substances, at their transition to the violet blue, or black colour, were reduced to the metallic state: but the effect is, as he remarks, merely an incomplete difoxygenation. The metal is merely depressed to the minimum, as happens with iron and copper, when by various means they are brought from their entire state of oxygenation, to that in which the colour becomes blue, black, or red, more or less deep. There are no metals but mercury, gold, and arsenic, in which a complete difoxygenation is observed.

But to return to tin: Proust has found, with Bergman, that tin oxyded to the minimum acquired no more than an addition of 30 parts in the hundred, and even in this state is not exempt from marine acid. If it be ignited in a crucible it loses weight, and emits the vapours of muriate of tin; and when the tin is oxyded to the maximum it is charged with 40 per cent. But it is easy to reduce it 30, in which state it is blueish, and insoluble in acids. The author here terminates his memoir, by remarking, that although Pelletier has not mentioned the white muriate of copper, he is not less persuaded that it was known to that chemist; and this more particularly, because he has spoken slightly of the difoxydation of that metal by tin, as if he meant to speak more fully on another occasion. But while he renders this justice to Pelletier, he affirms with the open freedom of truth, that though his results coincide with those of that chemist, they were not undertaken subsequent to his experiments.

There are a great number of other facts and observations in the Memoir of Proust which deserve to be related; but the abundance and density of these facts are such, that the attempt to communicate them would convert this abridgement into a memoir no less ample than the original.

## XV.

*Observations on the Differences which exist between the Acetous and Acetic Acids. By J. A. CHAPTAL.\**

**T**H E acid of four wine presents to our observation two very distinct states, which are known by the names of the acetous and the acetic acids.

Chemists have hitherto referred this difference to the varying proportion between the oxygen and the radical, and it has generally been believed, that the acetic acid differs from the acetous simply in a stronger dose of the acidifying principle†.

Citizen Adet has resumed this interesting research, and has presented to the society a course of experiments, from which he concludes:

\* Annales de Chimie, XXVIII. 113.

† We may except Citizen Perès, who, in the Journal des Pharmaciens, has announced, that the difference between the acetous and acetic acids consists in the proportion of carbone.—C.



1. That no such thing as the acetous acid exists.
2. That the acid of vinegar is always at the highest possible degree of oxygenation; and, consequently, is always acetic.
3. That the difference between the acid obtained by the distillation of vinegar, and that afforded by the like process from the acetate of copper, depends on the less quantity of water contained in the latter.

I must confess, that notwithstanding the high degree of confidence I place in the labours of Citizen Adet, it has been impossible for me to adopt his conclusions in this respect; and as I was very far from calling his experiments in question, I have thought it proper to analyse, compare, and discuss, his results, much less with the view of ascertaining their accuracy, than to determine whether they justify the consequences deduced from them by their author.

I conclude, therefore, immediately with Citizen Adet, that it appears to be demonstrated by his experiments, that the oxygen exists nearly in equal proportions in the distilled acetous acid and the acetic acid; that it is equally difficult, and even impossible, to oxygenate the acetous acid, and to disoxygenate the acetic; and that most of the salt separately formed by these two acids are of the same nature. These consequences, no less new than interesting, belong to Citizen Adet, and naturally flow from his experiments.

But to conclude, from the same facts, that the acetous and the acetic acids are exactly the same, and differ only in their proportions of water, is more than the experiments will justify; and while we must agree with Citizen Adet as to the truth of his first consequences, it may also be shewn, that there is a very great difference between the acetous distilled acid, and the acetic. I shall endeavour to ascertain the cause of this difference.

The two acids in question differ, no doubt, in smell, in taste, and solvent power; but as the cause of these differences might be referred to their respective degrees of concentration, I have thought it proper to bring both to the same specific gravity, by diluting the acetic acid with a sufficient quantity of distilled water; and it was with these two acids, at the same degree, that I made the following observations and experiments:

1. The smell and taste shew the difference between these two acids. The effects, in this respect, are more evident, and the sensation is much more penetrating on the part of the acetic than the acetous acid.

2. The action of the acetic acid on the lips, and upon the metallic oxides, is more speedy and energetic than that of the acetous acid. These two acids, set to digest in equal portions upon the oxide of copper, precipitated from the sulphate by pot-ash, washed, and then distilled, presented very different results. The acetic acid dissolved the oxide, and formed beautiful crystals by cooling; but the acetous was simply coloured of a blueish green, and let fall nothing but a green saline crust, on the borders of the fluid.

Neither of these acids perceptibly attacked copper. They merely acquired, by long digestion, a light green shade of colour.

Eleven parts of the acetic acid required for their saturation 6,98 of pure pot-ash; the same quantity of acetous acid required only 5,73.

There, consequently, exists a difference between these oxides. The following experiments will shew in what this difference consists, and render us acquainted with its cause:

I. If sulphuric acid be poured on the concentrated acetic acid, and then distilled, the first consequence is the production of considerable heat, the colour of the mixture becomes red, and increases in intensity, until at length it becomes black. A great quantity of carbene is precipitated by the continued action of the heat, at the same time that much sulphureous gas is disengaged.

II. I put separately into two glass retorts equal parts of the acetic and acetous acids, at the same degree of concentration; I poured upon each one-fourth of its weight of sulphuric acid. This mixture produced a very strong heat.

The colour of the mixture of sulphuric acid with the acetic acid at first appeared of a pale yellow, while that of the acetous acid did not change colour.

The mixture of acetous acid arrived at the point of ebullition sooner than the other. It became yellow after the evaporation of one-third of the fluid, and its colour grew deeper and deeper, until it had acquired the tinge of highly-coloured wine.

The mixture of the acetic acid was not coloured in the same proportion, and it never acquired a deeper tinge than that of straw-coloured white wine.

The distillation being long kept up, and urged by a strong fire, caused copious white vapours of sulphureous acid to pass over; and towards the end the two residues became colourless, and contained nothing but the concentrated sulphuric acid.

The two first third parts of the product of the distillation had nearly the same smell and taste, and the acetous acid appeared to me to have been brought to the state of acetic acid by its decarbonization in the retort.

III. I saturated, with pure pot-ash, 100 parts of each of these two acids, and put the solution to evaporate, and obtained, from both, white foliated deliquescent salts. I put equal parts of each of these salts into two retorts, and exposed them to an equal heat, which was gradually increased till it became violent. There passed at first into the receiver of the apparatus in which the acetate was placed, two or three drops of offensive smelling acid water, which indicated no acidity to the taste. The distillation of the acetite produced nothing but certain vapours, which emitted a similar odour.

The salt was first liquified, and then became black, in both the retorts. In proportion as I increased the heat, the retorts and receivers became lined with a white fume, which afterwards disappeared.

In both retorts were left black residues, upon which I poured boiling distilled water. I washed them several times, in order to deprive them of all soluble matter.

These residues being dried, presented all the characters of carbene. Their weight compared with that in salt made use of, afforded the following results:

The acetate afforded one-seventeenth part of its weight in carbene.

The acetite afforded one-thirteenth part.

There is, consequently, a difference between the acetous and the acetic acids; and this difference arises from the greater proportion of carbene in the acetous beyond that in the acetic acid.

It appears to me, that the phenomena presented by the distillation of the acetite of copper, ought to lead us to the same consequence. In fact, the acetite of copper is merely a solution

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of the oxyde of copper in the acetous acid, and when this salt is distilled, the acetous acid is decarbonated. Part of the carbone combines with the oxygen of the oxyde of copper, and escapes in the form of carbonic acid; while the other part remains in its solid form in the retort along with the oxyde itself. The acetous acid, thus deprived of a portion of its carbone, passes into the receiver with characters which no longer appertain to the acetous acid, and the oxyde of copper is nearly reduced to the metallic state.

The acetous does not therefore become acetic acid, but by a subtraction of carbone. It appears that the metallic oxydes alone, and some of the acids, are capable of effecting this decomposition.

The acid appears, therefore, to exist in the state of acetous acid in the salt of copper, improperly called acetite. It does not become acetic acid but by distillation, because it is by this process only that it loses a portion of its carbone.

Independent of the proofs we have exhibited in support of this truth, we may adduce the two following facts:

1. When verdigrease is manufactured with the distilled acetous acid of copper, a pure salt is obtained, which affords by distillation the same products as the crystals of Venus, or the acetate of copper. I have mentioned this fact in a memoir concerning the compound processes of the fabrication of verdigrease. 2. Every one knows that vinegar is so much the stronger, and more completely approaches in its smell to the acetic acid, in proportion as it is more completely deprived of the extractive matter with which it was combined. We may, in this case, consider the acid of vinegar as originally existing in a state nearly saporaceous, which diminishes its action, and weakens its properties. It is disengaged from this addition, 1. by simple repose, which suffers part of the extractive matter to precipitate, or fall down; 2. by the sulphuric acid, which decomposes and more completely carbonizes the extractive matter; 3. by the oxyde of copper, which retains a portion of the same principle with which it appears to form a pyrophoric combination.

I was desirous of ascertaining whether the acetic acid could be brought back to the state of acetous acid, by causing it to resume, in the course of distillation upon carbone, that portion which it had lost: but all the methods I made use of to succeed in this respect were ineffectual.

From the preceding facts I conclude: 1. That there is a difference between the acetous and the acetic acids. 2. That this difference arises from a smaller proportion of carbone in the acetic acids, than in the acetous. 3. That the acids in the acetous state in metallic salts. 4. That it does not pass to the acetic state but by decarbonization. 5. That the difference between this acid and some others, equally susceptible of modifications by a change in the preparation of their constituent parts, is, that in this the oxygen does not appear susceptible of addition, or subtraction; but its carbone alone undergoes which variation, and determines all its changes; whereas in the other acids the oxygen is the principle which is more particularly subject to variation, and occasions the changes observable in their properties.

## SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

*Lantern Pinions of Glass for Mill-work.*

CITIZEN MOLARD has communicated to the French Institute an account of the advantages of making the trundles in lantern pinions of glass; to work against the hard wooden teeth of wheels in mill-work. The trundles are set in the same manner as those of iron, only taking the necessary care which the nature of the material must require. A pinion of this kind, in which the trundles were two inches in diameter, did not undergo the least wear, or alteration of the surface of the glass, in 18 months' use; and the wooden teeth had lost about one-twelfth part of an inch. Iron trundles wear out the teeth in about four months, and load the work with much more friction. The kinds of glass which were tried, were green window-glass, white sheet-glass, and the white glass for goblets (*goblerie*). This last was the hardest and best. The inventor, Cit. Renaut, of the department of La Meurthe, thinks bottle-glass might be of good service. He does not blow the trundles, but makes them solid. It is probable, that with us the glass of wine bottles might be the cheapest, and most valuable for its hardness and tenacity, if it can be easily wrought to the figure by the workmen who are employed upon it.—*Decade Philos. No. 5, Year VII.*

*Institute of the Ligurian Republic.*

THE lately established Institute of the Ligurian Republic is composed of 72 members, of which 36 are resident, and 36 associates, inhabitants of the republic, divided into two classes, and each of these into three sections.

The first is the class of mathematical and physical sciences, of which the three sections are: 1. Agriculture, commerce, and manufactures. 2. Nautical science, mathematics, natural philosophy, and natural history. 3. Chemistry, botany, anatomy, medicine, and surgery.

The second class includes (moral) philosophy, literature, and the fine arts; of which the sections are: 1. The art of reasoning, and analysis of the operations of the understanding, grammar, eloquence, and poetry. 2. Politics, history, and antiquities. 3. Arts of design.

On the 14th Brumaire (Nov. 4) the Executive Directory of the Republic repaired to the Hall of Session of the Institute, and the establishment was opened by the formality of proclaiming the members, &c. after which the Institute proceeded to choose their president and secretaries, and to organize their future labours.

*Institute of Cairo.\**

THE learned men who accompanied Buonaparte in his expedition have formed a national Institute at Cairo, composed of four classes, each consisting of twelve members. They hold their meetings on the first and sixth day of each decade.

The names of the members of each class are as under:

*Class I. Mathematics.* Andréossi, Buonaparte, Costaz, Fourier, Gerard, Lepère, Leroi, Malus, Monge, Nouette, Quefnot, Say.

\* From the *Magazin Encyclopedique*, IV. 552. Nivose VII.

*Class II. Natural Philosophy.* Beauchamp, Berthollet, Champi, Conté, Delisle, Descotils, Desgenettes, Dolomieu, Dubois, Geoffroy, Savigny. (There is one place vacant.)

*Class III. Political Economy.* Caffarelli-Dufalga, Gloutier, Poussielgue, Sulkowsky, Sucy, Tallien. (Six places vacant.)

*Class IV. Literature and the Arts.* Denon, Dutertre, Nery, Parceval, Redouté, Riegel, Venturi, Raphael, a Greek priest. (Four places vacant.)

On the 6th Fructidor, in the year VI. (Aug. 23, 1798), at seven in the morning, the Institute of Egypt held its first sitting, at which Buonaparte presided. The officers were elected: Monge, president; Buonaparte, vice-president; and Fourier, perpetual secretary; in whose place, on account of his absence at Rosetta, Costaz was provisionally appointed.

In this first sitting Buonaparte made the following propositions:

1. *What are the best methods of improving the construction of ovens to bake bread for the army?* The commissaries named to examine this question were, Berthollet, Caffarelli, Say, and Monge.

2. *What product can be substituted instead of hops in making beer?* Commissaries, Berthollet, Malus, Costaz, Gloutier, and Desgenettes.

3. *How may the waters of the Nile be clarified, and rendered pure?* Commissaries, Monge, Berthollet, Costaz, and Venturi.

4. *Are wind, or water mills, to be preferred? (in the present circumstances).* Commissaries, Caffarelli, Malus, Say, and Costaz.

5. *What are the resources for procuring gunpowder?* Commissaries, Andréossi, Malus, and Venturi.

6. *What is the state of legislation in Egypt, and how can it be meliorated?* Commissaries, Say, Sulkowsky, Tallien, and Costaz.

7. *A project of regulation (of the Institute?)* Commissaries, Monge, Caffarelli, Tallien, Geoffroy, Costaz.

(To be continued).

Cases of the Diabetes Mellitus, with the Results of the Trials of certain Acids, and other Substances, in the Cure of the Lues Venerea. By JOHN ROLLO, Surgeon-General, Royal Artillery. Second Edition, with large Additions.

THE second edition of this treatise was announced at p. 427 of the present volume, but time did not then permit me to give an account of the additions and facts now first published.

The additional cases and communications on this diabetes, appear to establish the author's opinion respecting the efficacy of diet consisting entirely of animal food, when rigorously persevered in. The dissections likewise prove, that the sweet urine does not proceed from any organic affection of the kidneys, but most probably from some vitiated state of the stomach, and organs of digestion.

The additional cases are, two from Dr. Gerard of Liverpool; a continuation of Walker's case, by Dr. De la Rive; four from Dr. Cleghorn of Glasgow, with a continuation of his former cases; some remarks and cases by Dr. Storer of Nottingham, with a continuation of Dr. Aldrich's case; one from Dr. Jamefon, surgeon of the Royal Artillery; one from Mr.

Sherriff

Sheriff of Deptford; one from Mr. Houlston of London; three from Dr. Pearson of St. George's Hospital, with remarks; one from Dr. Marshall, with the dissection; one from Dr. Willan; and one from Mr. Thomas of Leicester-square, with the dissection.

Dr. Rollo informs us, that, since the first dispersion of his notes on Capt. Meredith's case, in Jan. 1797, the number of diabetic patients, which he has either seen or heard of, amounts in all to 48; a circumstance which proves, that this complaint has been frequently overlooked, and is by no means so rare as has been generally supposed. At the end of the first part are some experiments and observations on urine and sugar, by Mr. Cruickshank.

In the second part, we find a continuation of the trials of the nitrous acid, and other oxygenating remedies, in the lues venerea; which appear to establish their efficacy in this disease, both in its primary, and secondary stages. The number of cases cured in this way, since March 1797, amount to 155; of these, 59 were cured by the nitrous acid; 59 by the oxygenated muriate of pot-ash; 7 by the oxygenated muriatic acid, and muriate of manganese; 3 by lemon juice; 11 by the nitrous and other acids, combined with the oxygenated muriate of pot-ash; and 16 by a combination of mercury, with the new remedies.

In a few instances, where these medicines had not been continued for a sufficient length of time, relapses occurred, and the secondary symptoms made their appearance; but all these were afterwards completely cured, by persevering in the same mode of treatment.

At the conclusion of this part are some useful and necessary observations, with regard to the management of these remedies, more especially the oxymuriate of pot-ash: but for particulars, the reader must refer to the work itself; of the character and importance of which, the public is sufficiently aware.

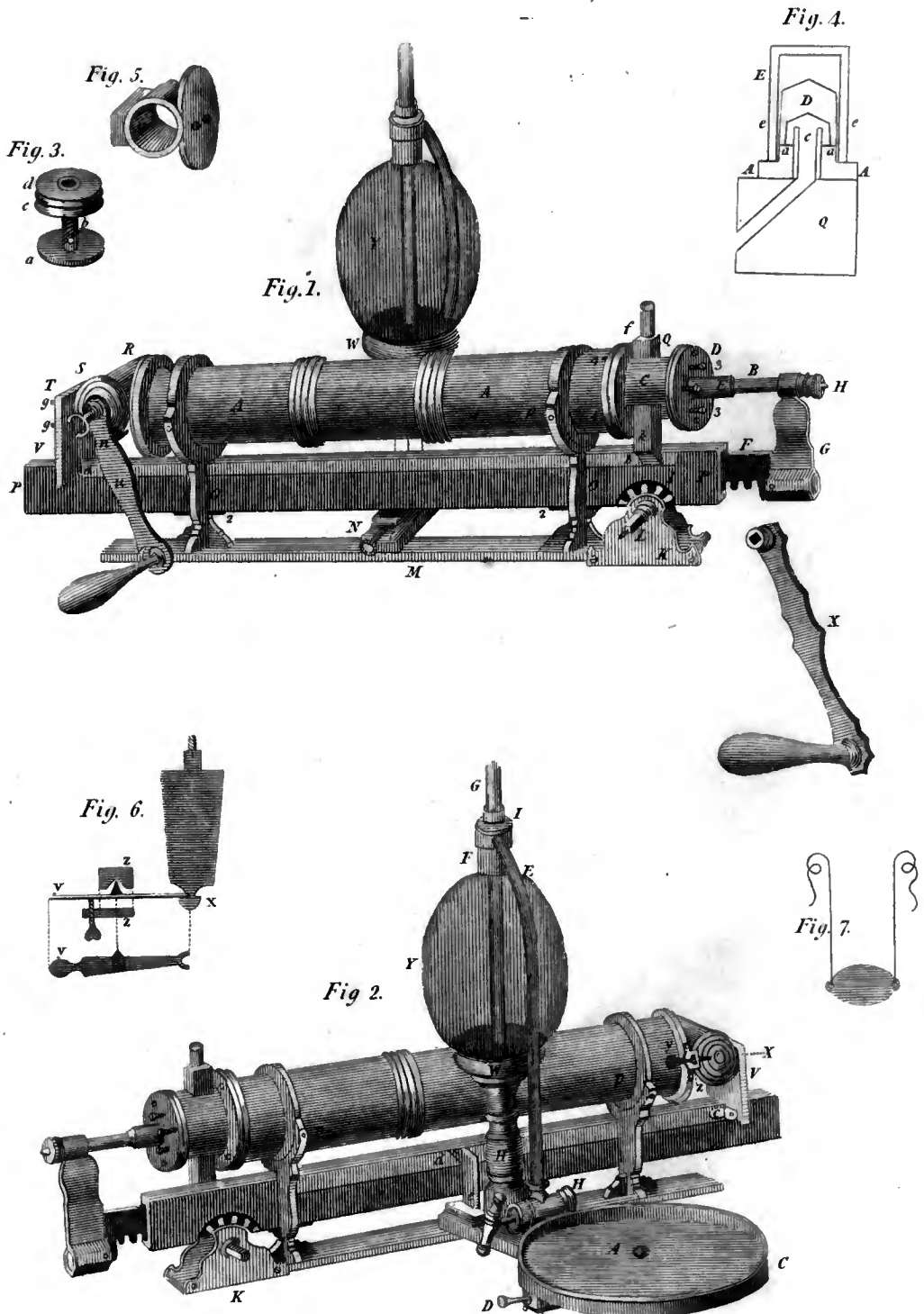
Moyens d'apprendre à compter surement et avec facilité, ouvrage posthume de Condorcet; or, Methods of learning Accounts with certainty and ease. One volume, 8vo. 132 pages. Sold by Maulardier at Paris, year vii.

The distinguishing character of these elements, says the editor, are, that they exhibit the elements of arithmetic and logic at the same time. The author uses the decimal system. Mag. Encycl.

Mr. Lowry, engraver, of Titchfield-street, the artist alluded to at page 429 of the present volume, has shewn me his machine for ruling, which is very different from mine, in all its parts. As he has no actual division in the part which produces the shift, he can regulate his distances to incommensurate, as well as commensurate, measures. The parallax of the ruling point, against which I had made no provision, is, by a very simple and happy contrivance, taken away in common ruling, or rendered variable at pleasure, for the purpose of thickening the stroke in shading. This, together with his great professional skill and science, has given Mr. Lowry's engravings the degree of precision and effect for which they are so justly admired.

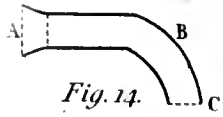
In July 1786, I communicated to the celebrated Mr. Troughton, of Fleet-street, a small apparatus, for securing the point from lateral deviation, in dividing mathematical instruments, which I did not think necessary to add to the instrument in plate 20.—The execution of that plate, in which the point was left to the usual management of the artist, shews, however, that such a contrivance is necessary, and I shall take an opportunity of describing it hereafter.

*Air Pump by the Rev. James Little.*

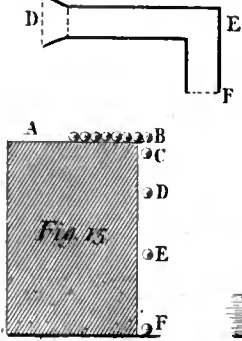




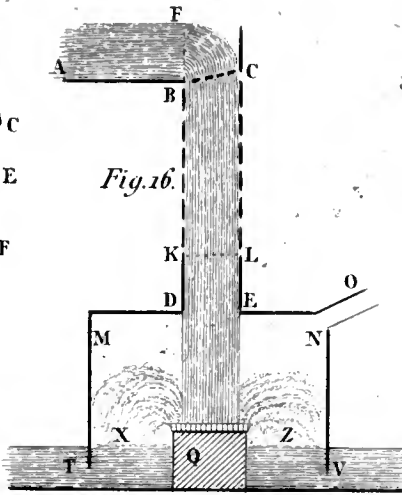




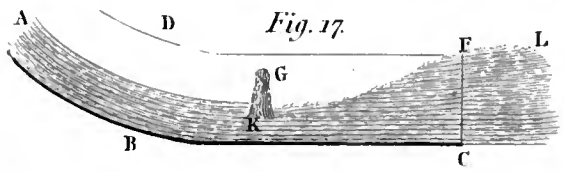
*Fig. 14.*



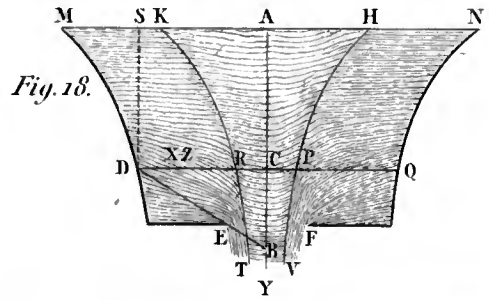
*Fig. 15.*



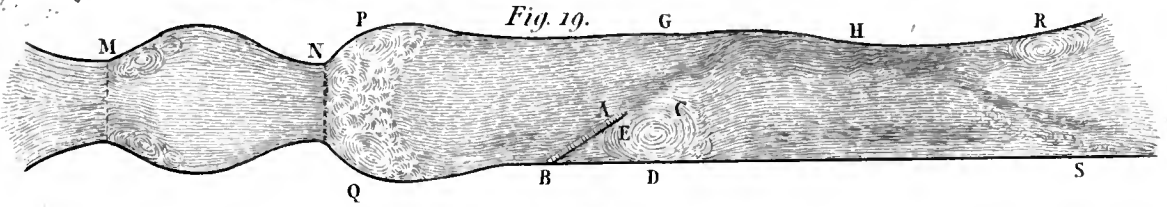
*Fig. 16.*



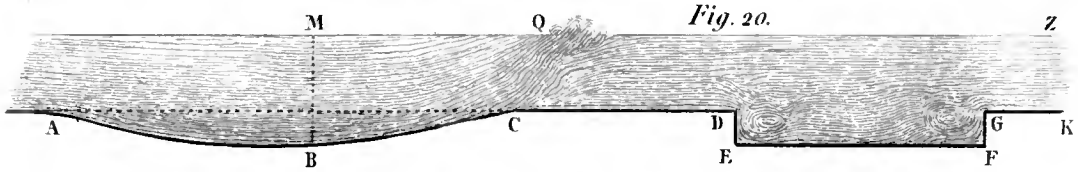
*Fig. 17.*



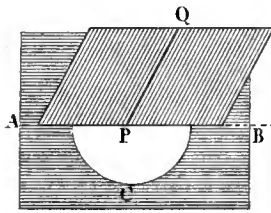
*Fig. 18.*



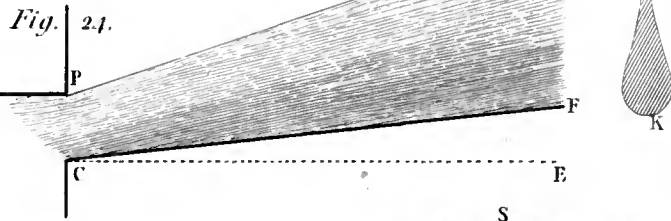
*Fig. 19.*



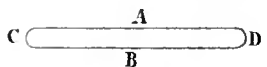
*Fig. 20.*



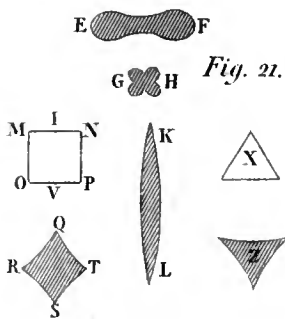
*Fig. 21.*



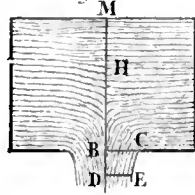
*Fig. 22.*



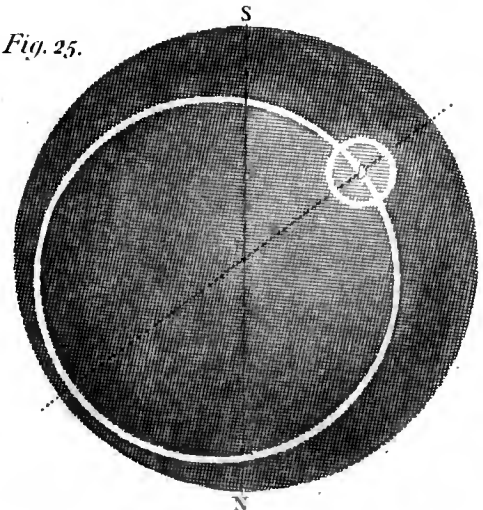
*Fig. 23.*



*Fig. 24.*



*Fig. 25.*





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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MARCH 1799.

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ARTICLE I.

*An Account of Improvements in Electrical Batteries; a Method of augmenting their Power, with Experiments, shewing the proportional Lengths of Wire fused by different Quantities of Electricity, and a Description of a new universal Electrometer. By MR. JOHN CUTHBERTSON\*, No. 53, Poland-street, London, 1799.*

IN the year 1774, Mr. E. Nairne made an Electrical Machine, far superior in acting power to any that had been made before\*, and a Battery more judiciously constructed and larger than any former one, with which he made a number of interesting experiments. One in particular, affords an accurate measure of the power of his battery, compared with such batteries as have been made since that time. I mean the experiment of melting or dispersing a metallic wire. His battery contained 50 square feet of coated surface; and he found that it was capable of receiving a charge so high, that the discharge melted 45 inches of iron wire of  $\frac{1}{156}$  part of an inch in diameter, which answers to about  $\frac{7}{8}$  of an inch in length for each square foot, which was the greatest length of wire ever melted. We have no account of this experiment having been afterwards repeated on a scale of any considerable magnitude till the year 1785; when I constructed a battery for the Teylerian society, at Haerlem, containing 135 square feet of coated surface. With this battery 180 inches of the same sort of wire was melted, which seemed to be much more than in proportion to the size of the battery, as this was about 1,3 inches for each square foot. This battery was afterwards increased to 225 square feet of coating, and with this 300 inches of the same sort of wire melted, which was also at the rate of 1,3 inches for each square foot. Some time after this, I made another battery for the same society, containing 550 square feet of coating, composed of 100 jars of  $5\frac{1}{2}$  square feet

\* Communicated by the author. † Philos. Trans.

each. The same sort of wire was not tried with this; but it could be calculated from other sorts of wire which it melted, that it was capable of melting 655 inches, being also at the rate of 1.3 inch for each square foot. This increase of power, which is almost double that of Mr. Nairne, might be attributed to the acting power of the machine; for though Mr. Nairne's machine possessed the strongest acting power of any machine made at that time, yet it could not be supposed to possess that high charging property of the Haerlem machine.

Since my return to London, I have made several batteries, commonly composed of 15 jars, each containing 168 square inches of coated surface, consequently the whole battery contains 17 square feet of coating. This battery, according to the proportion of that made by Mr. Nairne, should fuse 6.3 inches, and in proportion to the Haerlem batteries, it ought to fuse 22 inches; but instead of following that proportion, it is found to fuse 60 inches, which is an astonishing increase of force. For the battery is only about one-third part of that of Mr. Nairne, and fuses a much greater length of wire; and though it is only  $\frac{1}{3}$  part of that at Haerlem, yet it fuses  $\frac{1}{3}$  of the length of wire. It seems difficult at first sight to account for this advantage. I have before remarked, that the proportional difference between the charge of the battery at Haerlem and Mr. Nairne's, might be accounted for, from the high charging power of the great machine; but the result of the last-mentioned experiments overturns that notion; as it can by no means be supposed, that a single 2-feet plate machine, which I have used to charge the battery of 17 square feet so high as to fuse 60 inches, can have a higher charging power than that at Haerlem; so that it must proceed from some other cause. It might be questioned whether all the batteries were alike judiciously constructed. As to Mr. Nairne's, it had certainly faults, both with respect to the coating and the mounting of the jars; but the batteries at Haerlem were as judiciously constructed as my present one, which I am speaking of, and which exceeds them in such an astonishing degree in its proportional force. The only difference between my present batteries and these at Haerlem is in the glass. They were composed of glass, blown in Bohemia, and those which I make here are of white flint-glass. I mention this fact, but I am not inclined to think that the cause of the difference depends on the glass, because I remember to have melted the same quantity of wire with one jar of that kind of glass when in Amsterdam, as I do at present with white flint-glass; so that it only remains now to be sought for in the manner of using or charging each battery, and here we shall probably find a means of solving this paradox.

With regard to the batteries at Haerlem, they were never attempted to be charged but in dry weather, being such as was then commonly called favourable for electrical experiments. There was no convenience in the room where the machine and batteries were used, for making a fire, which was therefore ill calculated for electrical experiments: the batteries previous to charging, were made as clean and dry as possible; and if they received a charge so high as to cause a spontaneous explosion, they were then looked upon to be in their most favourable state.

It was about this time that we were told by Mr. Brooke, that a coated jar would take a higher charge when dirty, than when clean; but the degree of dirtiness was so ill defined, that I must own I never could dirty a single jar so as to answer, or to come near what was said of it; and to pretend to bring all the jars in a large battery, containing upwards of two hundred, into that state of dirtiness was never attempted; neither does it appear that Mr. Brooke ever thought

thought of dirtying his battery jars, as he only mentions trying two small bottles, whose charging property was very differently increased by his method of dirtying.

Some time afterwards, in the year 1792, I happened casually to discover that a coated jar, when it was a little dampish in the inside above the coating (which is always the case when a jar is fresh coated), would take a higher charge than it would do after it had been coated for some time, and was quite dry in the inside; and also, if the atmosphere was in a moist state, and the jar not dried in the inside, it would take an equally high charge. From this it appeared evident to me, that if I could, by any means, render the inside of jars damp, it would answer the same purpose. Breathing into a jar was tried, and the success was such, that it would receive and retain nearly double the quantity of electric fluid it could retain when dry: and in trying to fuse wire with the charge of one jar in a dry state, no more than 5 inches could be fused, though after breathing into it, 12 inches were fused.

This method appeared at first sight to have increased the force to more than double; but notwithstanding so evident and striking an effect, I did not think of trying what would be the result of charging a battery, after the jars had been breathed into; being deterred, as I suppose, from the idea of its being so contradictory to the common method of using batteries, which was never attempted to be done, but when the atmosphere was in a tolerably dry state, and the jars previously cleaned. But in March 1796, being engaged in a course of experiments, when the atmosphere was so very dry, that a spontaneous discharge always took place before I had a sufficient force to answer my purpose, it then occurred to me to try what the effect of breathing into the jars of the battery would be. In this trial, or research, it became necessary first, to ascertain the real charge that the battery was capable of receiving, before a voluntary explosion took place. This battery contained 17 square feet of coated glass, and was composed of 15 jars: it was found in the then state of the atmosphere to be incapable of fusing a greater length of wire than 18 inches. But after breathing into each jar through a glass tube, it took a charge which fused 60 inches, to my very great surprise and satisfaction, as I then thought I had obtained a method of making one battery perform the function of three; because three times the quantity of wire was fused, as appears by comparing this with what had been performed, by increasing the surface of batteries by former electricians. This notion seemed to be justified, by observing in Dr. van Marum's works, that I had enlarged his batteries at three different times; his first contained 135 square feet coating, the second 225 square feet, and the third 550; and the highest charge of the first was just sufficient to fuse 180 inches of iron wire of  $\frac{1}{16}$  of an inch diameter, or 6 inches of iron wire of  $\frac{1}{40}$  of an inch diameter; the highest charge of the second fused 300 inches of the first-mentioned wire, or 10 inches of the last-mentioned; the highest charge of the third fused 25 inches. We find that these batteries increased in power in the same proportion as the coated surface was increased. I was present when the wire was fused by the two first-mentioned batteries, but at the third not; however, we have no reason to doubt Dr. van Marum's report. These experiments supported me in my first notion, that I had discovered a new method of increasing the force of a battery to three times its usual power; but being unable to account for it to my own satisfaction, I resolved to make a course of experiments, in order to throw some light on the subject.

The chief experiments which have been made on the force of batteries, by Mr. Brooke\*, at Norwich, in the year 1786, and by Dr. Van Marum, in 1785 and 1795. The results were very different. Some experiments which I made in Holland, and afterwards repeated here, did not seem to confirm either of the two. All that had been done either by Dr. van Marum, or myself, was done without the help of such an electrometer as could indicate the proportional quantities of electric fluid with a sufficient degree of accuracy.

Mr. Brooke was possessed of an instrument of his own invention, with which it was possible to ascertain the comparative strength, if managed with the same dexterity as Mr. Brooke himself possesses. But this instrument came so high in price, and was so very difficult in its use, that few electricians provided themselves with it; which, perhaps, is one reason, why this subject has so long remained in obscurity. I have lately had the good fortune to invent an electrometer which has all the properties that such experiments require, and is very simple and easy in its use; and with this I found myself enabled to go through such experiments as were necessary, with greater accuracy than any which had been made before.

The electrometer is represented in Pl. xxiii. G H is a long square piece of wood, about 18 inches long, and six inches broad, in which are fixed three glass supports, D E F, mounted with brass balls, *a b c*. Under the brass ball *a*, is a long brass hook; the ball *c* is made of two hemispheres, the under one being fixed to the brass mounting, and the upper turned with a groove to shut upon it, so that it can be taken off at pleasure. The ball *b* has a brass tube fixed to it, about three inches long, cemented on to the top of F, and the same ball has a hole at the top, of about one-half inch diameter, corresponding with the inside of the tube. A B is a straight brass wire, with a knife-edged centre in the middle, placed a little below the centre of gravity, and equally balanced with a hollow brass ball at each end, the centre, or axis, resting upon a proper shaped piece of brass fixed in the inside of the ball *c*; that side of the hemisphere towards *c* is cut open, to permit the end *c* A of the balance to descend till it touches the ball *a*, and the upper hemisphere C is also cut open to permit the end *c* B to ascend; *i* is a weight, weighing a certain number of grains, and made in the form of a pin with a broad head; the ball B has two holes, one at the top, and the other at the bottom; the upper hole is so wide, as to let the head of the pin pass through it, but to stop at the under one, with its shank hanging freely in *b*; a number of such pins are commonly made to each electrometer of different weights; *k* is a common Henley's quadrant electrometer, and when in use, it is screwed upon the top of *c*.

It is evident from the construction, that if the foot stand horizontal, and the ball B be made to touch *b*, it will remain in that position without the help of the weight *i*; and if it

\* Though I had read Mr. Brooke's book, as I thought, with a sufficient degree of attention when it was first published, I did not, till lately, observe that it contained any experiments relating to this subject, till I began to write this paper, and had occasion to look into his book for some references. I believe these experiments had escaped Dr. van Marum's notice likewise, as I never heard him speak of them when he was making others of the same kind. Though Mr. Brooke's experiments were conducted with much skill and intelligence, they are so confusedly arranged, that this had entirely escaped my notice; and I doubt not but that it had also escaped the notice of several other electricians.

should by any means receive a very low charge of electric fluid, the two balls *b*, B, will repel each other; B will begin to ascend, and, on account of the centre of gravity being above the centre of motion, the ascension will continue till A rest upon *a*. If the balance be set again horizontal, and a pin *i*, of any small weight, be put into its place in B, it will cause B to rest upon *b*, with a pressure equal to that weight, so that more electric fluid must be communicated than before, before the balls will separate; and as the weight in B is increased or diminished, a greater or less quantity of electric fluid will be required to effect a separation.

When this instrument is to be applied to a jar, or battery, for which purpose it was invented, one end of a wire, L, must be inserted into a hole in *b*, and the other end into a hole of any ball proceeding from the inside of a battery, as M\*: *k* must be screwed upon *c*, with its index towards A; the reason of this instrument being added, is to shew, by the index continuing to rise, that the charge of the battery is increasing, because the other part of the instrument does not act till the battery has received its required charge.

If this instrument be examined with attention, it will be found to consist of three electrometers; and answers three different purposes, namely, a Henley's electrometer, Lane's discharging electrometer, and Brooke's steelyard electrometer; the first not improved, but the two last, which were very defective when first invented, I flatter myself are here brought to perfection. As the only use of Henley's electrometer to this instrument is, as I have said before, to shew, by its continuing to increase in divergency, that the battery continues to receive a still stronger charge, it required no improvement; but Lane's electrometer, in its primitive state, could by no means answer the required purpose for batteries, because the ball intended to discharge the battery, was necessarily placed so near to the ball of the battery, that dust and fibrous particles were always attracted by and adhered between the two balls, so as to retard the charging, and often render a high charge impossible: whereas, in this, they are placed at four inches asunder; and when the desired height of charge is obtained, and not before, the ball of the electrometer moves of itself nearer to the ball which is connected with the outside of the battery, and causes a discharge. The defects in Brooke's steelyard electrometer were, 1st, that it could not cause a discharge, and 2dly, the difficulty of observing the first separation of the balls caused great error. If it were not placed in an advantageous light (which the nature of the experiments could not always permit), it would not be seen, without the attention of an assistant, which is sometimes unpleasant, and cannot always be commanded. But the instrument which I have described, requires no attention or assistance; for as soon as the separation takes place between B and *b*, the ball A descends, and discharges the battery of itself.

By this combination and improvements, we possess in the present instrument all that can ever be required of an electrometer; namely, by *k*, we see the progress of the charge; by the separation of B, *b*, we have the repulsive power in weight; and by the ball A, the discharge is caused, when the charge has acquired the strength proposed.

\* A chain, or wire, or any body through which the charge is to pass, must be hung to the hook at *m*, and carried from thence to the outside of the battery, as is represented by the line N.

*Experiments made with a View to determine in what Degree the charging Capacity of coated Jars is increased by breathing into them before the charging.*

*Experiment 1.* Prepare the electrometer in the manner shewn in the plate, with the jar M annexed, which contains about 168 square inches of coating\*; put into B the pin, marked 15; take two inches of watch-pendulum wire, fix to each end a pair of spring tongs, as is represented at G m, hook one end to m, and the other to the wire N, communicating with the outside of the jar; let the uncoated part of the jar be made very clean and dry; and let the prime conductor of an electrical machine, or a wire proceeding from it, touch the wire L; then if the machine be put in motion, the jar and electrometer will charge, as will be seen by the rising of the index of k, and when charged high enough, B will be repelled by b, and A will descend and discharge the jar through the wire, which was confined in the tongs, and the wire will be fused and run into balls.

*Experiment 2.* Put into the tongs eight inches of the same sort of wire as before, hang one pair of tongs to the hook m, and apply the other to the wire which forms the outside communication: take out the pin in B, and put in its stead one marked 30; all the other part of the apparatus remaining as before, and the uncoated part of the jar being previously cleaned and dried; the machine being then put in motion, the jar and electrometer will charge, as is shewn by the rising of the index as before; but as soon as the jar has received a greater quantity of electric fluid than before, a spontaneous explosion will happen without affecting the balls B b, because the discharge will have passed along the uncoated part of the jar from the inside coating to the outside: whence it follows, that while the jar remains in that clean state, it is incapable of receiving a charge high enough to affect the balls, or even a higher charge than it had received in the first experiment. Let the uncoated part of the jar be therefore rendered, in a slight degree, damp; which is easily done, by breathing into the inside, through a glass tube; put the machine in motion, and no spontaneous explosion will happen, but the balls B b will repel, as in the first experiment, and the discharge will happen from A to a, and pass through the wire placed in the circuit; and though it was eight inches, it will be fused in the same degree as two inches in the last experiment, namely, the wire seen red hot the whole length, and then fall into balls.

Very different degrees of fusion are caused by electric discharges, which may cause great mistakes, if not well attended to. It is proper to adhere to the degree above-mentioned, and particular care ought to be taken to lay the wire, intended for fusion, straight, without any bendings or angles in it. The wire used in the two last experiments, was that which is commonly called watch-pendulum wire, which is flatted; and as it approaches very near to such a sharp edge as might be supposed to affect the experiment, by permitting a dissipation of the electric fluid in its passage, round wires were tried, and the result was the same.

\* Take out the pin in B, and observe whether the ball B will remain at rest upon b; if not, turn the adjusting screw at C, till it just remains upon A.



By the last experiment it appears, that breathing into the jar had increased its charging capacity nearly in the same proportion as it had done the batteries: after breathing, it received a charge sufficient to fuse four times the length of wire it did when clean; but by the weight in the electrometer, and also by the greater number of revolutions given before the discharge happened, it might be supposed that the jar had received only a double charge.

The following experiments are intended to shew the lengths of wire, which are just fused by various quantities of electric fluid at the same intensity.

*Experiment 3.* For this purpose, a second jar was placed at the wire L, the pin marked 30 was taken out, and 15 put in its place, two inches of the same sort of wire as used in the last experiment was placed in the circuit, every other part of the apparatus remaining unaltered; the machine was then put in motion, till B begun to ascend, when it was stopped, and before A could reach a, one of the jars was pushed from the wire L (to do which, there is always sufficient time while the electrometer is in motion), the discharge was effected, and the two inches of wire was just fused.

*Experiment 4.* The jar which was pushed away in the last experiment was discharged, and placed at the wire L, as before, and eight inches of the same sort of wire placed in the circuit; the outside coating of the jars either touched each other, or had a metallic communication. All the other part of the apparatus remained as before, and the machine was put in motion, till B begun to ascend; the jar was not removed, as in the last experiment, but suffered to discharge with the other, and the eight inches of wire was fused in the same degree as the two inches in the last experiments.

It is evident from the position of the apparatus, that the quantity of electric fluid discharged in the last experiment must be double that of the former; yet, in repeating the experiment, I had different results, which made me again suspect the edges of the wire, I therefore resolved to take round wire, and of as large a diameter as could be conveniently fused.

*Experiment 5, with three jars.* Iron wire of  $\frac{1}{15}$  part of an inch in diameter, and six inches in length, was placed in the circuit; three jars were placed so that the balls proceeding from their insides touched the wire L, and their outside coatings touched each other. The machine was turned till B begun to ascend, the discharge was caused, and the whole length of the wire was just run into balls.

*Experiment 6, with three jars, one removed.* Two inches of the same sort of wire was placed in the circuit in the same manner as the last, and the three jars remained; the machine was turned till B begun to ascend, then one of the jars was drawn away, consequently only two discharged, and the wire just run into balls as the last.

*Experiment 7, with four jars.* Wire of  $\frac{1}{15}$  part of an inch was taken, and four jars placed in contact with the wire L, with their outside coatings in contact with each other, and eight inches of wire was placed in the circuit; the weight in the electrometer remained as before; the machine was then put in motion till B begun to ascend, the discharge was effected, and the wire was fused, and run into balls. The experiment was repeated with the same sort of wire  $8\frac{1}{2}$  inches long; the discharge was just sufficient to run it into balls.

Repeated with nine inches of the same sort of wire, and the discharge caused it to be red hot the whole length.

*Experiment.*

*Experiment 8, with four jars, two removed.* Two inches of the same sort of wire was placed in the circuit, all the jars remaining as in the last experiment, the machine put in motion, till B begun to ascend, then two of the jars were drawn away; the discharge was caused, and the wire was fused, and run into balls.

Repeated with the same sort of wire  $2\frac{1}{2}$  inches long, the discharge caused it to be red hot the whole length.

*Experiment 9, with fourteen jars.* Wire of  $\frac{1}{100}$  part of an inch diameter was taken, eight inches long, and proceeded according to Experiment 7; it was fused and run into balls.

*Experiment 10, with fourteen jars, seven removed.* Two inches of the same sort of wire was taken, and proceeded with, according to Experiment 8; it was fused and run into balls.

The result of the foregoing experiments proves sufficiently, that double quantities of electric fluid, in the form of a discharge, will melt four times the length of wire of a certain diameter; and Experiments 5 and 6 prove that when one-third part is added to two, three times the length of wire was fused.

These experiments give reason to apprehend some error in Dr. van Marum's experiments, because he found his batteries to increase in power only in the same proportion as the coated surface was increased, viz. that double surface of coated glass only could fuse double lengths of wire of the same diameter.

The doctor might, perhaps, have been led into a mistake in the following manner: first, he may not have charged the batteries to an equal height, as he did not, at that time, possess an electrometer of sufficient accuracy for that purpose; and, secondly, he may not have been aware of the different degrees of fusion caused by electric discharges, but only judged of the force by the wires being converted into balls; by which great mistakes may happen. For if a wire be taken 18 inches long, and of such a diameter, that when a jar or battery is charged to such a height as just to cause it to run into balls, much shorter lengths of that same sort of wire may be subjected to the same force, and still be only converted into balls by it; even if only seven inches were taken, nothing but balls will appear; the only difference will be, that the balls will be smaller, and dispersed to a greater distance, which might be easily overlooked. If six inches of the same sort of wire be taken, it will be converted into balls and flocculi, or brown oxyde of iron; so that to be accurate in this point, the lowest degree of fusion must be had, which is known when the charge has passed, by the wire being seen red-hot the whole length, and afterwards run into balls.

Having now sufficiently proved by experiment, in what proportion different quantities of electric fluid act upon different lengths of wire, which was required to be known, in order to explain in what proportion the charging capacity of a jar or battery is increased by breathing into it, before the charging begins, I shall proceed in the next place to explain this point.

The opinion that I had at first entertained (though supported by Dr. van Marum's experiments), that I had found out a method of increasing the charging capacity of batteries to three times their usual force, was not supported by the facts that the usual power of a clean and dry battery, containing 17 square feet coated surface, namely, that of fusing from 18 to 22 inches of iron wire of  $\frac{1}{100}$  part of an inch in diameter, will be increased by breathing into the jar, so as to become capable of fusing 60 inches. If the first-mentioned effect be taken at

a mean

a mean, it will be 20, then the increased effect, gained by breathing, will be just  $\frac{2}{3}$ , as determined by the wire; and experiments 5 and 6 prove, that in order to produce such an increased effect, an addition of  $\frac{2}{3}$  part of the coated surface must be added to the battery, which is about 816 square inches. This would amount to an addition of 54 square inches to each jar; or, in other words, if that quantity of coating could be added to each jar, the same effect would be produced as when breathed into. But this would require the coating to be within an inch of the top, which would render the battery unchargeable, at least, to that degree. A battery of 15 jars constructed in the usual manner, will, therefore, by this treatment, become equivalent in power to 21 jars of the same kind, if clean and dry.

To explain the effect of breathing into the jars, appears to be a matter of some difficulty. This experiment has been shewn to several electricians, and different opinions have been advanced, most of which seem to imply, that breathing acts as a coating to the uncoated part, which will appear in the sequel to be absurd. Mr. Nicholson's opinion (see *Philos. Journal*, II. 219) comes much nearer to the truth, though it does not appear to me to be sufficient to account for the effect produced. I admit, with him, that a spontaneous explosion over the uncoated part is most commonly caused by undulation; but that this undulation is caused by the discharging of different charged zones, will be difficult to prove, because such zones cannot exist upon clean and dry glass.

When the uncoated part of a Leyden jar is made perfectly clean and dry, and the jar set to the conductor of a machine in action, it will begin to charge, and, while charging, the coated part of the jar, and the wire which is connected with it, become equally charged, and each endeavours to throw off that surplus of electric fluid which is forcing into them; the coating from its edges upwards, and that part of the wire which is above the coating and within the jar, will endeavour to throw it in all directions, which will cause it to be surrounded by an electric atmosphere, increasing in density as the charge increases. This atmosphere, together with that given out by the coating, fills the whole jar. Part of the electric fluid forced into the coating enters the surface of the glass, but the uncoated part, being clean and dry, both within and without, the inside resists the fluids entering its surface, which is kept suspended at a distance, because the natural electric fluid contained on the outside, finds no means of escape. But the action of the machine still continuing, presses it still closer to the surface, and at last overcomes that resisting force, and some of the particles on the outside give way, which causes an undulation in the inside, and the electric fluid closes instantly in upon its inside surface, and forces a greater quantity from the outside. Flashes, or coruscations, are thus caused, which are always seen when a jar is charging in the abovementioned circumstances: the charge still continuing to be made, forces another quantity from another part of the outside of the jar, and causes a second coruscation and undulation, which may be so strong as to cause a spontaneous discharge; or two or three more coruscations and undulations may happen, before the discharge, according to the steadiness or unsteadiness of the action of the machine, the quantity of electric fluid thrown off from the outside at each undulation, and also the degree of dryness and cleanness of the uncoated part of the jar. A discharge sometimes happens without having previously occasioned any perceptible coruscation. This is the case when the first undulation has been so strong, as to cause the whole

discharge with the first coruscation, the one being so quickly followed by the other that it is imperceptible.

A jar will sometimes, while it is charging, give a great many small coruscations, quickly succeeding each other, which afterwards cease without having caused a spontaneous explosion, though the action of the machine be continued. This happens when the uncoated part is nearly clean and dry, but not perfectly so; its surface still containing some conducting particles, but not so connected, that the electric fluid can pass from one to the other without leaps, or small coruscations on the outside, which permit the electric fluid to spread gradually over its inside surface, and prevent the undulations from being so strong as to cause a discharge.

After this explanation of the cause of the flashes, or coruscations, which are seen upon the uncoated part of a jar while charging, and also that such coruscations produce undulations, which terminate in a spontaneous explosion; it remains now to explain how a jar is charged when the coruscations are prevented by breathing upon the uncoated part.

When a coated jar is breathed into, and then subjected to the process of charging, the electric fluid is forced into it, along the wire in the inside to the coating, where it instantly and equally spreads itself over the whole coated part, and at the same time, though with difficulty, and consequently gradually, it spreads itself over the uncoated part, taking the condensed film of humidity for its conductor, as it proceeds from the edges of the coating upwards towards the mouth of the jar, according to the arrangement of the particles of moisture, and rises higher or lower, depending entirely on their arrangement, and the force with which it is repelled from the machine. If the conducting particles be almost uniformly diffused over the uncoated part, the whole jar, in the inside, will become charged, though the uncoated part will be charged in a much less degree than the coated, on account of the imperfection of the conducting particles which has adhered to its surface; no coruscations will be perceived, on account of the gradual and equal diffusion of the electric fluid over its inside surface: and though the charging be continued, yet, if the exhaled conducting particles be favourably diffused, no spontaneous explosion will happen from one coating to the other, along the uncoated surface, but the jar will either be perforated, or, if it be of sufficient strength to resist that effect, the electric fluid will be seen to run in a stream over the mouth of the jar, as quickly as the machine supplies it. Whenever a spontaneous electric explosion happens, it must be from a body of sufficient bulk and conducting property to contain that quantity of electric fluid at that point from which it explodes, otherwise no explosion ever happens. But the humid conducting particles are just sufficient merely to admit the electric fluid, by the action of the machine, to be spread over the surface of the glass, but in no part of sufficient density, either to receive, or contain an explosion. If, therefore, a spontaneous explosion do happen, it must either proceed from the inside coating, or the wire which is connected with it to the outside; and, if we examine the state of the coating, we shall understand, that the edge of the coating (from which part only it is ever possible to explode), and also above it, to a short distance upwards, is as strongly charged as the coated part; and by the action of the machine it is so strongly loaded with electric fluid, that it is repulsive in all directions, which keeps back, or entirely stops, a spontaneous explosion from the

the edge of the coating. With regard to the wire, the only place from which it explodes spontaneously, is that part which is nearly of an equal height with the edge of the mouth of the jar. The fluid is nearly as much condensed on this part as on the other, so that an explosion from the wire is hindered by the same cause as from the coating. A jar under such circumstances cannot, therefore, explode spontaneously; but the fluid will run over the edge of the jar as quickly as the machine furnishes it, when its charging capacity is full.

I have stated, at page 530, that a jar of the dimensions there given, being clean and dry, can only contain a charge sufficient to fuse 2 inches of a certain wire, and when breathed into, its charging capacity will be so much increased, that it will contain a charge sufficient to fuse 8 inches of the same sort of wire; and a battery of 15 jars, in the first-mentioned state, can only fuse 20 inches, and in the last-mentioned, 60 inches. This increased charging capacity, proceeds, no doubt, from the particles of moisture, though not from their acting as a coating, as has been supposed, but by their being brought into a state or capacity of resisting a spontaneous explosion, so that a stronger charge is forced in upon the coated part. Some of the electric fluid which was forced upon the uncoated part to a certain height (perhaps  $\frac{1}{2}$  an inch, more or less, according to the degree of dampness, and the situation of the particles) may, indeed, be discharged along with that from the coated part; but this is of little importance, and by no means capable of producing that increased effect, which, as I have shewn by experiment, would require an addition of seven jars to a battery of fifteen.

## II.

*Discovery of Sulphate of Strontian, near Sodbury, in Gloucestershire. By G. S. GIBBES, B. M. F. R. S.*

To Mr. NICHOLSON.

SIR,

IF the following imperfect analysis should meet your approbation, I shall feel myself flattered by seeing it noticed in your excellent monthly publication.

I have the honour to be,

Sir,

Bath,

Your humble servant,

No. 28, Gay-street.

GEORGE SMITH GIBBES.

A friend of mine, the Rev. Mr. Richardson, some little time since, shewed me a specimen of a substance which, he said, was found in great abundance in the neighbourhood of Sodbury, in Gloucestershire, where it was used for the purpose of making gravel walks. The stone was composed of a vast number of small crystals, which cohered together with but little force. The crystals were easily reduced to powder, and were not affected by any acid. I exposed equal parts of these crystals in the state of a fine powder and charcoal to the

action of heat, and I found, on pouring the muriatic acid on the substance, when cool, that there was a great disengagement of fulphurated hydrogenous gas.

I filtered this solution in muriatic acid, and I found the crystals exactly similar to those which I had often formed, by adding the muriatic acid to the strontian earth, found in Scotland. This muriate possessed the property of changing the flame of the candle to a most beautiful red colour. A copious precipitation was formed, when the sulphuric acid was added to a solution of this muriate. From these experiments, and from the external characters of the stone, I have no hesitation in believing, that it is composed of the sulphuric acid and strontian earth.

P. S. Should you wish to possess a specimen of this substance, on receiving a line from you, I will take the first opportunity of sending one to you.

### III.

*On the Corundum Stone, from Asia. By the Right Honourable CHARLES GREVILLE, F. R. S.*

(Continued from p. 485, Vol. II.)

**M**R. Tranckell, who resides in Ceylon, and from whose communications I derived lately much information, had, about five years ago, a sapphire, the greater part blue, and the remainder of a ruby colour. I saw, in Romé de L'Isle's collection, at Paris, a small gem, which was yellow, blue, and red, in distinct spots, and he called it oriental ruby. M. de la Metherie, to avoid the confusion of the denomination oriental ruby, with octoedral ruby, calls it a sapphire: with more correctness, I think, the above-mentioned gems should be classed as argillaceous, under the denomination of corundum. I am not uninformed, that corundum is said to be found in France. The Count de Bournon is convinced, that the specimens mentioned in Crell's Journal, as having been found by him in a granite in the Forez, were corundum. M. Morveau also says, he found it in Bretagne; but the Abbé Haüy, in No. 28, of the Journal des Mines, asserts, that the corundum, found in France, is titanite; he does not say whether this observation extends both to the corundum of Bretagne and that of the Forez. In the same manner I had observed in the specimens, which Mr. Raspe called jade, or a new substance from Tiree, on the west coast of Scotland, a great resemblance to corundum; but, having then only had a cursory view of the substance, I am indebted to Mr. Hatchett for the examination of a specimen of it, which he had from Mr. Raspe's collection. The Tiree stone resembles crystallized corundum of the coast, in texture and colour; it is also as refractory, when examined by the blow-pipe, with different fluxes. Its specific gravity is 3,049; consequently nearer the specific gravity of pure corundum than the above-mentioned lump, 2,785, and the matrix of corundum, 2,768. The Tiree stone will scratch glass readily, but not rock crystal; its hardness, therefore, corresponds with that of the matrix of corundum. The substance of the lump, described in page 481, cuts glass and rock crystal, and the Tiree stone, readily.

It will, therefore, be sufficient for me to say, that there is great probability corundum may  
be

be found in Great Britain, and on the Continent of Europe, as well as in Asia; and the above slight essays may show, that observations on corundum, in its different states of purity, may lead to accurate distinction between substances hitherto imperfectly known, and will lead to a revision of the siliceous genus, whereby the argillaceous gems may obtain its due pre-eminence in mineralogy. When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their crystals, they are unavoidably subjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingness of European Jewellers to deal in coloured gems. I have some specimens of a sapphire-blue stone, India cut, very small and pellucid; they were purchased in India as sapphires, and were supposed to be fluor by a lapidary in London, but are cyanite. The above could scarcely have happened, if the stones had been of sufficient size and value to require much examination, the weight and degree of hardness being exceedingly deficient. The colour, therefore, will not be a safe guide. The diamond, whether white, blue, red, yellow, or green, can be distinguished by its crystal, or by its specific gravity and hardness, or, when it is polished, by its lustre. Other stones, which compose the order of gems, might equally depend on their crystallization, specific gravity, polish, and hardness, for a distinct arrangement. The near relation of argil, which Bergman gave to this order, is daily confirmed; and it will be, perhaps, to Mr. Klaproth, more than to any other existing chemist, that we shall owe our correct information on the subject of other gems, as we do on the subject of corundum.

Many of the varieties of corundum, particularly the coloured and transparent sorts, with their regular crystallizations, are yet *desiderata*. Many crystallized stones, from defect of colour, lustre, &c. are of little value in the market, such as jargon, chrysolite, tourmaline; and an infinity of unnamed stones of Ceylon, Pegu, Siam, &c. would be valuable to the mineralogist, if obtained adhering to their strata, and in crystals, when external form is not obliterated. I have no doubt, when it is known how much such information will tend to illustrate the history of the earth, and particularly that of gems: the spirit of enquiry, so laudably afloat in British India, will be directed to attain it. I have not heard of any metallic veins being found in corundum, unless a stone, which Alonso Barba, *lib.* 1, c. 13, describes, should give an instance. "The chumpi, so called from its grey colour, is a stone of the nature of emery, and contains iron; it is of a dull lustre, difficult to work, because it resists fire long. It is found at Potosi, at Chocaya, and other places, with the minerals, negrillos, and rosicleres."

Having mentioned the varieties of crystallized and amorphous corundum, and the miscellaneous facts relative to my collection of that substance, from India and China, it might be sufficient to give an *icon* of the crystal, and close a paper already prolix; but having with satisfaction observed, within the last years, the science of mineralogy gaining ground in Great Britain, from the knowledge acquired by several gentlemen, who have examined the mines, and formed personal acquaintances with the most learned and experienced men on the continent, and also from ingenious foreigners, who have communicated their observations on English fossils, and connected them with the most approved systems, it may, perhaps, be accepted as a sufficient apology for what follows; that I consider it as a desideratum to English mineralogists, to be invited to a preference of permanent characters, which the study of crystallization has collected, and which promises

promises to be a certain method of ascertaining the laws, by which elective attraction arranges and combines molecules of matter. It is true, the progress of crystallography has been extremely slow, and different nations have contributed to its present improvement. It is rather remarkable, that the earliest treatise on metallurgy, of authority, was published in Italy, by Vannoccio Biringuccio, just before Agricola published his Treatise, in 1546, in Germany; and the first treatise, on the Structure of Crystals, I know, is also from Italy, by Nicolas Steno; *Prodromus Dissertationis de Solido intra Solidum naturaliter contento*. Florentie, 1669, in 4to. A work of great merit. Louis Bourguet of Neuchâtel, in his *Lettres sur la Formation des Sels et des Crystaux*. Amst. 1729, 12mo. connected, by observation and measure, triangular and rhomboidal, and cubic, and pyramidal tetraedral molecules, for all different substances. His contemporary, Maurice Antoine Capeller\*, attempted to deduce a system from geometrical principles; and in this state did Linnæus find the subject, when he attempted to reduce the science of minerals to external characters, and crystallized bodies to salts.

None of the observations of Linnæus will prove useless to science; but his system alarmed the chemists and mineralogists, who rejected every other criterion, than internal character from analysis, and the system of Cronstedt was preferred by general assent. By this means, a spirit of controversy deprived the chemist and lithologist of mutual assistance; and the general opinion was correct, on the supposition, that a mixed system of chemical and external characters would be irreconcilable; but it has been admitted, even by those who most decidedly opposed Linnæus's system, that the best system of mineralogy should be founded on external and internal characters combined†. Among the few, who ventured to express their obligations, at the same time, to Linnæus, and to Cronstedt, was Baron Born, whose abilities and character, in addition to his distinction, as one of the counsellors of mines of his Imperial Majesty, obtained his enrolment among the fellows of the Royal Society. He connected the intrinsic and extrinsic characters of minerals, in the Index Fossilium, which he published in 1772. In Sweden, Bergman's Treatise on the Forms of Crystals, published in the Upsal Transactions, in 1773, was a more authoritative recommendation to the investigation of the principles of crystallization; and it can be of little importance for me to add, that since I have possessed the collection of Baron Born, in 1773, I have had every confirmation of the same opinion. The progress of chemistry, and of crystallography, applied to mineralogy, has rendered the examination of strata, and of mines, a source of amusement as well as instruction; and the arrangement of interesting facts, in the chemistry and mechanism of nature, suits my occasional researches in geology, which, from variety of avocations and circumstances, have been very much interrupted. My acknowledgment of obligation to the learned, who have made this progress in science, is the best recommendation I can give to others to examine their works. Those whose talents and time are devoted to the investigation of every mineral substance can have no respite to their labour; minerals, in every state of their

\* Prodomus Crystallographiæ, &c. and Litteræ ad Scheuzerum, de Crystallosum Generatione. Act. Nat. Cur. vol. 14. Append. p. 9.

† Nullum itaque est dubium, quin hujusmodi methodus mixta, quæ notis characteristicis tam extrinsecis quam intrinsecis simul combinatis, est superstructa, proxime ad naturalem accedens, maximam indicans symmetriam, reliques sit præferenda methodis. J. G. Wallerius, de Systemate Mineralogico rite condendo. §. 102.

formation,



formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained; and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance numbered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which irregular external characters can acquire, by fixing appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals; and the perfection he has since given to it, has rendered it very general. The Leskean collection, arranged after Mr. Werner's method, has procured in Mr. Kirwan a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the continent. It is, therefore, at this time, the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography.

Romé de L'Isle, in the year 1772, published the first edition of his *Essay on Crystallography*, which he states to be a supplement to Linnæus; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice; but that the Count De Bournon, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges, in his second edition, to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, &c.

The Abbé Haüy, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the beginning, the differences of Bourguet and Capeller were not more pointed than those of Romé de L'Isle, and the Abbé Haüy; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé Haüy is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modification of crystals. So far as they are the result of calculation and measurement, we may admit the laws of calculation; for whenever the superposition, or subtraction, of simple or compound molecules, on a nucleus, shall, by calculation, give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures.

These laws may be reduced to simple practice; for instance, the Abbé Haüy, by measuring the rhombic plane of corundum, found its two diagonals to be as two to three: which gives

to its acute angle  $81^{\circ} 47' 10''$ , and to its obtuse angle  $98^{\circ} 12' 50''$ ; the same as martial vitriol\*. The forms of fragments in corundum are all acute rhomboids. The cosine of the little angle in corundum is one-seventh of the radius; but in calcareous spar, the cosine is one-fifth of the radius; in schorl two-fifths of the radius; in the garnet, one-third; and in rock-crystals, one-seventeenth. Thus the application of general laws, to ascertain constant character, after they shall have been fully verified, may be very simple and general. It will not require perfect crystals; for when crystals separate into laminæ, which subdivide into fragments, and shew the form or arrangement of their molecules, it is easy, from such fragments, to connect them with their primitive crystal, and, consequently, with their class. It will be a great step, to obtain one regular and permanent external character.—Attention to other characters will be necessary, to ascertain the nature of the substance; and other external characters, such as irregular fracture, colour, &c. must be resorted to, when no permanent characters exist; but from their nature they are fallible, and, in fact, are seldom conclusive.

The progress of crystallography appearing, to me, of consequence to the progress of mineralogy, induced me to desire the Count de Bournon, above mentioned, one of the honourable victims to his allegiance to his king, to describe such crystals, in my collection, as shewed the different known modifications of corundum; which will develop the theory of crystallization, so far as is consistent with the avowed object of this paper. The subject, I believe, has not hitherto been submitted to the consideration of this society. The translation of the Count de Bournon's description has been carefully made, to preserve its clearness; and, I hope, it will be favourably received by the Society, and make some amends for my tedious introduction. After it, I have added a table, connecting in one view the specific gravities of corundum, &c. herein mentioned, with those given by other authors.

*An Analytical Description of the Crystalline Forms of Corundum, from the East-Indies, and from China. By the Count DE BOURNON.*

THE most usual form of corundum is a regular hexædral prism (Plate XXIV. Fig. 1.); in general, the surface of the crystal is rough, with little lustre, owing to unfavourable circumstances, under which it is crystallized.

The crystals of corundum, hitherto found, were not formed in cavities, where each crystal being insulated, its surface could preserve that smoothness, and natural brilliancy, which are common to all substances that freely assume a crystalline form. Like the crystals of feldspar which we meet with in the porphyroid granites, the corundum crystals have been enveloped, at the time of their crystallization, by the substance of the rock, which was forming at the same time with themselves, in an imperfect and confused crystalline mass; and the corundum crystal, before it had acquired its perfect solidity, necessarily received on its surface the im-

\* This result is extracted from the Journal de Physique; but it appears, from the Journal des Mines, No. 28, that the Abbé Haüy has since rectified this measure, and given  $86^{\circ} 26'$  for the acute angle, and  $93^{\circ} 34'$  for the obtuse angle.—G.

pression of the different particles of the rock which enveloped them: this naturally renders the surface rough and dull. Crystals of feld-spar, formed in the granitic porphyroid rocks, exhibit the same kind of appearance, from the same cause.

The corundum crystals are, in general, opaque, or at least they have only an imperfect transparency at the edges: when broken into thin fragments, the pieces are semi-transparent; when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull, by an infinite number of small flaws, crossing each other, much resembling the medullary part of wood, when it is viewed in the same manner. The degree of transparency of the small interstices, which are between these flaws, is further evidence that this texture of small flaws occasions opacity, which augments in proportion to the thickness of the fragments.

This kind of internal structure has also a very strong analogy with that of feld-spar in granite and porphyry. The endeavour to split these crystals in a direction, either perpendicular, or parallel to their axes, meets with a very considerable resistance: they may, indeed, be broken in these directions; but the rugged and irregular surface of the broken parts, clearly proves that the direction in which the crystalline laminæ have been deposited one upon another has not been followed.

The regular hexaedral prism of these crystals cannot therefore be considered as the form of the nucleus of the crystal; and, consequently, is not the primitive form of the crystals of this substance.

If, in order to discover the direction of the crystalline laminæ, a variety of crystals be examined, some will hardly fail to be met with, which, on their solid angles, formed by the junction of the sides of the prism, with the planes of the extremities, present small isosceles triangles. These are sometimes greater, and sometimes smaller, and form solid angles, of  $122^{\circ} 34'$ , with the extreme planes of the crystal. They are, in some instances, real faces of the crystal, but most frequently they evidently are the effect of some violence on that part. The smoothness and brilliancy of these small faces, in the latter case, shew that a piece has been detached in the natural direction of crystalline laminæ. It is, indeed, much less difficult to separate a portion of the crystal at those angles, than at any other part; and in following the natural direction of the faces, with a little patience and dexterity, all the crystalline laminæ may be detached, and progressively increase the size of the triangular face.

This operation, however, cannot be done indiscriminately on all the solid angles of the crystals, but only on the alternate ones at the same extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impossible to detach them. When, instead of the solid angles of an hexaedral prism, small triangular planes are met with (which frequently happens, whether caused by violence or otherwise), they are always placed in the direction above mentioned. If by following this indication of nature, we continue to detach the crystalline laminæ, we shall at last cause the form of the hexaedral prism to disappear totally, and in place of it, a rhomboidal parallelopiped will be obtained (fig. 2.), of which the plane angles at the rhombs will be  $86^{\circ}$  and  $94^{\circ}$ ; the solid

angles at the summit \* will measure  $84^{\circ} 31'$ ; and that taken at the reunion of the basis will be  $95^{\circ} 29'$ .

We can split this parallelopiped only in a direction parallel to its faces; it will still consequently preserve the same form, which is that of the nucleus of this substance, and its primitive form. It is, therefore, by a modification of the rhomboidal parallelopiped (fig. 2.) that nature has formed the regular hexaedral prism (fig. 1.) which this substance presents.

For if we conceive that in any period whatever of the increase of the rhomboidal parallelopiped, a series of laminæ, or crystalline plates, has been deposited on all the sides of the parallelopiped; and that these laminæ have all undergone a progressive decrease of one row of crystalline molecules, at the acute angle which tends to form the summit; and also along the sides of the opposite acute angle (fig. 3. and 4.); there will necessarily result from the continuation of this superposition, to a certain period, an hexaedral prism, terminated by two triedral pyramids, placed in a contrary direction; and their planes, or faces, which form a solid angle, of  $147^{\circ} 26'$ , with the sides of the prism, will be either pentagonal (fig. 3.) or triangular (fig. 4.). They will also have, in place of a summit, an equilateral triangular plane, sometimes greater and sometimes smaller.

If the superposition continues, the equilateral triangular plane, on the summit, will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped than small isosceles triangular planes (fig. 5.): if the superposition still continues, until the last crystalline lamina is reduced to a single molecule, or point, no appearance of the rhomboidal parallelopiped will then remain; and the crystal resulting from this operation of nature will be a regular hexaedral prism (fig. 1.).

In the same manner, viz. by a decrease on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous spar passes to a regular hexaedral prism of that substance, though more frequently it does so by a decrease on the lower angles of the laminæ. When the laminæ of the corundum crystal have, during their superposition on the planes of the primitive rhomboidal parallelopiped, experienced a progressive decrease at one of their acute angles, and along the sides of the other, at the same time, and in the same proportion, it is easy to conceive that the height of the hexaedral prism must be the same as that of the rhomboidal parallelopiped, upon which it has been formed. The height B C (fig. 1.) must therefore bear the same proportion to the line A B, drawn through the middle of the two opposite sides of the planes, on the extremities, as the whole height E F of the rhomboidal parallelopiped (fig. 2.) bears to the small diagonal G H, from one of the rhombs, that is, nearly as 6, 45 : 5.

But although this exact proportion appears in a very great number of corundum crystals, yet we meet with some, whose lengths are more or less considerable; and this is owing

\* For greater clearness, this rhomboidal parallelopiped may be considered as being formed by the junction of two triedral pyramids, base to base; and the two solid angles (each of which is formed by the reunion of three of the acute angles on the planes of the rhomb) will then be considered as the summits of these pyramids.

to different circumstances, which have existed at the time of their crystallization. We may conceive, for instance, that if before the progressive decrease of the crystalline laminæ, in the manner above mentioned, the increase of the rhomboidal parallelepiped had taken place, by a superposition of laminæ, in which the rows of crystalline molecules experienced a progressive decrease along the edges of the acute angle of the base only (fig. 6.), and that (the sides of the prism having already acquired a certain length) the succeeding crystalline laminæ had experienced a decrease at the acute angle of the summit, the same regular hexædral prism would have resulted from this process; but the proportion between the height, and the line drawn from two of the opposite sides of the planes, on the extremities, would have been much greater than that of  $6.45 : 5$ , and consequently this prism would have been longer than that of the rhomboidal parallelepiped, which served as its nucleus. On the other hand, if the increase of the rhomboidal parallelepiped had taken place, by a superposition of crystalline laminæ, decreasing at the acute angle of the summit, and some time after decreasing also along the sides of the acute angle of the base (fig. 7.), the regular hexædral prism resulting from this process, would have been shorter, in proportion to the duration of the mode of decrease in the crystalline laminæ, which were first deposited. There are some of the hexædral prisms, in corundum crystals, which are so short, that they appear no more than segments. Calcareous spar offers the same phenomenon; as do likewise all the substances in which the hexædral prism has any analogy of formation, with that which we have here described.

It happens frequently, when the superposition of the crystalline laminæ does not go on equally on all the faces of the rhomboidal parallelepiped, that one or two only of the solid angles of the hexædral prism, taken alternately, still shew, by small isosceles triangular planes, some remains of the faces of the parallelepiped, while the others do not shew any at all.

Mr. Greville, in his collection of this substance, has a crystal of corundum, upon one side of which only two of the planes of the rhomb have experienced an equal and perfect superposition; while there has been but a very small number of crystalline laminæ deposited on the third plane. Consequently this crystal presents a regular hexædral prism, one of whose solid angles is so much truncated, that the half of the plane of the end of the hexædral prisms disappears (fig. 8.); and this cut, or section, forms an angle of  $122^{\circ} 34'$  with the plane on the extremity.

It is unnecessary to observe, that the regularity of the hexædral prism depends on that of the rhomboidal parallelepiped, on which it is formed.

When, by detaching the laminæ from the alternate solid angles of the regular hexædral prism, the planes resulting from this operation begin to run into one another; and the crystal begins to assume the form of the rhomboidal parallelepiped, to which it owes its origin; we frequently see the surface of these new planes divided into an immense number of small rhombs, formed thereon by the intersection of lines, that are parallel to the sides, which belong to the rhomboidal form of the new faces (fig. 9.).

These lines are owing to the extremities of the laminæ, which have been deposited on the inferior faces, corresponding with those on which we observe them; and they serve to cor-

robamate still further, the demonstration we have given of the formation of the regular hexadral prism in this substance.

(To be concluded in our next.)

#### IV.

##### *On Water-wheels.*

To Mr. NICHOLSON.

SIR,

**Y**OUR attention to the inquiries sent last month, induces me to transmit the annexed paper for your consideration.

Your opinion that a float-board-wheel is preferable to a bucket, agrees with that of an experienced mill-wright.

I hope, through the medium of your valuable Journal, the subject will receive further discussion; as the ground on which the preference is given is not obvious.

It will, no doubt, be agreeable to you to learn, that the sphere of your Journal's useful communication is extended to the northern verge of Ireland; and that, amidst the tumult of political strife, the arts of peace, and the interests of science, are not yet forgotten.

I am, your obliged reader,

February 1798.

DERRIENSIS.

##### *On Water-wheels.*

**THE** effect of water-wheels, arising from the gravitating power of the water; to retain the water, seems the point necessary to be attended to, in the construction of the wheel.

The form of the bucket-wheel seems best adapted to this end; yet in streams, where the fall is not considerable, a preference is given to float-board-wheels.

Why is this preference given to float-board-wheels? Suppose a stream which has a fall of six feet, and a supply which renders it equal to the power of six horses:

Allow this power to be adequate to raise a given weight, one foot high, thirty times in a minute.

It is convenient to have a water-wheel of ten feet diameter. A bucket-wheel is made of a width adequate to the supply of water. Will this wheel be less competent to the work than a large float-board-wheel? If so, to what is the loss of power in the small, or the gain of power in the large, wheel owing?

**WITHOUT** entering minutely at present into the subject here offered for consideration, which I leave to my other correspondents, I shall only remark, that the preference seems to have been given to the close breast-wheel, for streams affording considerable quantities of water

water with a low fall, for the following reasons: 1. The load upon the bucket-wheel produces more friction, on the extremities of the axis, than is produced in the breast-wheel by the water, during its passage down the channel. That this reasoning is good, may be shewn by a very familiar experiment. Let any wheel be made to rest on its pivots, and then turned round; it will gradually lose its velocity, by the friction, to a certain point, when it will stop all at once. The suddenness of this stop will indicate the magnitude of the friction. If this be small, the velocity will much more gradually decay than in the contrary case. On the other hand, to shew the friction at the surface of a fluid, let a bowl be filled with water, and the fluid be stirred, so as to give it an horizontal motion; it will be found, that this large and heavy mass, bearing on a surface so very much exceeding that of the pivots of the wheel, will turn round for a considerable time, and lose its motion by very imperceptible gradations: whence it will follow, that the friction is much less. 2. In the usual construction of overshot-wheels, part of the fall is lost, in delivering the water, at a certain distance below the vertex, and part of its action is lost, by its running out of the buckets, before it has arrived at the lowest point of the revolution. 3. It is a general notion, that large wheels go steadier, from their operating in the manner of a fly, by means of their inertia; and wheels have been actually constructed of iron, with a view to this effect. The breast-wheel is always larger than the overshot-wheel, under like circumstances; and it may be constructed, under certain limitations, of as large a size as the engineer may think fit. 4. It is true, that a breast-wheel, of any size, might be converted into a bucket-wheel, and carry down the water in its buckets, instead of its being suffered to slide down the usual channel. But, when we imagine this conversion to take place, we see, at once, that the action would be less steady, from the successive filling of the buckets, and that the lower portion of the loaded periphery would discharge some of its contents much earlier than the point at which the breast-water stream would have ceased to act.

The numerical computation of the difference of effect between one wheel and the other, according to the dimensions given in the present communication, may be made from the data at page 466 of our present volume. It is there deduced, that one horse will raise two hogsheds and a half of water, ten feet high, in a minute; and this effect, from the note at page 465, would require the fall of twice that quantity of water, as a power to overcome friction, and produce velocity of working. The stream mentioned by my correspondent must, therefore, afford 415 cubic feet of water per minute, with a fall of 6 feet, if it be equal in power to six horses.

If certain dimensions be assumed for the buckets of the overshot-wheel, and an ordinary breast-wheel be compared with it, the quantity and fall of the water, uselessly expended on the former, will shew the difference of effect.

W. N.



## V.

*On the Glafs Trundles of Citizen RENAUT; and the Duration of the Teeth of Mill-work.*  
By C. B.

To Mr. NICHOLSON.

SIR,

I OBSERVE in your Journal for January last, an account of Citizen Molard's having substituted glafs in the place of iron for the trundles of lantern pinions, which he states to be much preferable to the latter; as he says, cast-iron wears out the wooden teeth they act against in about four months; whereas, the glafs, in the experiment he made, wore the wooden teeth about one-twelfth of an inch only in eighteen months.

The use of glafs, in the trundles of lantern pinions, is liable to many objections; experience, however, may point out methods of obviating many of these: but I think it right to mention, that Citizen Molard is extremely wrong, when he says, cast-iron wears out the wooden teeth in four months; if it does so in France, it is otherwise in this country: I have had a mill of my own at work for these four years, in which wooden teeth act against iron, and I have never been obliged to get so much as a new tooth. Some months since I saw a steam engine, at Mess. Fish and Yates', in St. John-street, which turns machinery for his grinding of snuff and cutting tobacco. This machine was erected by Mr. Rennie, engineer of this city, in the year 1786, and has been constantly at work for about fifteen hours per day ever since; yet the wooden teeth, in the first motion, which act on cast-iron, had not worn above one-sixth of an inch; and the iron teeth did not seem to be worn  $\frac{1}{8}$  part of an inch: and from every appearance, they will work without repair for ten or twelve years to come; as the workmen told me, that they had not, apparently, worn any thing for these six years past. They appeared to be very well executed; but not better than Mr. Rennie generally does such work.

By publishing the above, you may, perhaps, prevent an ill-founded prejudice from being taken against the use of cast-iron, in mill-work; which, from experience, I can say, is the best material I have known to be used.

I am, Sir,

Your most humble servant,

Feb. 4th, 1799.

C. B.

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I have visited the manufactory of Mess. Fish and Yates', and find that the statement given above is perfectly correct. It may be presumed, that the iron trundles, mentioned by Citizen Renaut, at page 522, were either ill finished, or rough from the forge; and, at all events, it appears, that they ought not to afford ground for any prejudice against good work in the usual materials.

W. N.

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VI.

*On Dr. Parr's Theory of Light and Heat. By a Correspondent.*

TO MR. NICHOLSON.

SIR,

**A**CCIDENT prevented my seeing, for some time, your Journal, published in December last; and I now take the liberty of making a few observations on some remarks of your correspondent, respecting Dr. Parr's theory of light and heat\*. He observes, "with respect to Dr. Parr's theory, that light and fire repel each other, when disengaged from matter, it appears to me too fanciful to be solid, and too inconsistent to be supported," &c.—I do not quote the whole passage, because I mean not to commence a controversy upon the subject, as from the observation just transcribed, as well as from what follows, it is evident that your correspondent has seen only the quotation in Dr. Pearson's paper; and as the essay was published before the appearance of your Journal, I request only room for a short analysis of the theory, that it may not be, as I suspect it has been in the passage before me, misunderstood. I shall take up a little space only in your valuable publication, but you will allow me to add, that M. Gadolin has since published an opinion not very different; and that, when Count Rumford speaks of changes effected, not "by any chemical combination of the matter of light with such bodies (as are exposed to it), but merely by the heat which is *generated* or *excited* by the *light* which is *absorbed* by them," he in reality uses the same language.

Dr. Parr does not merely assert an opinion, but supports his hypothesis by numerous and well-connected facts. Light he considers, with the chemical philosopher of the present day, as a substance capable of combining with the various bodies it meets, producing by that means many chemical changes. Among these, he traces with peculiar care its influence upon vegetation; and from the decomposition of water, afforded in the leaves of vegetables, seemingly by its power, he explains the great variety of gases expired by plants in different situations.

As light thus separates oxygen, and as the latter probably contains heat in a less compounded state than any other substance, he steps over the next difficulty, and supposes that it separates heat. This is the only step unsupported by immediate facts; but if assumed for a moment as an hypothesis, the concurrence of numerous other facts will raise it to a higher rank. By what means light and heat are brought together in union, when they are ingredients in inflammable bodies, he pretends not to explain; but if they are really different elements, as is now generally believed, our not knowing the bond of union, is no proof against its existence. In the examination of the various explosive substances, Dr. Parr traces in each the source of its light and heat, and shows by what means the balance subsisting between these active ingredients is destroyed; sometimes by the addition of the one, sometimes by that of the other principle. There are various explosive substances which will not ex-

\* Essay upon Light, in a miscellaneous volume, published by a society at Exeter.

plode without light, however intense the heat, and *vice versa*.—This part of the essay, where he examines the source of light in the composition of the body, and explains the different modes by which the violent separation is effected, as well as the consequences of the explosion, appears highly ingenious.

Such, Sir, is the outline of the paper which I think is misunderstood and misrepresented by your correspondent. The different parts can be only fairly appreciated by an examination of the facts. This is a business to which I cannot now attend, and which is not suited to your Journal. Your giving me a little room for this analysis will oblige, Sir,

Yours,

A CONSTANT READER.

## SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

### *Institute of Cairo.*

(Continued from page 523, vol. II.)

THE second sitting was held on the 11th Fructidor, at seven in the morning, at which Citizen Andreossi made a report on the fifth question proposed by Buonaparte. He observed, in the first place, that Egypt possesses no sulphur, but formerly imported it from Venice. The charcoal is obtained by burning the lupine in a trench, and afterwards sifting it. The saltpetre is native, and is even said to be found in veins (*par veines*) round Cairo. It is refined in the same manner as (commonly) in Europe. It is a true nitrate of potash, and not of lime, as in France. The boiling is made with the stalks of Turkey-wheat, and it is purified with whites of eggs. The gunpowder is made by hand, and the workmen are naked. Each mortar contains fifteen pounds, and the process lasts seven hours. The pestles weigh from nineteen to twenty-five paros. A small quantity of water is added, and the granulation is performed by a sieve. The reporter affirms that *this powder is very good*; but that to render it still better it is necessary that the proportions of France should be followed. Two thousand cantars (were) formerly made at Cairo, of which much was sent to Leghorn. The beys had very little powder. Murat Bey had no more than 1,500 cantars. It would be easy, says the reporter, to augment this fabrication, and even to export large quantities to Europe.

At the same sitting, the citizen Monge read a memoir on the phenomenon, called *le mirage* by French seamen, and by ours *looming*. This effect of refraction was remarkably seen by the French, in the course of their march through the desert. Villages seen at a distance appeared elevated, and as if built on an island in the middle of a lake. The surface of this apparent water became narrower as they approached, until at length, when they were only at a small distance, it disappeared; but the same illusion presented itself, with regard to the

next

next remote village. The author ascribes this phenomenon to a diminution of the density of the lower stratum of the atmosphere, occasioned by evaporation of water from the sands. It is impossible to deduce the particulars of his theory from the mere report; but it seems to be nearly the same as that of Mr. Huddart\*.

*The third sitting was held on the 16th Fruetidior.* Berthollet read a memoir on the formation of ammoniac. Citizen Sulkowsky read a description of the route from Cairo to Salehia. Egypt has hitherto been known only on the banks of the Nile. The road followed by the French army in pursuit of Ibrahim Bey was entirely unknown. On going out of Cairo by the gate of Nasr, you enter the desert, where several forsaken houses were observed. The village of Elmaria, on this road, is the ancient Heliopolis. In another village, called Elmaria, there were thousands of palm-trees. On the right of this road, there are nothing but deserts of sand; but, on the left, are many cultivated lands. The places which the army passed are Lacoubey, Elhanea, Elmenia, Belbeys, Souva, Coraim, Salehia, and many others, which it merely passed through in haste.

Citizen Say spoke of the methods of obtaining better means of grinding, and shewed that water-mills ought to be constructed.

C. Balholet (q. Berthollet?) read a memoir, in which he examines the gun-powder of Cairo. It contains only five parts out of thirty-two of salt-petre; the rest is sulphur, charcoal, earthy matter, and muriate of soda: so that *the only means of rendering it useful, is to wash out the salt, and manufacture it a second time.*

Citizen Monge read a memoir on the antient monuments of Cairo. The safar, in the street which leads from the institute to the castle, presents a vase of granite with hieroglyphics within and without. It was a tomb, and resembles that of the mosque of Alexandria. Citizen Monge proposes that it be removed by the Institute, in order to convey it to the museum of France. Near the castle, after having passed the palace of Joseph, there is the cell of a gate with hieroglyphics well preserved.

*The fourth sitting, 21st Fruetidior.* Citizen Sulkowsky read a memoir on a bust of Isis. Say, another on the materials for fuel in Egypt. The commission for grinding, announced their intention of erecting a wind-mill. Citizen Geoffroy read a memoir on the ostrich.

*The fifth sitting, 26th Fruetidior.* The commission for fuel, reported that the stems of carthamus, reeds, and straw, may be used for heating ovens, at less charge than in France.—Buonaparte presented the *Connoissance des Temps* for the year VII, and invited the Institute to compose an almanack. A commission was appointed for this object, consisting of Monge, Beauchamp, Nouette, and Raphael. It was at the same time decreed, that the three following almanacks should be united, namely, that of the Copts, the Mussulmen, and the French. Citizen Fourier read a memoir on the solution of algebraic equations. Citizen Parceval, a translation of a fragment of Tasso. Citizen Desgenettes read a dissertation on the diarrhoea, the dysentery, and the endemic ophthalmia, which are more peculiarly the disorders of Egypt.

\* Philosophical Journal, I. 145.—This curious subject, which does not yet seem to be adequately explained in all its particulars, has employed the meditations of various observers; for some of which, the reader may refer to the authors quoted at page 418 of our present volume.—N.

*The sixth sitting, on the first complementary day.* Citizen Beauchamp presented an annuary, ready for immediate printing, and also several astronomical observations. Citizen Berthollet read a letter from Citizen Laplace, announcing a correction in the metre. He also gave an account of the manufacture of indigo, and pointed out considerable improvements. Citizen Fourier communicated a project of a wind-mill, to water lands.

*Sixth sitting, on the 6th Vendemaire, year VII.* Citizen Pouffielque presented a sketch of a new method of analytically demonstrating geometrical theorems, by Corancez the younger. Citizen Norry read a memoir on Pompey's pillar. Savigny described a new species of nymphæa. A commission was charged with the establishment of a school of design, among whom were Redouté and Norry. Citizen Costaz read a memoir on the colours of the sea, and Citizen Parceval another translation from Tasso.

*Eighth sitting, Vendemaire, year VII.* Fifty mummies of birds, sent to the Institute, were delivered for examination to a commission, composed of Buonaparte, Geoffroy, Dolomieu, and some others. Porte, a native of France, and inhabitant of Cairo, who is employed on Indigo, presented samples to the Institute. Citizen Larrey read a memoir on the ophthalmia. Citizen Beauchamp read another on his voyage to Trebifond. He indicates the longitude of Isphahan, and observes, that the longitude of Trebifond is  $37^{\circ} 18' 5''$  from Paris, and not  $43^{\circ}$ , as Bonne affirms; which deducts more than eighty leagues from the Black Sea. Citizen Delisle read a memoir on the palm-tree, which bears the fruit called domm. It is the cassiophora of Theophrastus. Citizen Dolomieu read a memoir on the study of ancient and modern geography. He fixes the position of the ancient Alexandria between two hills of calcareous sand-stone, and explained the subsequent changes. He thinks the sea must have risen a foot since the time of the Ptolemies. Citizen Norry made a report concerning the school of design; and Citizen Parceval read a translation from Tasso.

Since the commencement of the VIIth year, a literary journal has appeared, under the title of *Decade Egyptienne, Journal litteraire et d'Economie politique*. The prospectus was signed Tallien, and is composed of thirty-eight pages. The Journal is to appear every decade, each number containing two and a half or three sheets, in octavo. The price per number is to be 1 franc French money, or 10 francs per 12 numbers. Subscriptions are taken by Citizen Marc-Aurell, printer to the army, quartier des François, at Cairo. The first number appeared the 10 Vendermaire, year VII.

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An Account of the Operations carried on for accomplishing a Trigonometrical Survey of England and Wales, from the Commencement in 1784, to the End of the Year 1796. Begun under the Direction of the Royal Society, and continued by Order of the Honourable Board of Ordnance. First published in, and now received from, the Philosophical Transactions. By Captain William Mudge, F.R.S. and Mr. Isaac Dalby, Vol. I. Illustrated with 22 Copper-plates. London, printed by Bulmer, and Co. for Faden, Charing-Cross, 1799; 437 pages, Price 1l. 8s. boards.

ALL Europe is acquainted with more or less of the particulars of this survey, which is a work of the utmost utility, and an honour to our government, and the parties to whom the execution

execution has been intrusted. The accounts having hitherto been published only in the Philosophical Transactions, must necessarily have been of confined circulation, while the number and expence of the engravings have prevented any satisfactory detail from appearing in other publications. The geographer and man of science will, consequently, learn with great pleasure, that the account of the operations, as far as they have yet been carried, together with the plates of instruments, surveys, and other results, are to be had, in a beautifully printed quarto volume, at a very easy price.

The original design had, for its immediate object, the ascertaining of data, by which the difference in longitude, between the observations of Greenwich and Paris, might be determined. Soon after the death of General Roy, the general survey of the kingdom was commenced, of which accounts were published, in the Transactions for 1796. And in 1798, Mr. Faden having determined on republishing all the papers relative to this object, the president and council of the Royal Society rendered him the very essential service of furnishing him with the original copper-plates, and the master-general of the ordnance granted him permission to reprint the accounts of the subsequent trigonometrical survey. This is now done under the superintendence of the able men whose names appear in the title page, who have omitted no care to render this collection of equal value with the originals, and in some few respects more so. An account of the changes, which the editors have made in the original papers, is given in the preface; where we also observe, that Mr. Faden, with the permission of the Board of Ordnance, intends to publish a very superior map of Kent, in the course of the present year, from documents supplied by the labours of the gentlemen employed on the general survey by government.

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A public institution for diffusing the knowledge and facilitating the general introduction of useful mechanical improvements, and for teaching by courses of lectures and experiments the application of science to the common purposes of life, has been proposed to be established, by subscription, in London, by some gentlemen of the first respectability. I have seen a printed paper, containing the outlines of a plan, and a list of gentlemen, who have subscribed fifty guineas each, for this purpose. As the plan itself is still under deliberation, with regard to various essential particulars, and the present list of subscribers does not contain the whole of the names, on account of the rapidity with which that list increases, I shall enter into no further detail at present, than simply to observe, that the plan appears to have already met with a degree of support, which is no less honourable to the public spirit of the subscribers, than distinctive of its own value and importance.

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*M. Cuthbertson's new universal Electrometer.*

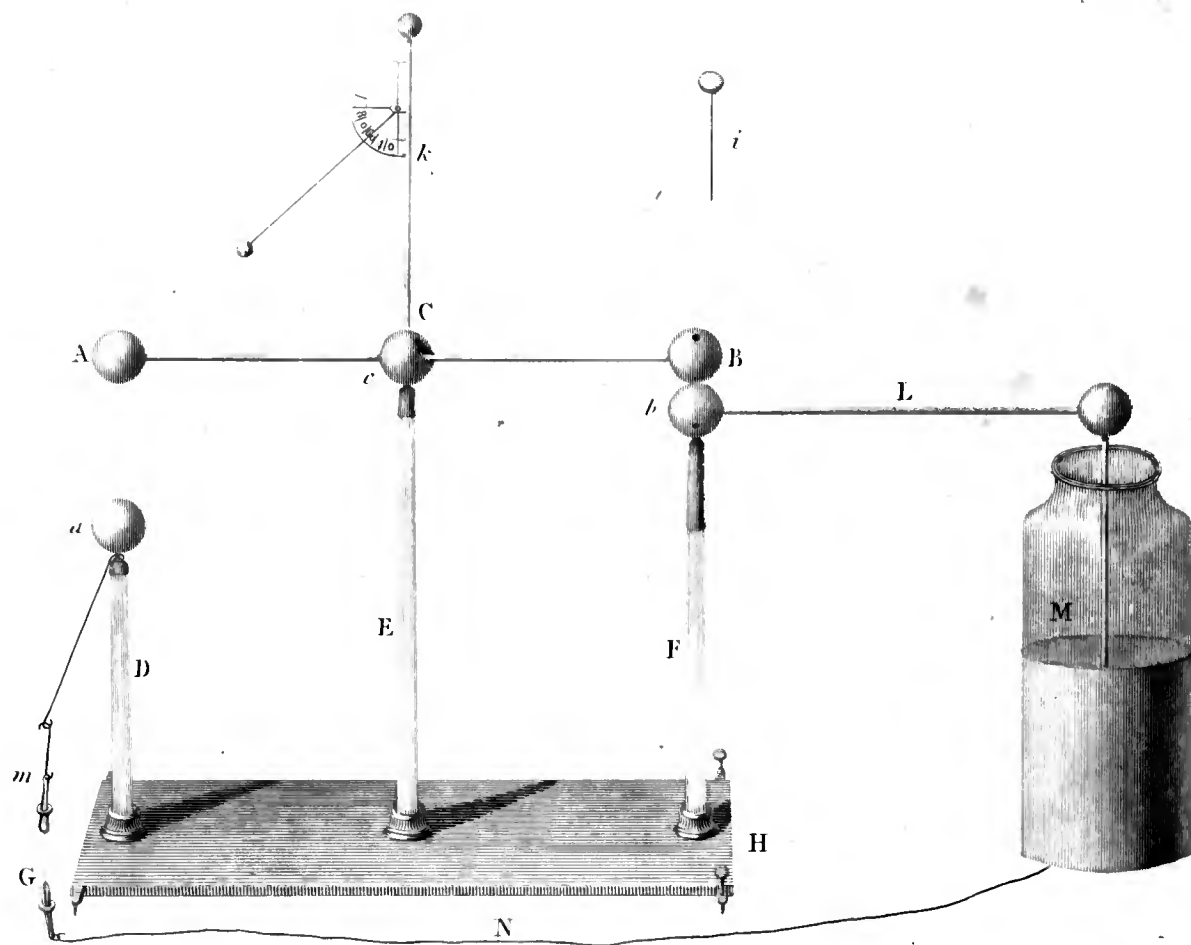


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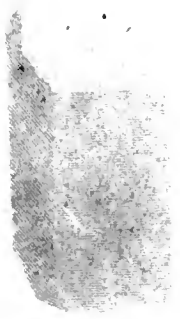


Fig. 1



Fig. 2



Fig. 3



Fig. 4

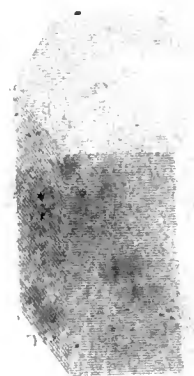


Fig. 5



Fig. 6



Fig. 10



Fig. 11

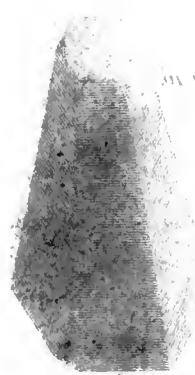
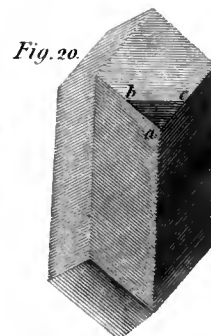
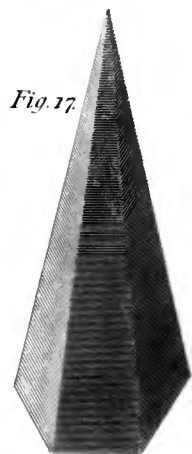
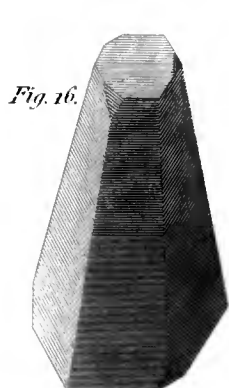
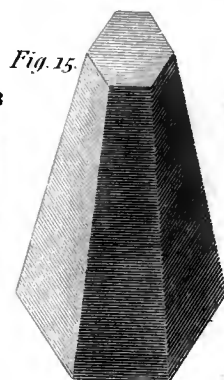
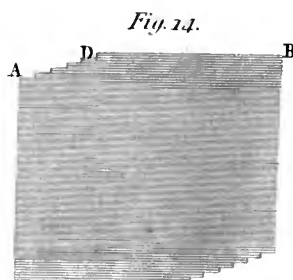
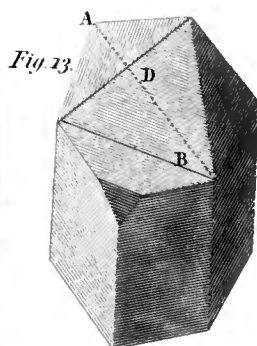
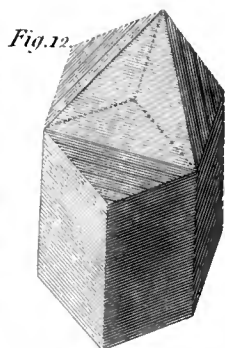
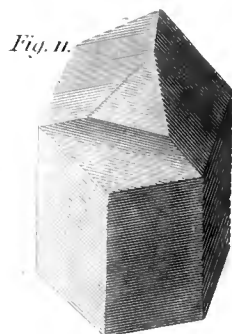
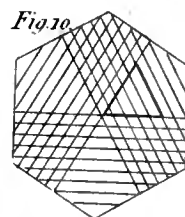
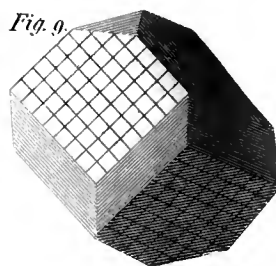
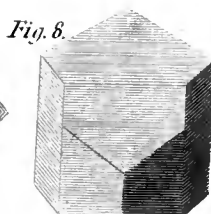
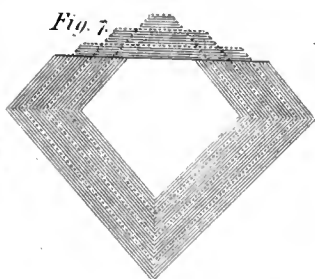
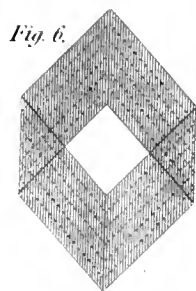
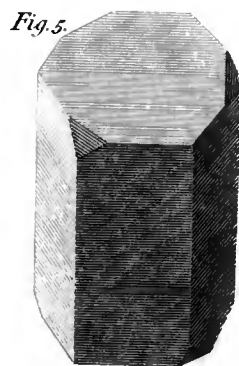
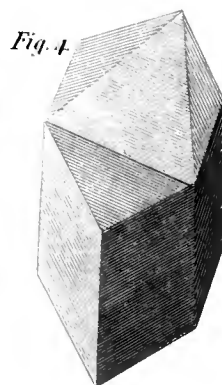
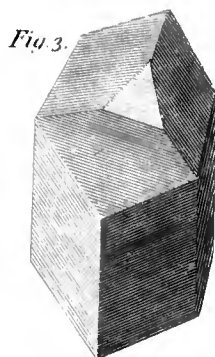
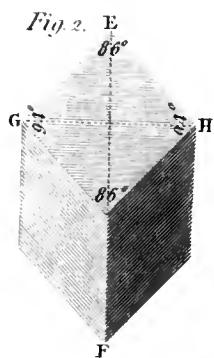
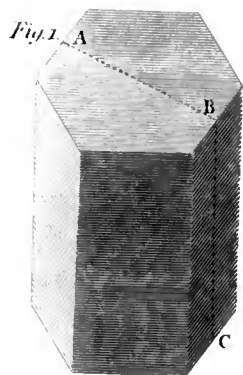


Fig. 19









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## ERRATA IN VOL. II.

## Page. Line.

- 29 3 from the bottom, for *as*, read *aa*  
46 13 After page 424, insert *Vol. I.*  
The plate of *Apparatus* should be numbered II.  
123 18 for *periodical*, read *synodic*  
19 for i d. 8 b. 26  $\frac{1}{3}$  m. read i d. 18 b. 28 m. 36 s.  
124 37 for *not quite + 37"*, read *upwards of — 30"*  
— 40 for *97 $\frac{1}{2}$  day's use*, read *117 and 97 $\frac{1}{2}$  day's use respectively*  
— *ult.* for *x* read of.  
125 11 What is said of *the first and second satellites*, respecting the annual

## Page. Line.

- error, is exactly true, only with respect to *the second*:—the error of *the first* being *minus*, and in a smaller proportion.  
264 — In some copies, the figures in the note have slipped, and give an imperfect impression. Read *the amount of Gold coined between the years 1762 and 1772 both inclusive, was 8,157,203l. 15s. 6d. and between 1782 and 1792, both inclusive, was 19,675,666l. 14s. 6d. and between 1773 and 1777, both inclusive, was 19,591,833l.*

## Page. Line.

15. And in the fourth line from the bottom, read *reckoned at  $\frac{1}{2}$  (half) per cent.*  
275 8 read *the pipe A B F E*  
439 8 from the bottom for *state* read *plate*  
488 14 from the bottom, erase the full stop after the word *polished*, and add a full stop after *length*  
490 7 for *offered*, read *afforded*  
— 3 from the bottom for K B read K D  
491 12 read *interior* part of the tube  
— 3 from the bottom for D D F C read D B F C  
558 6 for *Biringuccius* read *Beringuccius*.





















